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Deceased.

HENRY R. WORTHINGTON..................................Dec. 17, 1880.
THEODORE R. SCOWDEN..................................Dec. 31, 1881.
ALEXANDER L. HOLLEY...................................Jan. 29, 1882.
ERASTUS W. SMITH......................................June 12, 1882.
PETER COOPER, Honorary Member.........................April 4, 1883.
JAMES PARK, JR..April 21, 1883.
W. K. SEAMAN..July 2, 1883.
REDMOND J. BROUGH.....................................July 21, 1883.
C. W. SIEMENS, Honorary Member........................Nov. 20, 1883.
HENRY F. SNYDER.......................................Nov. 25, 1883.
O. HALLAUER, Honorary Member..........................Dec. 5, 1883.
WILLIAM ATWOOD..Feb. 16, 1884.
WILMER G. CARTWRIGHT..................................Feb. 23, 1884.
THEODORE H. RISDON....................................May 19, 1884.
ISAAC NEWTON..Sept. 25, 1884.
J. H. BURNETT...Jan. 31, 1885.

Horace Lord .... Feb. 28, 1885.
D. H. Hotchkiss .... April 29, 1885.
Henri Tresca, Honorary Member .... June 24, 1885.
Henry H. Gorringe .... July 6, 1885.
Wilbur H. Jones .... July 29, 1885.
Frederic E. Butterfield .... Sept. 5, 1885.
Wm. Cleveland Hicks .... Oct. 19, 1885.
D. S. Hines .... Nov. 9, 1885.
Theodore Bergner .... January 5, 1886.
Emile F. Loiseau .... April 30, 1886.
John C. Hoadley .... Oct. 21, 1886.
Homer Hamilton .... Nov. 29, 1886.
John B. Root .... Dec. 11, 1886.
Bishop Arnold .... Feb. 16, 1887.
B. F. Emerson (*Associate*) .... April 5, 1887.
Wm. L. Nicoll .... July 2, 1887.
Jackson Bailey (*Associate*) .... July 7, 1887.
James Sheriffs .... July 18, 1887.
William Wallace Hanscom .... Jan. 19, 1888.
Barnabas H. Bartol .... Feb. 10, 1888.

RULES

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

[*Adopted November 5th*, 1884.]

OBJECTS.

ART. 1. The objects of the AMERICAN SOCIETY OF MECHANICAL ENGINEERS are to promote the Arts and Sciences connected with Engineering and Mechanical Construction, by means of meetings for social intercourse and the reading and discussion of professional papers, and to circulate, by means of publication among its members, the information thus obtained.

MEMBERSHIP.

ART. 2. The Society shall consist of Members, Honorary Members, Associates and Juniors.

ART. 3. Mechanical, Civil, Military, Mining, Metallurgical and Naval Engineers and Architects may be candidates for membership in this Society.

ART. 4. To be eligible as a *Member*, the candidate must have been so connected with some of the above-specified professions as to be considered, in the opinion of the Council, competent to take charge of work in his department, either as a designer or constructor, or else he must have been connected with the same as a teacher.

ART. 5. *Honorary Members*, not exceeding twenty-five in number, may be elected. They must be persons of acknowledged professional eminence who have virtually retired from practice.

ART. 6. To be eligible as an *Associate*, the candidate must have such a knowledge of or connection with applied science as qualifies him, in the opinion of the Council, to co-operate with engineers in the advancement of professional knowledge.

ART 7. To be eligible as a *Junior*, the candidate must have been in the practice of engineering for at least two years, or he must be a graduate of an engineering school.

The term "Junior" applies to the professional experience, and not to the age of the candidate. Juniors may become eligible to membership.

ART. 8. All Members and Associates shall be equally entitled to the privileges of membership. Honorary Members and Juniors shall not be entitled to vote nor to be members of the Council.

ELECTION OF MEMBERS.

ART. 9. Every candidate for admission to the Society, excepting candidates for honorary membership, must be proposed by at least three members, or members and associates, to whom he must be personally known, and he must be seconded by two others. The proposal must be accompanied by a statement in writing by the candidate of the grounds of his application for election, including an account of his professional experience, and an agreement that he will conform to the requirements of membership if elected.

ART. 10. All such applications and proposals must be received and acted upon by the Council at least thirty days before a regular meeting, when the Secretary shall at once mail to each member and associate, in the form of a letter ballot, the names of candidates recommended by the Council for election.

ART. 11. Any member or associate entitled to vote may erase the name of any candidate, and may, at his option, return to the Secretary such ballot enclosed in two envelopes, the inner one to be blank and the outer one endorsed by the voter.

ART. 12. The rejection of any candidate for admission as member, associate, or junior, by *seven* voters, shall defeat the election of said candidate. The rejection of any candidate for admission as honorary member by *three* voters shall defeat the election of said candidate.

ART. 13. The said blank envelopes shall be opened by the Council at any meeting thereof, and the names of the candidates elected shall be announced in the first ensuing meeting of the Society, and also in the first ensuing list of members. The names of candidates not elected shall neither be announced nor recorded in the proceedings

ART. 14.—Candidates for admission as honorary members shall

not be required to present their claims; those making the nominations shall state the grounds therefor, and shall certify that the nominee will accept if elected. The method of election in other respects shall be the same as in case of other candidates.

ART. 15. All persons elected to the Society, excepting honorary members, must subscribe to the rules and pay to the Treasurer the initiation fee before they can receive certificates of membership. If this is not done within six months of notification of election, the election shall be void.

ART. 16. The proposers of any rejected candidate may, within three months after such rejection, lay before the Council written evidence that an error was then made, and if a reconsideration is granted, another ballot shall be ordered, at which thirteen negative votes shall be required to defeat the candidate.

ART. 17. Persons desiring to change the class of their membership shall be proposed in the same form as described for a new applicant.

FEES AND DUES.

ART. 18. The initiation fees of members and associates shall be $15, and their annual dues shall be $10, payable in advance. The initiation fee of juniors shall be $10, and their annual dues $5, payable in advance. A junior, being promoted to full membership, shall pay an additional initiation fee of $5. Any member or associate may become, by the payment of $150 at any one time, a life member or associate, and shall not be liable thereafter to annual dues.

ART. 19. Any member, associate or junior, in arrears may, at the discretion of the Council, be deprived of the receipt of publications, or stricken from the list of members, when in arrears for one year. Such person may be restored to membership by the Council on payment of all arrears, or by re-election after an interval of three years.

OFFICERS.

ART. 20. The affairs of the Society shall be managed by a Council, consisting of a President, six Vice-Presidents, nine Managers, and a Treasurer, who shall be elected from among the members and associates of the Society at the annual meetings, to hold office as follows:

ART. 21. The President and the Treasurer for one year; and

no person shall be eligible for immediate re-election as President who shall have held that office for two consecutive years; the Vice-Presidents for two years, and the Managers for three years; and no Vice-President or Manager shall be eligible for immediate re-election to the same office at the expiration of the term for which he was elected.

ART. 22. A Secretary, who shall be a member of the Society, shall be appointed for one year by a majority of the members of the Council at its first meeting after the annual election, or as soon thereafter as the votes of a majority of the members of the Council can be secured for a candidate. The Secretary may be removed by a vote of twelve members of the Council, at any time after one month's notice has been given him by a majority of its members to show cause why he should not be removed, and he has been heard to that effect. The Secretary may take part in any of the deliberations of the Council, but shall not have a vote therein. His salary shall be fixed for the time he is appointed by a majority vote of the Council.

ART. 23. At each annual meeting, a President, three Vice-Presidents, three Managers and a Treasurer shall be elected, and the term of office of each shall continue until the end of the meeting at which their successors are elected.

ART. 24. The duties of all officers shall be such as usually pertain to their offices or may be delegated to them by the Council or by the Society. The Council may, in its discretion, require bonds to be given by the Treasurer.

ART. 25. The Council may, by vote of a majority of all its members, declare the place of any officer vacant, on his failure for one year, from inability or otherwise, to attend the Council meetings, or to perform the duties of his office. All such vacancies and those occurring by death or resignation shall be filled by the appointment of the Council, and any person so appointed shall hold office for the remainder of the term for which his predecessor was elected or appointed; *provided* that the said appointment shall not render him ineligible at the next annual meeting.

ART. 26. Five members of the Council shall constitute a quorum; but the Council may appoint an Executive Committee, or business may be transacted at a regularly called meeting of the Council, at which less than a quorum is present, subject to the approval of a majority of the Council, subsequently given in writing to the Secretary and recorded by him with the minutes. Absent mem-

bers of the Council may vote by proxy upon subjects stated in the call for a meeting, said proxy to be deposited with the Secretary.

ART. 27. The President on assuming office shall appoint a Finance Committee and a Publication Committee and a Library Committee of five members each. The appointment of two members of each Committee shall expire at the end of each year. The Secretary shall, *ex officio*, be a member of all three Committees.

ART. 28.—The Finance Committee shall have power to order all ordinary or current expenditures, and shall audit all bills therefor. No bill shall be paid except upon their audit. When special appropriations are ordered by the Society, they shall not take effect until they have been referred to the Council and Finance Committee in conference.

ART. 29. It shall be the duty of the Publication Committee to receive all papers contributed, to decide which shall be published in the *Transactions*, and which shall be read in full at the meetings.

ART. 30. It shall be the duty of the Library Committee to take charge of the collection of all material for the Library of the Society, and to supervise all regulations for its use.

ELECTION OF OFFICERS.

ART. 31. At the regular meeting preceding the annual meeting a nominating committee of five members, not officers of the Society, shall be appointed, and this committee shall, at least thirty days before the annual meeting, send to the Secretary the names of nominees for the offices falling vacant under the rules. In addition to such regularly appointed committee, any other five members or associates, not in arrears, may constitute an independent nominating committee, and may present to the Secretary, at least thirty days before the annual meeting, all the names of such candidates as they may select. All the names of such independent nominees shall be placed upon the ballot list with nothing to distinguish them from the nominees of the regular committee, and the Secretary shall at once mail the said list of names to each member and associate in the form of a letter ballot, it being understood that the assent of the nominees shall have been secured in all cases.

ART. 32. In the election of Vice-Presidents, each member and associate may cast as many votes as there are Vice-Presidents to be elected. He may give all these votes to one candidate, or dis-

tribute them among more, as he chooses. Managers shall be voted for in the same way.

ART. 33. Any member or associate entitled to vote may vote by retaining or changing the names on said list, leaving names not exceeding in number the officers to be elected, and returning the list to the Secretary—such ballot inclosed in two envelopes, the inner one to be blank and the outer one to be indorsed by the voter. No member or associate in arrears since the last annual meeting shall be allowed to vote until said arrears shall have been paid.

ART. 34. The said blank envelopes shall be opened by tellers at the annual meeting, and the person who shall have received the greatest number of votes for the several offices shall be declared elected.

MEETINGS.

ART. 35. The annual meeting of the Society shall be held on the first Thursday in November of each year, in the City of New York, unless otherwise ordered, at which a report of proceedings and an abstract of the accounts shall be furnished by the Council. The Council may change the place of the annual meeting, and shall, in that case, give timely notice to members and associates.

ART. 36. Other regular meetings of the Society shall be held in each year at such time and place as the Council may appoint. At least thirty days' notice of all meetings shall be mailed by the Secretary to members, honorary members, associates and juniors.

ART. 37. Special meetings may be called whenever the council may see fit; and the Secretary shall call a special meeting at the written request of twenty or more members. The notices for special meetings shall state the business to be transacted, and no other shall be entertained.

ART. 38. Any member, honorary member or associate may introduce a stranger to any meeting; but the latter shall not take part in the proceedings without the consent of the meeting.

ART. 39. Every question which shall come before the Society shall be decided, unless otherwise provided by these rules, by the votes of a majority of the members and associates present, provided there is a quorum.

ART. 40. At any regular meeting of the Society thirteen or more members and associates shall constitute a quorum.

ART. 41. Unless otherwise ordered, papers shall be read in the

order in which their text is received by the Secretary. Before any paper appears in the *Transactions* of the Society a copy of the paper shall be sent to the author, and, so far as possible, a copy of the reported discussion shall be sent to every member who took part in the same, with requests that attention shall be called to any errors therein.

ART. 42. The Society shall claim no exclusive copyright in papers read at its meetings, nor in reports of discussions, except in the matter of official publication with the Society's imprint, as its *Transactions*. The Secretary shall have sole possession of papers between the time of their acceptance by the Publication Committee and their reading, together with the drawings illustrating the same; and at the time of such reading, or as soon thereafter as practicable, he shall cause to be printed, with the authors' consent, copies of such papers, "subject to revision," with such illustrations as are needed for the *Transactions*, for distribution to the members and for the use of technical newspapers, American and foreign, which may desire to reprint them in whole or in part. The policy of the Society in this matter shall be to give papers read before it the widest circulation possible, with the view of making the work of the Society known, encouraging mechanical progress, and extending the professional reputation of its members.

ART. 43. The author of each paper read before the Society shall be entitled to twelve copies, if printed, for his own use, and all members shall have the right to order any number of reprints of papers at a cost to cover paper and printing; *provided*, that said copies are not intended for sale.

ART. 44. The Society is not, as a body, responsible for the statements of fact or opinion advanced in papers or discussions, at its meetings; and it is understood that papers and discussions should not include matters relating to politics or purely to trade.

AMENDMENTS.

ART. 45. These rules may be amended, at any annual meeting, by a two-thirds vote of the members present; *provided*, that written notice of the proposed amendment shall have been given at a previous meeting.

CONTENTS OF VOLUME IX.

LIST OF ILLUSTRATIONS, VOLUME IX.

PAPERS

OF THE

PHILADELPHIA MEETING

(XVIth),

BEING ALSO THE EIGHTH ANNUAL MEETING,

NOVEMBER, 1887.

CCLXI.

PROCEEDINGS

OF THE

PHILADELPHIA MEETING

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

November 28th to December 1st, 1887.

LOCAL COMMITTEE OF ARRANGEMENTS :—HORACE SEE, *Chairman ;* M. R. MUCKLE, *Secretary ;* J. S. Bancroft, J. C. Brooks, E. B. Coxe, S. A. Hand, Edward Longstreth, W. C. Williamson, Jas. Butterworth, A. B. Couch, H. C. Francis, Washington Jones, H. G. Morris, Walter Wood.

THE sessions of the Philadelphia (XVIth) Meeting, which was also the Eighth Annual Meeting of the Society, was held in Parlor C of the Continental Hotel. The opening session was called to order at eight o'clock on Monday evening, November 28th. The Secretary's registers showed the following members in attendance :

Albrecht, Otto	Philadelphia, Pa.
Aller, A.	New York City.
Allison, Robert	Port Carbon, Pa.
Almond, T. R.	Brooklyn, N. Y.
Ashworth, Daniel	Pittsburgh, Pa.
Babcock, Geo. H. (*President*)	New York City.
Bailey, W. H	New York City.
Baldwin, S. W	New York City.
Ball, F. H	Erie, Pa.
Bancroft, J. S	Philadelphia, Pa.
Barnard, Geo. A	Orange, N. J.
Barr, Wm. M.	Philadelphia, Pa.
Beach, Ch. S.	Bennington, Vt.
Beardsley, Arthur	Swarthmore, Pa.
Betts, Alfred	Wilmington, Del.

Betts, William ....Wilmington, Del.
Bilgram, Hugo..............................Philadelphia, Pa.
Bond, Geo. M...... ....Hartford, Conn.
Boyd, Jno. T.................... ....Philadelphia, Pa.
Brooks, E. C ....Cambridge, Mass.
Bulkley, H. W..............................New York City.
Butterworth, Jas...........................Philadelphia, Pa.
Carpenter, R. C............................Lansing, Mich.
Cartwright, Robert............. ....Rochester, N. Y.
Cavanagh, Joseph........... ....Philadelphia, Pa.
Christensen, Aug. C........................New York City.
Christie, James .. ..Pencoyd, Pa.
Clark, David........................... .. Catasauqua, Pa.
Coffin, John...............................Johnstown, Pa.
Collins, C. C..............................Newark, N. J.
Comly, Geo. N..............................Edgemoor, Del.
Cooper, Jno. H.............................Philadelphia, Pa.
Cotter, John ....Mt. Holly, N. J.
Couch, A. B ....Philadelphia, Pa.
Crane, Thos. S.............................Newark, N. J.
Cremer, James M ....Brooklyn, N. Y.
Cullingworth, G. R.........................New York City.
Dagron, J. G ....Pencoyd, Pa.
Davis, E. F. C.... ....Pottsville, Pa.
Davis, Isaac H.............................Dorchester, Mass.
Dean, F W..................................Cambridgeport, Mass.
De Kinder, J. J............................Philadelphia, Pa.
Dent, E. L ....Washington, D. C.
Denton, James E................ .... Hoboken, N. J.
Dingee, W. W........... ....Racine, Wis.
Doane, W. H ....Cincinnati, O.
Dodge, J. M ....Philadelphia, Pa.
Durfee, W. F ....Birdsboro, Pa.
Emery, A. H ....Stamford, Conn.
Engel, L. G .... .Brooklyn, N. Y.
Falkenau, Arthur...................... ....Philadelphia, Pa.
Felton, E. C...... ....Steelton, Pa.
Fladd, F. C ....New York City.
Foster, C. H ....Chicago, Ill.
Francis, H. C..............................Philadelphia, Pa.
Francis, W. H ....Philadelphia, Pa.
Fraser, N. D...............................Chicago, Ill.
Fritz, John........ ....Bethlehem, Pa.
Fuller, L. K ....Brattleboro, Vt.
Galloupe, F. E.............................Boston, Mass.
Giddings, C. M ....Massillon, O.
Gill, J. L., Jr................ ....Philadelphia, Pa.
Gould, W. V .... .Norwich, Conn.
Grant, Jno. J..............................Philadelphia, Pa.
Grinnell, F'd'k ....Providence, R. I.
Hague, Chas. A.............................New York City.

Halsey, F. A	New York City.
Hamilton, Alex., Jr	Johnstown, Pa
Haskins, H. S	Philadelphia, Pa.
Hawkins, J. T	Taunton, Mass.
Hayward, H. S	Jersey City, N. J.
Hazard, V. G	Wilmington, Del.
Hewitt, Wm	Trenton, N. J.
Higgins, M. P	Worcester, Mass.
Hillmann, Gustav	City Island, N. Y.
Hobbs, A. C	Bridgeport, Conn.
Holloway, J. F	New York City.
Hunt, C. W	New York City.
Huston, C. L	Coatesville, Pa.
Hutton, F. R. (*Secretary*)	New York City.
Hyde, C. E	Bath, Me.
Jenkins, John	Milton, Pa.
Jenkins, W. R	Bellefonte, Pa.
Jones, H. C	Wilmington, Del.
Jones, Washington	Philadelphia, Pa.
Kent, Wm	New York City.
Kerr, W. C	New York City.
Klein, J. F	Bethlehem, Pa.
Ladd, J. B	Steelton, Pa.
La Forge F. H	Waterbury, Conn.
Lanza, Gaetano	Boston, Mass.
Leavitt, E. D., Jr	Boston, Mass.
Le Van, W. B	Philadelphia, Pa.
Lewis, Wilfred	Philadelphia, Pa.
Lipe, C. E	Syracuse, N. Y.
Loring, C. H	Boston, Mass.
Low, F. R	Boston, Mass.
Lyall, W. L	New York City.
Lyne, L. F	Jersey City, N. J.
McBride, J	Brooklyn, N. Y.
MacKinney, W. C	Philadelphia, Pa.
Main, C. T	Lawrence, Mass.
Matlack, D. J	Philadelphia, Pa.
Mattes, Wm. F	Scranton, Pa.
May, De Courcy	Philadelphia, Pa.
Meyer, J. G. A	New York City.
Miles, F. B	Philadelphia, Pa.
Miller, Walter	Cleveland, O.
Mirkil, T. H., Jr	Philadelphia, Pa.
Morgan, C. H	Worcester, Mass.
Morgan, Jos., Jr	Johnstown, Pa.
Morgan, T. R., Sr	Alliance, O.
Morris, H. G	Philadelphia, Pa.
Mucklé M. R	Philadelphia, Pa.
Müller, M. A	Newark, N. J.
Murphy, E. J	Hartford, Conn.
Murray, S. W	Milton, Pa.

Naylor, J. S. . . . . Philadelphia, Pa.
Nicolls, Wm. J . . . . Philadelphia, Pa.
Odell, W. H. . . . . Yonkers, N. Y.
Pankhurst, J. F . . . . Cleveland, O.
Parker, Chas. D . . . . Worcester, Mass.
Parks, E. H . . . . Providence, R. I.
Parsons, H. de B. . . . . New York City.
Parsons, Willard P . . . . Hoosick Falls, N. Y.
Perkins, Geo. H. . . . . Philadelphia, Pa.
Philips, Ferd . . . . Philadelphia, Pa.
Phillips, Franklin. . . . . Newark, N. J.
Porter, H. F. J . . . . New York City.
Potter, Chas., Jr. . . . . Plainfield, N. J.
Pusey, C. W. . . . . Wilmington, Del.
Randolph, L. S . . . . Mt. Savage, Md.
Raynal, A. H. . . . . New York City.
Ridgway, J. T. . . . . Trenton, N. J.
Roberts, F. C. . . . . Philadelphia, Pa.
Robinson, J. M . . . . New York City.
Rood, V. H. . . . . Philadelphia, Pa.
Sanguinetti, P. A . . . . Philadelphia, Pa.
Schleicher, A. W . . . . Philadelphia, Pa.
Schuhmann, Geo. . . . . Reading, Pa.
Schumann, F. . . . . Trenton, N. J.
Schutte, Louis. . . . . Philadelphia, Pa.
See, Horace . . . . Philadelphia, Pa.
Sellers, Coleman, Jr. . . . . Philadelphia, Pa.
Sharp, Joel. . . . . Salem, O.
Sinclair, Angus . . . . Cincinnati, O.
Smith, C. D . . . . Plantsville, Conn.
Smith, Geo. H . . . . Providence, R. I.
Smith, Oberlin. . . . . Bridgeton, N. J.
Smith, Sidney L . . . . Boston, Mass.
Snell, H. I . . . . Philadelphia, Pa.
Sorzano, J. F . . . . New York City.
Spies, Albert . . . . New York City.
Sprague, W. W . . . . Lake, Ill.
Stahl, A. W. . . . . Brooklyn, N. Y.
Stewart, W. G. . . . . Reading, Pa.
Stiles, N. C. . . . . Middletown, Conn.
Stillman, F. H. . . . . New York City.
Stratton, E. P . . . . New York City.
Strong, Geo. S. . . . . New York City.
Sunstrom, K. J . . . . Worcester, Mass.
Sweet, J. E. . . . . Syracuse, N. Y.
Tatnall, Jas. E. . . . . Bethlehem, Pa.
Taylor, F. W. . . . . Nicetown, Pa.
Taylor, J. A . . . . Wilmington, Del.
Thompson, C. T . . . . Philadelphia, Pa.
Thorne, W. H . . . . Philadelphia, Pa.
Thurston, R. H . . . . Ithaca, N. Y.

Tilden, J. A ....Boston, Mass.
Tobey, G. A..Newark, N. J.
Tompkins, S..Crozet, W. Va.
Towne, H. R ....Stamford, Conn.
Townsend, David ....Philadelphia, Pa.
Trautwein, A. P ....Brooklyn, N. Y.
Trump, C. N ....Wilmington, Del.
Ulmann, C. J..Providence, R. I.
Unger, Jno. S ....Steelton, Pa.
Ward, Chas..Charleston, W. Va.
Ward, W. E..Portchester, N. Y.
Warner, W. R..Cleveland, O.
Warren, B. H..Boston, Mass.
Watson, Wm..Boston, Mass.
Watts, Geo. W ....Philadelphia, Pa.
Webb, J. B..Hoboken, N. J.
Webber, S..Charlestown, N. H.
Webber, S. S ....Reading, Pa.
Webber, W. O ....Boston, Mass.
Webster, J. H ....Boston, Mass.
Weeks, G. W ....Clinton, Mass.
Weightman, W. H..New York City.
Wells, J. L..New York City.
Wheeler, F. M..New York City.
Wheelock, Jerome ....Worcester, Mass.
White, Joseph J ....Philadelphia, Pa.
White, Maunsel ....Bethlehem, Pa.
Whitehead, Geo. E ....Providence, R. I.
Whiting, C. W ....Pottsville, Pa.
Whitney, B. D ....Winchendon, Mass.
Whitney, W. M..Winchendon, Mass.
Wilcox, Jno. F..Pittsburg, Pa.
Wilder, M. J ....Philadelphia, Pa.
Wiley, W. H ....New York City.
Williams, S. T ....Tacony, Pa.
Williamson, W. C ....Philadelphia, Pa.
Willson, F. N ....Princeton, N. J.
Wilson, J. E ....New York City.
Witherow, J. P..Pittsburg, Pa.
Wood, De Volson ....Hoboken, N. J.
Wood, Walter ....Philadelphia, Pa.
Woodbury, C. J. H ....Boston, Mass.
Woolson, O. C ....Newark, N. J.
Worthington, C. C ....New York City.
Wright, J. Q..New York City.
Wyman, H. W ....Worcester, Mass.
Yost, T. M ....Middletown, Pa.

There were also many ladies and several guests in attendance. The first business was the President's Annual Address, entitled

"The Engineer: his Commission and his Achievements," which appears among the papers of the meeting. Following this address, the first paper of the session was read by Prof. Jno. E. Sweet, entitled "A New Principle in Steam Piston Packing." This was discussed by Messrs. Strong, Webb, Wood, Ball, Sinclair, Almond, Smith, Parsons, Woolson, Ashworth and Babcock. The two Topical Queries in reference to driven wells were discussed by Messrs. Briggs, Woodbury, Hutton, McBride, Barr, Oberlin Smith and Babcock. The queries were: "What is the best form of pump to use with driven wells where the lift is ten to twenty feet and air is likely to get into the suction? Should the pump be single or duplex and with piston or plunger?" and the second was, "Have you used driven wells successfully; of what sizes or depths and singly or in groups?" After announcements by the Secretary and the Local Committee, and the appointment of Tellers to count the ballots for officers of the Society, the Session adjourned.

Second Day. November 29th.

The meeting was called to order at ten o'clock. The Reports of the business session were presented as follows:

REPORT OF THE COUNCIL.

The Council would present its Annual Report under the Rules. It has held eight meetings during the year for the transaction of business, and the following is a summary of its action, besides the usual routine labor:

The substitution has been effected of a new and improved form for application for membership in the Society under the Rules, by which uniformity in statement of professional competence should be secured, and by which the responsibility of members who act as proposers of a candidate should be made more definite. The candidate applying (by Form D) mentions the names of those who can and will propose him, and letters are then solicited by the Secretary from such persons on a special form (Form E), by which the proposers confidentially may give the Council the benefit of their opinions as to the eligibility of the candidate to the grade which he seeks. The new form also codifies somewhat the general statements of the Rules in respect to the several grades of members, for

the guidance of such proposers. It was further resolved, that only such persons as cordially indorsed an application for membership should be recorded as proposers of such candidate.

The matter of the uniformity of badges of membership in all grades in the Society has been presented as unfortunate, and has been referred to a special committee. Applications for exchange of transactions with other societies and publications have been made and granted. The Council has also accepted a most cordial invitation to hold its spring meeting of 1888 in the city of Nashville, Tenn., the date to be hereafter agreed upon.

The Council has passed favorably upon the applications of 105 candidates during the Society's year. The present membership of the Society, including those joining at this meeting is 813, distributed among the grades as follows:

Honorary Members	15
Life Members	7
Full Members	713
Associate Members	35
Junior Members	43
Total	813

The above list includes the members whose dues to the Society for the preceding year were paid up to January 1, 1887. At that time, under the resolutions of the Council (reported on page 8, Vol. VIII.), the members in arrears for the preceding year or more were dropped to a "Suspended List," and did not appear in the Roll in the catalogue. That list at the beginning of the year numbered 20. Six have paid up arrearages this year, one has resigned, and from the remainder no reply has been received to letters addressed to them, and even their addresses are unknown in some cases.

The losses by death since the last annual report in Volume VIII., have been as follows:

Homer Hamilton	Member.
John B. Root	"
Bishop Arnold	"
B. F. Emerson	Associate.
Wm. L. Nicoll	Member.
Jackson Bailey	Associate.
James Sheriffs	Member.

The Council would present the report of its Tellers, as follows:

The undersigned were appointed a committee of the Council to act as tellers under Rule 13, to count and scrutinize the ballots cast for and against the candidates proposed for membership in The Society of Mechanical Engineers, and seeking election before the XVIth meeting of the Society in November, 1887.

They would report that they have met upon the designated days in the office of the Society, and proceeded to the discharge of their duties.

They would certify, for formal insertion in the records of the Society, to the election of the appended named persons to their respective grades upon the lists No. 1 and No. 2, colored respectively pink and yellow.

There were 326 votes cast in the ballot upon the pink list, of which 14 were thrown out because of informalities, and there were 364 votes cast using the yellow ballot, of which 8 were thrown out because of informalities.

The list is appended below.

WM. H. WILEY, } *Tellers.*
WM. KENT, }

AS MEMBERS.

Ames, Wm. Lewis	Terre Haute, Ind.
Atkins, James	Montclair, N. J.
Belcher, A. W	Rondout, N. Y.
Boyd, John T.	Philadelphia, Pa.
Buckingham, R. H.	Chicago, Ill.
Charnock, John M	Jamaica Plains, Mass.
Crowell, Luther C.	New York City.
De Valin, Charles E.	Washington, D. C.
Fladd, Frederick C.	New York City.
Gage, Howard	Saginaw, Mich.
Hildrup, W. T.	Harrisburg, Pa.
Jones, Willis C.	Cincinnati, Ohio.
Jordan, Samuel S.	Philadelphia, Pa.
Krause, Arthur	Jersey City, N. J.
Laforge, Frederick H.	Waterbury, Conn.
Miles, Frederick B.	Philadelphia, Pa.
Muncaster, Walter J.	Cumberland, Md.
Naylor, John S.	Philadelphia, Pa.
Pearson, Wm. A	Scranton, Pa.
Pierce, Wm. H.	Aurora, Ill.
Russel, Walter S.	Detroit, Mich.
Sague, James E.	Buffalo, N. Y.
Schumann, F.	Trenton, N. J.
Simonds, George F.	Boston, Mass.

Smith, A. P	Washington, D. C.
Southworth, E. P. B.	Rochester, N. Y.
Sprague, Wm. T	Minneapolis, Minn.
Swinscoe, Charles	Clinton, Mass.
Towle, Wm. M.	Terre Haute, Ind.
Whitlock, Roger H	College Sta., Texas
Wilson, James E.	New York City.
Wright, Alex. P.	New York City.
Yest, Thomas M.	Middletown, Pa.

PROMOTED FROM JUNIOR TO FULL MEMBERSHIP.

Edward L. Dent.

AS ASSOCIATES.

Bagg, S. F.	Watertown, N. Y.
Copeland, E. T.	New York City.
Haskins, Harry S.	Philadelphia, Pa.
Lemoine, Louis R.	Philadelphia, Pa.

AS JUNIORS.

Burgess, Isaac C.	Fall River, Mass.
Foran, George J.	Philadelphia, Pa.
Huson, W. S.	Taunton, Mass.
Merriam, Henry P.	New York City.
Rowland, George	New York City.
Sinclair, George M.	Philadelphia, Pa.
Wilkes, J. Frank	Charlotte, N. C.

Respectfully submitted.

By the Council.

The report of the Finance Committee to the Council at its final meeting was also read, giving the summary of their work during the fiscal year just closed. This report was as follows:

The Finance Committee would respectfully report to the Council, the following statement of the receipts and expenditures of the Society, under their direction for the twelve months ending November 1, 1887.

The receipts have been as follows:

Initiation Fees	$1,480 00
Current Dues	6,770 25
Past Dues	350 00
Advance Dues	72 05
Paper Sales	456 90
Binding	358 97
Library Permanent Fund	178 58
Library Current Fund	266 52

Brought Forward	$9,933 27
Badges	396 93
Engraving	105 75
Life Membership	150 00
Profit and Loss	25
Total Receipts	$10,586 20
Balance in Treasurer's hands November 1, 1886	984 69
	$11,570 89

The expenditures have been as follows:

General Printing and Stationery	$700 28
Postage	507 17
Library Permanent Fund	73 57
Library Current Fund	72 00
Expense and Rent	951 00
Salaries	2,663 26
Engraving	517 56
Binding	791 95
Meetings	400 38
Office Fixtures, etc	95 66
Badges	417 50
Traveling	57 47
Printing Transactions and Pamphlets	3,165 23
Total Expenditures for the year	$10,413 03
Balance in Treasurer's hands November 1, 1887	1,157 86
	$11,570 89

Of this balance $1,112.31 stands to the credit of the Society for the library in savings banks, and $45.55 is in the hands of the treasurer as a balance to carry forward to the next fiscal year.

There remains on the books outstanding $573.69 as due to the Society from thirty-one persons in the membership, all of which is probably collectable, and on the Suspended List brought over from January 1, 1887, $420, coming from several years back, probably uncollectable.

Respectfully submitted,

By the Finance Committee.

The Library Committee's Report was also presented to the Society as follows:

The Library Committee would present its Third Annual Report of the continued success of the plans outlined in the original report found on page 11 of Volume VI. of Transactions. Circulars were again sent out in the beginning of the year to the mem-

bers who had not already subscribed, explanatory of the scheme, with a form of agreement requesting contributions in any of the three following forms:

(*a*) Subscriptions to a Permanent Fund, in payments of $10 or upwards (payable in installments if preferred).

To this there have been responses since the last report from four members:

Edward L. Dent	$25 00	
Charles A. Fingal	10 00	
Frank L. Griswold	20 00	
E. D. Leavitt, Jr.	100 00	
		$155 00
Previously reported on this fund		678 40
Total		$833 40

(*b*) Annual subscriptions to an amount of two dollars to a fund for current library expenses, payable as an increase to the annual dues and at the same time.

To this there have been responses since the last report above referred to, from 27 members:

W. J. Nicolls,	W. W. Dingee,	Robert Hoe,
James Butterworth,	Stephen E. Babcock,	W. H. Jones,
Thomas J. Borden,	E. D. Leavitt, Jr.,	John Jenkins,
R. B. Collier,	Norman T. Fraser,	D. R. Fraser,
Lewis Miller,	Pardon Armington,	Peter Kirkevag,
Charles A. Knight,	Walter C. Kerr,	H. D. Williams,
Fred. W. Gordon,	W. H. Francis,	Walter Wood,
Edward S. Cobb,	Charles A. Fingal,	W. J. Root,
C. H. Foster,	James Francis,	Frank L. Griswold.

There are, therefore, 147 members now regularly contributing to this fund by this plan of a small increase in the dues, and it is urged that others should also co-operate in the further extension of this plan, and thus induce a more wide-spread interest in the Library among the members.

(*c*) Direct contributions of books and papers of value. To this there have been a number of responses during the year, particularly by members residing abroad.

The following list contains the contributions not catalogued in previous report:

By Hugo Bilgram:
Slide Valve Gears.

By W. A. Morrison:
Practical Engineering.

By Oberlin Smith (pamphlet):
Flow of Metals in the Drawing Process.

By W. H. Springer, Senr.:
Recent Locomotives, with Illustrations.

By A. H. Emery:
Patent Office Reports, 1856-71.

By J. Bauschinger:
Mittheilungen aus dem Mechanisch Laboratorium der K. Technischen Hochschule in Münschen.
Conclusions of the Conference at Munich and Dresden, on Uniform Methods of Investigation in the tests of standard material.

By Samuel Webber:
Manual of Power.

By Military Service Institution, Governor's Island:
Sets of Journal of Military Service Institution of the United States.

By Prof. Reuleaux:
New Discoveries in Steam Pumps and their operation.

By Prof. Dwelshauvers Dery:
L'enseignement de la Mechanique appliqué.

By C. W. Livermore:
Cooking and Heating by Gas. William H. Denny, F.R.S.
Speed and carrying of Screw Steamers. William H. Denny, F.R.S.
Economical advantages of Steel Ship Building. William H. Denny, F.R.S.
On Mr. Mansel's and the late Mr. Froude's methods of Analyzing the results of Progressive Speed Trials. William H. Denny, F.R.S.
Local Education in Naval Architecture. William H. Denny, F.R.S.
On Cross Curves of Stability. William H. Denny, F.R.S.
Stowage of Steam Ships. E. F. Purvis.
Denny's Confidential Report on the inclination of the S. S. "Fernbrook."
Official Catalogue of International Inventions Exhibition, London.
Official Guide to the International Exhibition, London.

By Thomas Shanks & Co.:
Set of Photographs of Shop and Tools of their Manufacture.

By George L. Fowler:
A System of Estimating for Foundry Work. By A. Messerschutt.
Translation—Fuels, Evaporation and Combustion. By J. Buchetti.

By Engineering:
Cable or Rope Traction. Bucknall Smith.

By The Marquis de Coligny:
Oscillation de l'Eau ; les Machines hydrauliques. 2 vols.

Anonymous:
Annual Report of the Toronto Water Works.
Club de Engenharia, Rio de Janeiro.
Revista Mensal Engenharia. Nos. 1, 2, 3.

By W. F. Durfee:
Iron and Steel and the Mitis Process.

By J. Wiley & Sons :
Weisbach's Mechanics. Vol. 2.

By Frederick Cook :
Proceedings of Louisiana Sugar Planters' Association, July, 1887.

By J. C. Trautwine:
Excavation and Embankments.
Railroad Curves.
Engineer's Pocketbook.

By George M. Bond:
Standards of Length and their Subdivision.
Standards of Length as applied to Gauge Dimensions, pamphlets. 2 lectures.

By A. P. Smith:
Graphical Statics of Mechanism, by Gustav Herrmann. Translated and annotated by A. P. Smith.

Moreover, since the previous report, the Society has acquired by purchase the complete proceedings of the Institution of Mechanical Engineers of Great Britain, from 1847 to the founding of this Society, since which time the volumes have come regularly by exchange.

The Journal of the Iron and Steel Institute of Great Britain, from 1873 to date, has also been presented to the Library, and that publication hereafter will be one of the exchanges.

In addition to the files of papers as previously published, we have also added to our exchanges:

L'Industria, Milan, Italy, July, 1887.
Giornal del Genio Civile, Rome, Italy, January, 1881.
Industries, London and Manchester, July, 1886.
Practical Engineer, Manchester, March, 1887.
Glaser's Annalen, Berlin, January, 1887.
Indian Engineering, Calcutta, January, 1887.

The Society also exchanges with the Canadian Society of Civil Engineers, Montreal, Canada, and Bureau of Naval Intelligence, Washington, D. C., which arrangement has gone into effect since last report.

By purchase from Mr. Frank E. Galloupe, of Boston, the missing volumes of London Engineering have been in part supplied, and the set now only needs Vols. I. and II.

The report of the Committee on securing Uniformity in Methods of Test and in Test Specimens was submitted by its hairman as follows:

Mr. H. R. Towne.—I can simply report on behalf of our committee that some progress has been made since the last

meeting, but not what we had hoped to accomplish. This is largely due to the absence of Mr. Henning, the most active member of the committee and the one who has done most of its work, who has been for nearly six months past in Paris on business, and who has not yet returned. I may state in the interim that, as previously reported, a second set of specimens and test reports were sent out to almost every institution that we knew of as having a testing machine, and that returns have now been received from almost all of them, with the exception of a few which did not have machines of a proper character. The results of this work are undoubtedly going to be of much interest and value, and I think I can safely promise that at the next meeting of the society a definite report will be made.

The report of the tellers to count the ballots cast for officers for the ensuing year was presented as follows:

In compliance with Article No. 34 of Rules of the Society, the tellers appointed to count the ballots for election of officers for the year 1887-88 respectfully submit their report as follows:

There were 395 votes cast.

For President..........	Horace See received.........	388—	scattering 7
" Vice-Presidents.....	H. G. Morris received........	394	
" "	W. S. Baker received........	392	
" "	C. J. H. Woodbury received..	393—	" 3
" Treasurer..........	W. H. Wiley received.......	395	
" Managers...........	Stephen W. Baldwin received	402	
" "	Frederick Grinnell received..	390	
" "	Morris Sellers received	390—	" 2

There were five votes cast which were thrown out for informality.

J. A. TILDEN, } *Tellers.*
W. V. GOULD, }

No new business being presented, the professional papers were taken up. The two papers of Mr. Henry I. Snell, "Experiments and Experiences with Blowers," "An Economical Method of Heating and Ventilating an Office and Warehouse Building," were presented together. They received discussion by Messrs. Woodbury, Ashworth, Denton, Mattes, Schuhmann, Felton and Thurston. The paper of Professor Thurston, entitled "Internal Friction of non-Condensing Engines," was discussed by Messrs. Denton and Towne. Mr. Oberlin Smith's paper was next read, "Power Press Problems," and discussed by Messrs. Grant, Lewis,

Crane, Coffin and Stiles, and the time for adjournment having been reached, the session adjourned. The afternoon was given to excursions to points about the city of Philadelphia, as detailed in the excursion memorandum.

EVENING SESSION, TUESDAY, NOVEMBER 29TH.

THE third session for professional papers was held in the evening at eight o'clock. The first paper was that of Mr. John J. Grant, entitled "The Milling Machine as a Substitute for the Planer in Machine Construction," and was discussed by Messrs. Towne, Webb, Potter, Denton, Sweet, Durfee and Hawkins. The paper of Mr. Frank Van Vleck, entitled "Standard Section Lining," was discussed by Messrs. Hutton, Grant, Towne, Willson, Halsey, Coffin, Ladd and Weightman, and the debate resulted in a motion that a committee be appointed by the chair to consider the matter of recommending some system of section lining which could be accepted as standard among engineers. This motion was debated by Messrs. Sweet, Denton and Kent, but when the vote was taken the motion was lost. The debate on the motion is printed in connection with that upon the paper, as they were so intimately connected together, and appears in the subsequent part of the volume. The discussion was in part also upon the matter of precedent as to the work and results of such committees of the society in connection with the former debate at pages 365 and 877 of Volume VI. of the Transactions.

The next paper was that of Mr. Percy A. Sanguinetti, entitled "On the Divergencies in Flange Diameters of Pumps, Valves, etc., of different Makers," and was discussed by Messrs. Wm. O. Webber, Taylor, Towne, Le Van, Weightman, Engel, Raynal, Kent, Mattes, Hawkins, Davis, Towne. This debate resulted in a motion for the appointment of a committee to consider this subject, which motion was carried, and the chair subsequently appointed Messrs. P. A. Sanguinetti, E. F. C. Davis, W. F. Mattes, S. S. Webber, A. H. Raynal.

The final paper of the evening session was by Mr. Wm. O. Webber, entitled "Centrifugal Pumps and their Efficiencies," and was discussed by Messrs. Thurston, Wood, Bilgram and Denton. A late hour having been reached, at the close of this debate, the meeting adjourned.

Fourth Session. Wednesday, November 30th.

The session was called to order at eleven o clock, instead of at the earlier hour first assigned, in order to admit of a visit to a water-gas establishment near the city.

The first paper was that of Professor Gaetano Lanza, entitled the "Friction in Toothed Gearing," and was discussed by Messrs. Webb, Bilgram, Lewis and Hawkins. Professor Lanza also presented a paper by Mr. Jerome Sondericker, entitled "An Investigation as to how to Test the Strength of Cements," which was discussed by Messrs. Parsons and Denton. Mr. H. DeB. Parsons' paper on "The Influence of Sugar upon Cement," was discussed by Messrs. Denton and Engel. The paper by Mr. John Coffin, on "Steel Car Axles," was discussed by Messrs. Webber, Hewitt, Denton, Felton, Almond, Taylor, Hawkins and Babcock, and at its close the author exhibited some interesting experiments upon small steel bars in the heat of a Bunsen flame, two pieces being clamped together in tongs, heated to a low orange. It was found that it required considerable force to separate them, and that they did not separate exactly at the cleavage point of previous breakage.

The session was resumed after luncheon, with the paper by Mr. Edgar C. Felton on "Notes on Results obtained from Steel Tested shortly after Rolling." This was discussed by Messrs. Reese and Hewitt. The paper by Lewis F. Lyne, on "The Use of Kerosene Oil in Steam Boilers," was discussed by Messrs. Ridgway, Halsey, Engel, Denton, Schuhmann and Babcock. The paper by Mr. James M. Dodge, entitled "A New Method of Stocking and Reloading Coal," was then discussed by Messrs. Mattes and Davis. The paper by Mr. O. C. Woolson was illustrated by further black-board illustrations and a model, and was discussed by Professor Denton. "An Interesting Indicator Diagram" was the title of a brief paper from Mr. C. E. Emery, discussed by Mr. Hutton, and the paper of Mr. Frank H. Ball, "An Improved form of Shaft Governor," was discussed by Messrs. Webb, Hawkins, Babcock, Denton, Kerr, Bilgram, Lyne, Davis and Ladd.

After the close of this final paper, the Topical Discussions were taken up until the hour of adjournment. Messrs. Raynal, Collins, Bilgram, Towne, Lyne, Engel, Doane, Mattes, Woolson, Falkenau, McBride answered the query: "What is the best way

to secure tight fit of set screws tapped into heavy parts of a machine?" The topic: "Are roller-bushings expedient in journals at low velocities and under high pressures?" was discussed by Messrs. Warner and Woolson. The best material for lining brake-straps on elevators, cranes, drums, etc., was discussed by Messrs. Leavitt, Raynal and Davis. Mr. T. R. Morgan, Sr., spoke upon the best material for moulds for complicated steel castings. Messrs. Morgan, Towne, Mattes, Leavitt, Raynal, Kent and Davis spoke upon the effect of adding percentages of wrought iron or steel scrap in the foundry cupola or ladle. Messrs. Lyne, Towne, Woolson, Davis, Babcock and Mattes discussed the best kind of pig iron for use in light castings where easy tool-treatment is the essential rather than strength.

At the close of the last discussion new business was in order, and Mr. W. H. Doane presented a most cordial invitation for the society to stop over as a body in Cincinnati on its way to the spring meeting in Nashville. By vote this matter was left to the council to arrange for, if possible.

The following preamble and resolutions were then presented by Mr. Kent, warmly seconded, and passed by a rising vote:

Whereas, The American Society of Mechanical Engineers, in closing its Eighth Annual Convention, desires to place on record its appreciation of the many courtesies extended by the several members of the local committee, the committee of ladies, the manufacturers of the city and vicinity, the United States Government and other officials, and the many citizens of Philadelphia, who have contributed to make this meeting one of the most memorable and enjoyable in the Society's experience:

Resolved, That we extend our heartiest thanks to Horace See, chairman, and to the members of the local committee for their untiring efforts, and their brilliant success in arranging and carrying out the programmes of excursions and entertainments, unsurpassed in their variety and unequaled in their excellence.

To John De Keim, of the Philadelphia and Reading R. R. Co., for transportation facilities furnished us in the excursions on the Delaware River.

To Frederick B. Miles, Jos. J. DeKinder, Walter Wood, S. Ashton Hand, and J. M. Dodge, members of the local committee on transportation, for their highly successful efforts in diminishing the magnificent distances of the city, so enabling us to visit the various industrial establishments.

To the Hon. Daniel M. Fox, Director of the U. S. Mint, for his courtesy in extending an invitation to visit that institution.

To the proprietors and managers of the following industrial establishments, who have kindly thrown them open to our inspection:

Baldwin Locomotive Works.

Bement, Miles & Co.

Bergner & Engel Brewing Co.

The Wm. Cramp & Sons Ship and Engine Building Co.

Henry Disston & Sons.
The Keystone Light & Power Co.'s Incandescent Electric Light Station.
The Keystone Watch Case Co.
The Kensington Engine Works.
The I. P. Morris Co.
The Otto Gas Engine Works.
Wm. Sellers & Co.
The Southwark Foundry & Machine Co.
Williamson Bros.
Wm. Wharton, Jr., & Co.
Riehle Bros.

To the Managers of the Pennsylvania Museum and School of Industrial Art, for their invitation to visit the Museum and School.

To John L. Ogden, Chief Engineer of the Philadelphia Water Department, for his invitation to visit the Spring Garden Pumping Station.

To the citizens of Philadelphia, for affording us the privilege of a private view of the magnificent art collection at the Academy of Fine Arts, and the delights of a reception in that beautiful building.

Last, but not least,

To the ladies of the local committee for their hospitable entertainment of the visiting ladies, in showing them the various points of interest in the city of Brotherly and Sisterly Love.

At the close of the lunch provided by the Bethlehem Iron Company on Thursday, the following resolutions were prepared and offered, and passed with acclamation:

The American Society of Mechanical Engineers, desiring to put on record their most enthusiastic appreciation of the courtesies which have been extended to them by the
Lehigh University,
Lehigh Zinc & Iron Co., and
The Bethlehem Iron Co.,
have prepared the following resolutions:

Resolved, That our thanks are hereby tendered to our Bethlehem hosts for their most generous and hospitable entertainment of the Society.

Resolved, That we carry away from Bethlehem a more thorough appreciation than ever of the great interests, national in their character, which have been committed to our profession, and of the triumphs, world-wide, of mechanical engineering as applied to great industries.

Resolved, That these expressions be conveyed to our hosts with the assurance that they are no mere formal recognition of what we have this day enjoyed.*

After the passage of the resolutions of thanks, the convention was adjourned.

* Considerable amusement was afforded by the presentation, in the form of a minority report to the above, of the following travesty:

Resolved, 1st. It is a long while since we have had so bully a time. In fact not since last night.
2d. We have had a real bully time to-day.
3d. Let us have as bully a time again soon. (!)

Entertainments and Excursions.

The afternoon of Tuesday, November 29, was left without assignment of professional sessions, to admit of visits to engineering establishments which had extended invitations to the members. Parties were made up on that day for Baldwin Locomotive Works, Bement, Miles & Co., Bergner & Engel Brewing Co., Wm. Cramp & Sons, Keystone Light & Power Co., Keystone Watch Case Co., Kensington Engine Works, I. P. Morris Co., Otto Gas Engine Works, Wm. Sellers & Co., Southwark Foundry & Machine Co., Williamson Bros., Wm. Wharton, Jr., & Co., Riehle Bros., and to the Pennsylvania Museum and School of Industrial Art. Small parties also visited Henry Disston & Sons' establishment at Tacony on Wednesday morning to see a fuel-gas plant in operation at that time, and on Wednesday afternoon, a test of the Gaskill pumping engine was in progress at the Spring Garden Pumping Station, and was witnessed by several.

On Wednesday evening a social reunion and reception was held for the Society in the Academy of Fine Arts on Broad and Cherry Streets, by citizens of Philadelphia. A committee of representative Philadelphians acted as hostesses to receive the guests, and an opportunity was given to see the regular collection of art treasures and a special loan collection, on private view. The Academy was decorated with a profusion of flowers, and an orchestra contributed to the enjoyment of all. A fine supper was served in the lecture hall below.

On Thursday, a special train was provided by the P. & R. R. R., on the North Penn Division, and took the party to Bethlehem, Pa. The conveyances at the depot took the visitors to the Lehigh University, to the Lehigh Zinc and Iron Co., and finally to the office of the Bethlehem Iron Co. After a lunch in one of the rooms, the party was piloted through the works, and examined with special interest the buildings for the shops in which the governmental contracts are to be elaborated. After passing the votes of thanks, the party in the main boarded the train in the yard of the Iron Co., and returned to Philadelphia, to enjoy the further courtesy of their hosts, who had provided theater parties as a closing entertainment. Operetta or tragedy was chosen as the varying taste of the members dictated. Quite a number remained over Friday for further opportunity to visit places of professional interest.

In addition to the entertainment provided for visiting members, a committee of ladies of Philadelphia put themselves at the service of the ladies present, and escorted them on different days to the U. S. Mint, Independence Hall, Girard College, Fairmount Park, and to Memorial and Horticultural Halls, besides arranging a social reception for them in the hotel parlors, on an evening while a professional session of the members was in progress.

CCLXII.

PRESIDENT'S ADDRESS, 1887.

THE ENGINEER: HIS COMMISSION AND HIS ACHIEVEMENTS.

BY GEORGE H. BABCOCK, NEW YORK CITY.
(President of the Society 1886-87.)

THE eighth annual meeting of our Society opens under auspicious circumstances. This Society, which less than eight years ago, was small and weak as a child just come into life, has leaped into vigorous manhood in so short a space of time that it seems almost to have sprung full-fledged from the brains of its founders, even as the fabled Athena from the head of Zeus. And possibly there is more in this comparison than at first might appear, for did not Hephæstus, chief engineer of the heavenly host, by his skill and might bring that goddess into existence? and was not she the projector and patron of those places named in her honor "Athenea," where learned men met to read papers and discuss matters of moment, even as we are gathered together at this time?

This Society, but lately only a conception in the brain of a few engineers, has now attained to a membership of over 800, among whom are more mechanical engineers than the whole world could have mustered 100 years ago. This is a cause for congratulation, but it is only one of the reasons why we meet to-night under auspicious circumstances. Another is that the character of the work done by the Society is improving year by year, while its transactions are being sought for by similar societies in different parts of the world. But the most auspicious element of the time is the fact that we are in the midst of great and wonderful achievements in science and engineering, while the opportunities opening before us are simply marvelous in their possibilities. As mechanical engineers we may perhaps be pardoned if we inquire what relations we bear to what has been done and what remains to be done in the line of the world's progress.

As was very truthfully said at the preliminary meeting for the organization of this Society, by one whom we delight to honor, A. L. Holley, the profession of the mechanical engineer underlies

all forms of engineering as well as architecture, manufactures and commerce, while Science even is dependent upon it for her means of progress. In like manner Hephæstus seems to have been the indispensable member of the Olympian household, for Jupiter was indebted to him for his thunderbolts, Achilles for his shield, all the gods for their dwellings, and even Aphrodite, the goddess of love, for her girdle. Thus, as was stated, the civil engineer may plan railroads, with their bridges, tunnels, cuttings and embankments, or lay out cities and waterways, but he cannot execute nor equip them without the aid of the mechanical engineer. Neither can the mining, electrical, marine or hydraulic engineer get along without calling in the assistance of his brother who deals mainly with dynamics. It is not necessary to elaborate this idea at this time, or before this audience. The fact is almost self-evident.

But mechanical engineering is not only the most important, it is also the oldest art or profession on the face of the earth, excepting only agriculture, if, indeed, that can be excepted, for not until a very recent date could that be said to have risen to the dignity of a profession. This is evidenced by the almost universal presence of an artificer among the mythological gods of all nations, as the Hindoo Tvashtri, the Egyptian Ptah, the Scandinavian Thor, the Roman Vulcan, the Greek Hephæstus, etc.

Engineering has, moreover, a divine commission, which is coeval with the creation of man, and co-extensive with the resources of nature.

It is written in the earliest history of which we have any knowledge—a history which the critics who delight to tear down, rather than build up, claim to have been but a compilation of much earlier manuscripts,—that after the Creator had made the world and fashioned it for the abode of man, and had placed thereon the first human pair, he charged them to "replenish the earth *and subdue it*." He had been at work for ages, storing away in the bowels of Mother Earth a vast accumulation of metals, minerals, compounds, salts, liquids, and other treasures, for the future use of man. He had created the forces which, acting under his well-considered laws, carry on the processes of nature, drive the chariots of the heavenly host and guide the infinite universe in one grand symphony of intermingling but harmonious motion. He had made man with God-like powers, and, having placed him upon the earth, he gave him as his task to subdue it to his own uses; to delve after the hidden treasures, that they might

add to his comforts and convenience; and to enslave the mighty forces of nature, compelling them to do his bidding even as they do the bidding of the Almighty.

This, then, is the commission under which the mechanical engineer from that day to this has been working, and under which he is working to-day, looking forward to a future which shall bring with it the complete fulfillment of the task thus divinely imposed.

Let us inquire first, what has been already accomplished toward this end, and then perhaps consider what achievements remain for us and those who come after us. Already man has dug deep and made grand discoveries of hidden treasures, such as iron, copper, gold, and about fifty other metals, coal, salt, sulphur, asphalt and several hundred other minerals, besides gems of untold value. From these the mechanical engineer has created wondrous constructions, and for them discovered various uses, all of which have contributed to the elevation and happiness of the race. We have but to recall the condition of man in the so-called stone age—which, by the way, is only relative, for there has not been a time from the creation of man up to the present, when a stone age has not existed somewhere upon the earth—to see what advantages have come to us from the knowledge of the metals and minerals, and the adaptation of them to our uses. And yet, of the fifty or more metals now known, only eighteen have so far been employed in the mechanic arts, and not more then a dozen of these until quite recently.

See what the engineer has wrought with the one metal, iron. From the small beginnings of the days of Tubal Cain it has come to be the most abundant and the most valuable to man of all the metals. It enters into nearly every construction, from that of the nail fastening the wooden box to the almost living locomotive, rushing with the speed of the storm-wind along the iron ways with which the engineer has imprisoned the earth as in a net. From it he constructs the larger part of the machinery with which he supplants and surpasses manual labor. He draws it into wire and stretches it around the globe that the ends of the earth may be brought together; and with the same wire he builds a marriage tie between cities which rushing waters strive in vain to separate. Of it he makes our cooking utensils, and our steam boilers; our pens and our printing presses; our keenest cutlery and our conquering cannon. It supplies the tools with which we build, and

itself enters largely into all our most important structural work. Of it the engineer makes the implements with which to cultivate the soil, and the mighty steamships which plow the oceans. This latter is probably true to a fuller extent than we are in the habit of thinking, for it is stated that of the 10,000 steamers in existence, 9,000 are built of iron and steel, the tonnage of which amounts to fully 96 per cent. of the total tonnage of the world.

With the minerals he also has supplied many needs and luxuries for man. There is coal, for example. The 400,000,000 tons, in round numbers, annually mined, give life and warmth and power to probably one-third of the inhabitants of the globe. Blot it out. Let it be as if it had never been. The wheels of commerce and production would stop, almost the pulses of life itself would cease to beat, and man would ere long relapse into savagery, unless he should find some substitute in other provisions of nature. There is some probability that the world's supply of coal will become exhausted in process of time. Before that time doubtless some other means will have been wrested from the secrets of Nature, by which the processes now dependent upon coal will be carried on; but at present it is scarcely too much to say that civilization, if not existence, is dependent upon coal. So, in a less important degree, has the liquid mineral, petroleum, come to be almost the world's source of artificial light. It came to pass that after the torch of the Aborigines, the smoky olive-oil lamp of the Orientals, the rush-light of the North-man, and the tallow candles of our more immediate ancestors had passed into antiquity, and the whaling fleet had nearly exterminated the oil-producing cetacea, that the mechanical engineer found means for burning petroleum, a substance which had long been known but not put to use, and then by ingenious boring implements he penetrated the storehouses in which beneficent Nature had laid it away for just such an emergency. Having devised means for refining it, and packing it securely from leakage, he now sends it to almost every part of the known world for the purpose of giving light to the inhabitants thereof. The extent of this business is simply enormous, if it might not be called incredible. There are no less than one hundred thousand tin cans, of five gallons each, put up and shipped from the United States alone, every day of the year. The production in this country last year was about one thousand million gallons, while the amount shipped over the Trans-Caucasian railway from the Russian wells is over one hundred million gallons more.

But there are other ways in which the mechanical engineer has subdued the earth in response to his divine commission. He has overcome its stoutest obstacles and compelled it to open to him pathways wherever he has desired to go. Rivers, oceans, and mountains have alike yielded to his imperious will. No river is so wide or so rapid but he can bridge it; no mountain so high or so broad but if he wish he can tunnel it. A monument in the city of Turin commemorates the completion of one of the triumphs of modern engineering—the great tunnel through Mt. Cenis, by which a pathway was opened between France and Italy through the Alps. It is a tall, pyramidal structure of dark, rough rocks, reminding one of some inaccessible Alpine pinnacle, on the top of which stands Victory, maimed but triumphant, with pen in hand writing down in endless honor the name of the engineer who had achieved the gigantic task. But, alas, along the pathway up the sides of the pinnacle by which Victory had gained the giddy height, lie the broken, crushed, and bleeding bodies of men who had given their lives to secure the final triumph. It is a sad memento of the cost, but just below them gushes out a perennial stream of pure, limpid water, emblematic of the never-ending beneficence which flows from the completed work. I remember seeing, about a dozen years ago, just as this giant work was finished, a prediction that the cost in life and treasure was so great that no similar work would ever be again undertaken, but in less than a decade the world saw the completion of another and longer tunnel through the same great range of mountains, and one which is, to my mind, a far more wonderful achievement. The approaches to the St. Gothard tunnel are not only unique, but are a stroke of genius, surpassing as an engineering feat the long tunnel itself. I refer particularly to the corkscrew tunnels, the invention of Mr. Helwag, by which the trains climb up the almost perpendicular mountains. Coming from the Italian side, after leaving the pretty little fortified town of Bellinzona, you run along up the ravines until you come to a place where it seems impossible to go farther. Here your train plunges into the face of the mountain, and for a mile and over you travel under ground. If you watch a compass you will notice that you are traveling in a circle, and directly you emerge into daylight you look down, and there, two hundred feet below is the track and tunnel-mouth where you entered. You have but a minute to see this, however, before you again plunge into the mountain, and after a time again emerge, another two

hundred feet higher up the face of the mountain, with the tracks and openings below you to show how far you have climbed. Four times this is repeated before you come to the great tunnel, which itself is over nine miles long, and in descending on the other side you find three more similar spirals. 'Tis said that Napoleon once rode his horse up the spiral incline in the Campanile at Venice, but it was reserved for our time to see a locomotive with a train of cars perform a similar feat every day in the year.

But the engineer has not only penetrated mountains for roadways and bored deep into the earth for treasures, but he has bound the earth in bands of iron and steel, until they have become a net-work upon the surface of the land. No less than 350,000 miles of railroad have already been built, nearly one-half of which is in the United States, and the work is going on in an increasing ratio every year. The waters of the globe are all his pathways; his ships and his steamers plow every ocean, carrying the commerce of continents; and he has almost learned to laugh at the fiercest fury and maddest moods of old Neptune, who no longer rules the sea.

There is also another way wherein the engineer has conquered Mother Earth and compelled her to yield him service, and that is by turning aside water-ways and causing them to irrigate barren places, whereby they have become green and fruitful. The extent to which this has been done in many lands is very great, and the cost of some of the works is almost fabulous. Beginning back in the days of Joseph in Egypt, it has spread over nearly all parts of the habitable globe. We have not been in the habit of looking upon Joseph, the favorite son of Israel, the boy dreamer, the incorruptible slave, the reader of visions, and the prime minister of Egypt, as an engineer also, but late discoveries, coupled with ancient tradition and statements of early historians—which have been looked upon as myths until proven true by such discoveries—compel us to place his name high among the fraternity of which we ourselves are members. There seems now to be little doubt that not only was he the inventor of artificial irrigation, but that he was also the engineer of the most extensive works ever constructed for that purpose. For many years an ancient canal called *Bahr Yoosuf*, or the "River of Joseph," has been known to exist on the west of the Nile, extending some 350 miles in its sinuosities along the river bank, and still supplying water for irrigation of a section called the Fayoum, which otherwise would be cut off from

the benefit of the annual overflowing of the river. There is also a tradition of a great artificial lake, Mœris, to the south of this tract, "so large that it not only modified the climate, tempering the arid winds of the desert, and converting them into balmy airs which nourished the vines into a fullness and a fragrance unknown in any other part of the country, but also added to the food supply of the land such immense quantities of fish that the royal prerogatives of the right of piscary at the great weir was valued at a quarter million dollars annually." Herodotus says of this lake, which was in existence in his time, some 2350 years ago, that it was 450 miles in circumference and some 300 feet deep, and that its most important service was as a grand reservoir for storing for use in times of drought the surplus waters of the Nile. Strabo also says that in his day, about 450 years later, it performed this function so well that when the Nile rose twelve cubits there was plenty in Egypt, and even when it rose eight cubits the dearth was scarcely felt, because the waters of Lake Mœris made up the lack.

Now, modern criticism has relegated all this to the myths and extravagance of early story tellers, but the researches and explorations of an American engineer, Mr. Cope Whitehouse, within the last five years, have demonstrated the truthfulness of the old historians, by locating and surveying the bed of the old lake, which he finds to agree remarkably with their descriptions. Moreover, the old *Bahr Yoosuf* was the feeder of this great artificial lake, through which the surplus waters of the Nile were taken to its basin, and the service which this canal now performs is but an inferior part of the work assigned to it by its illustrious engineer four thousand years ago. Tradition says it was built by the Joseph of Bible story, and that the great lake was finished during the reign of one prince; and what we know of chronology makes this probable. What is more likely than that, during and after the great famine, Joseph designed and carried out this gigantic work, to insure that no such calamity should ever again visit the land? History gives no instance of such a famine since. Plans have been prepared and steps are now being taken to open a new connection between the Nile and the bed of the old lake, and it is not improbable that a few years hence it will again be as it was in the days of the Pharaohs. The restoration of this work, it is estimated, will double the acreage of cultivable land in Egypt.

But probably the most important achievements of the engineer

toward fulfilling his commission lie in enslaving the forces of nature and compelling them to do his will. Doubtless it was early in the history of the race that man found that falling water could turn a wheel and save manual labor. Before that he had depended upon himself or a beast of burden. Now he had put to work a force, which, though he neither understood its nature or name, he could nevertheless compel to do him service. We know this force as "gravitation," and have mastered some of its laws, but to define what it is, and whence it comes, and through what medium it works, we are as much at a loss as was the first man who observed any of its effects. We know that it is the force which holds the worlds in their orbits and guides them in their flight; a force infinite in its outreaching, but not disdaining to do the humbler task of holding each grain of sand and each drop of water to the bosom of their mother Earth, or of bringing them back when they have been enticed away. This force the engineer has harnessed, and not only does he compel it to drive his water wheels and spindles, but he makes it parcel out his commodities of nearly every kind and quantity, and keep his time by the swinging of a pendulum. He calls upon it to lift his boats of hundreds of tons from lock to lock, and sets it in sleepless watch over each individual thread in his looms, that when one shall fail, this powerful force shall ring a bell or stop the machine. Thus in an almost infinite number of tasks, from mighty to minute, he calls it to his aid, and ever finds it a docile and efficient servant.

Another force or quality of matter which the engineer has called to do his bidding is that of momentum or inertia. He finds it riding on the wind and compels it to waft his boat whithersoever he will; he makes it grind his corn, pump his water, and perform many other tasks at little cost. He discovers it sitting on his hammers, and puts it at work driving nails, breaking stones, and riveting the boilers, and forging the enormous shafts required for his great steamships. He sets it to engraving the most delicate figures upon glass, and compels it to batter down the heaviest battlements and pierce the thickest steel armor he can construct. When it exhibits itself in the form of centrifugal force he sets it to skimming milk, drying clothes, purging sugar, pulverizing the hardest rocks, and regulating his powerful engines.

But the force which has done more and more varied work for the engineer is comprehended under the term *heat*. I am aware

that heat is not a force, according to the scientific definition of the term; that speaking in the strictest sense it is only the effect of a form of molecular motion upon animal nerves, but we may be pardoned for using it in this popular way for the want of a more comprehensive word to express the energy which exists in that mode of motion which we call heat. The effect of this "force, agent, or principle," as Webster defines it, to change the condition of matter must have early attracted the attention of man, for he very soon learned to melt, cast and forge metals, and to harden his pottery by its aid, as well as to cook his food. Ethnologists conclude that no race or tribe of men ever existed who were unacquainted with fire, and it is significant that in all the ancient mythologies the god of fire was the one who had charge of the engineering department of the cultus. This would indicate that, even in the earliest times, heat and engineering were closely associated. We cannot, however, find that the development of power by the agency of heat was attempted before the days of Hero, one hundred years prior to the Christian era, unless we credit the statement that the celebrated statue of that great engineer, Amenophis III., which the Greeks mistook for a statue of their god Memnon, was earlier caused to salute the sunrise by an audible sound produced through the heat of the sun's rays expanding air in a closed vessel.

It was, however, reserved for the century immediately preceding the present, to bring into being that greatest step in engineering art, the modern heat engine. It is not necessary before this audience to recapitulate the triumphs of this age in the development of this source of power, or to enumerate the ways in which it serves mankind. Suffice it to say that it is estimated that the steam engines of the world at the present time equal in capacity for work twice that of the total working population of the earth, and that there is scarcely a task which man can perform which is not being done better by steam power. When we consider that probably 80 per cent. of these engines have been built within the last twenty-five years, the mind refuses to contemplate the possibilities in the near future, should the increase continue at the same rate. The subjugation of this power has, moreover, not only tended to save labor to the great extent above indicated, but it has shortened the distance between places, measured in time, from 75 to 90 per cent., and by so much has added to our capacity for doing business, wherein the exchange of commodities

or correspondence forms a feature. We have only to stop and think what would be the result upon the business of to-day if all the steam power, including railway and marine, were blotted out and we had only the facilities which our grandfathers enjoyed less than one hundred years ago, to form some idea, though it must necessarily be an inadequate one, of the benefits we derive from the subjugation of this power to the uses of mankind, and the indebtedness of the world to the mechanical engineers through whom it has been accomplished.

It has been said that fire is a good servant but a bad master, and the enormous quantity of property which has been and is being annually destroyed by it would seem to prove the aphorism. The engineer has subdued it in some respects, and compelled it to do his work, but it is nevertheless true that at times it "takes the bit in its teeth," as horsemen say, and becomes master of the occasion in spite of all that man can do to prevent it. It is in consequence of this fact that $160,000,000 are yearly spent in this country in maintaining insurance companies and fire departments. The experience of the last ten years, however, leads us to hope that it is not always so to be. Through the efforts mainly of two of the members of this society, Mr. Grinnell and Mr. Woodbury, fire has been compelled to set a watch upon itself and act as its own fireman. By the use of what are known as "automatic sprinklers," the heat of the fire sets in action a discharge of water which quickly smothers it out. So efficient is this apparatus that it has reduced the losses from fires where it has been employed 95½ per cent., as is shown by the records for ten years of the Associated Mutual Fire Insurance Companies of New England. It would seem that this is about the last step in the subjugation of this mighty force, and that henceforth it must submit to the dominion of mind.

Yet another great force, or manifestation of force, in nature, is already yielding its neck to the yoke prepared for it by the mechanical engineer, and it will doubtless ere long become one of his most useful servants. In ancient times it was known only as the thunderbolts of Jove, or the recoiling hammer of Thor, the engineer god of our Scandinavian ancestors. Its subjugation to the uses of mankind began but a few years back, comparatively, but it bids fair to be in the not distant future the most docile as well as the most powerful force at the command of man. It had long been known as a terror, and had been played with as a toy, but

our own Franklin, in this very city where we are now assembled, was the first to show that the terror and the toy were one. He not only made the lightning a plaything, but he taught us to divest it of its direst dreads. Other ready hands took up the work of subduing it, and now it rings our call bells, lights our gas, watches sleeplessly in our dwellings for burglars, regulates the heat in our rooms to a fraction of a degree, makes our plated ware and our electrotypes, duplicates our medals, and starts our exhibitions at the nod of a president or a king a thousand miles away. It floods our streets and public halls with light, and furnishes a tiny fire-fly lamp to explore the inner recesses of our bodies. It drives our sewing machines and our street cars, and is even trying its hand at boats and railways. It concentrates into a point the heat equivalent to 500 horse-power of work, and vaporizes metals which we have scarcely been able otherwise to melt. It welds together rods and shafts, as it were, with a touch, and smelts the most refractory ores.

But its most useful and unique service is as a carrier of messages. It takes no note of time or distance, but runs as quickly ten thousand miles as one. When the genius of Shakespeare inspired the brain of the immortal Puck to conceive the boast that he would "put a girdle around the earth in forty minutes," he could not have foreseen the girdles of wire which now surround the globe, and on which even more sprightly elves than Puck himself travel around the globe with the speed of thought. It was two hundred years after Shakespeare wrote "Midsummer Night's Dream" before the first telegraph line was built. That was in the lifetime of many of those who hear me to-night. At the present time no less than 1,700,000 miles of telegraph wire are stretched upon poles throughout the world, and eleven cables lie upon the ocean's bottom connecting continents, not counting short cables between islands, etc. We read at our breakfast tables in the morning the news of the whole world of the events of the evening before, some of it at an earlier hour than it occurs, and business men trade with customers one thousand or three thousand miles away with almost the same facility as with their next door neighbors.

And this is not all. This wonderful sprite will carry our very voices miles away, and whisper our words in the ear of a listening friend, with every intonation and individual peculiarity, so perfect that the hearer imagines that he hears us speak. And to

all intents and purposes it is the same, for he never fails to repeat us with the fidelity of the Chinese artisan who copies all the patches and darns and flyspecks with the same accuracy as he does the more important items of his pattern. So it has come to pass that we cán talk with our friends hundreds of miles away as readily as if they sat by our side, and it is possible for us to listen to an opera or a sermon without leaving our own firesides. And now comes one who has taught the electric genius to write or draw, at almost any distance and at the same instant, the very words and characters we trace with our pen as we sit at our desk, so that our correspondent in Baltimore or Boston sees, as it were, the very motion of our pen before his eyes and recognizes our handwriting.

But I need not enumerate the wonders which mechanical genius and skill have wrought in this, our day, in the line of the first grand commission to "subdue the earth." You see them all about you. You bask in their beneficence. Life to you would no longer be life were you deprived of the advantages you enjoy through their existence.

The question only remains, What will be the outcome? To what shall we look forward as the field for future achievement? Can we go on at the same rate of advancement, or must we perforce stop, as did Alexander, for want of other worlds to conquer? Towards the end of the last century—after Priestley had discovered oxygen; Galvani had found the supposed secret of life; the Leyden jar had shocked crowned heads as well as philosophers; Watt had brought steam into practical use; Arkwright had invented the spinning frame and Cartwright the power loom; Murdock had begun lighting buildings by gas; Cort had invented the puddling furnace and the rolling mill, and Tennant had produced chloride of lime and applied it to bleaching purposes—a prominent scientist said that the end of discovery must be about reached, and that there could be no probability that the next age would advance as rapidly as that had done! But discovery has followed discovery in a geometrically progressing ratio, until at the present time we stand aghast, and say, "What can come next?" It is, however, probable that the future will bring many a discovery and application of knowledge, as far beyond anything we can now imagine as sewing machines and mowing machines, railroads and steamships, celluloid and dynamite, rock drills and rotary printing presses, vulcanized india-rubber and Bessemer steel, boring

for oil and burning water gas, photography and the phonograph, electric light and electric smelting, the telegraph and the telephone, the spectroscope and hundreds of other discoveries and inventions of the nineteenth century, were beyond the thoughts of our grandfathers one hundred years ago.

But though we cannot foretell what wonderful things may be in store for us in the future, we may at least speculate a little, based upon the knowledge we now have. I have spoken of the probable exhaustion of our coal fields, and the necessity of finding some substitute. There is a source of heat fully capable of supplying all we need, if it can be utilized ; that is the sun. It has come to be generally admitted that coal is, in a sense, nothing but condensed sunlight, stored up in ages past for our present use. Now if we can only devise means for imprisoning the heat of the sun, so as to use it as we may need, then we can abandon our coal mines and forget that they ever existed. The amount of this heat, in one minute, as determined by the United States expedition to Mount Whitney, is sufficient to heat one gram of water three degrees centigrade for each square centimeter of the exposed surface of the earth. But this conveys little idea to the mind not educated to think in such measures. It may be better understood if we say that it is equivalent to the heat generated by the combustion of, in round numbers, one thousand millions of tons of coal for every minute in the year; or one million three hundred thousand times as much as is now mined. Surely this is sufficient for all the wants of mankind, and already an honored member of our Society, Captain Ericsson, has shown us how a portion of it can be conserved. But the question may be asked, what would be the result upon the condition of things should this heat, which now goes to warm the earth and promote vegetation, be used for other purposes? To this it may be answered that as the earth does not grow warmer, year by year, all the heat it receives must be either changed to other forms of energy, or be radiated into space. It is likely, therefore, that the heat radiated from the earth is nearly as great as that received, and possibly some way may be discovered to catch it on its outward journey, if it be found that the amount necessary for our purpose would rob nature of her share. But as the burning of four hundred million tons of coal, and probably as much more wood, upon the earth per annum, does not seem to perceptibly alter the character of our seasons, it is probable that a similar amount taken from

the heat received from the sun would not produce a noticeable effect upon the processes of nature.

But we must learn to conserve our heat better, and instead of the present wasteful mode of making power by means of steam, by which only one-tenth of the heat generated is utilized, some better means must be discovered of turning heat into work. The generation of electricity directly by the combustion of coal has often been suggested as a possible thing, and one of our members, Mr. Edison, is even now working with some success upon the problem. Should some future genius succeed in utilizing the heat as fully in a thermo-electric generator as is now done in our steam boilers, the coming age will get its power at one-fifth the cost of the most economical engines of this day, and then if the radiant heat of the sun can be caused to take the form of electrical force, factories, railroads, and steamships will be driven and lighted with no expense beyond the interest and wear and tear of the machinery.

It is not, however, necessary for us to wait for the discovery of this efficient thermo-electric generator before we can conserve our fuel. We now have the means by which the water power which is going to waste can be transmitted to localities where more power is demanded. Electric dynamos have attained to as high as 90 per cent. efficiency, and if they can be made perfectly reversible, which at least seems probable, we should be able to transmit power with comparatively small loss. The water power of the world is estimated at 200,000,000 horse-power, and when we have conserved this we shall have enough to supply the world's work for years to come; then there are the enormous energies of the tides, and of the winds, which might be added thereto, when we have perfected storage batteries, so that the extremes of effort of such forces may be graded down into a constant mean.

In looking about for fields to conquer, the question springs to the mind, Shall we ever fly? That has been one of the dreams of mankind from the earliest ages. He has looked upon the birds as they sailed through the air, apparently without effort, and has wondered why he could not attain to the same mode of motion. Why not? It is simply a question of sufficient power within a given weight. The weight of our steam engines per horse power has been decreased 37 per cent. in the last five years, and we are not yet at the end. We are also reducing very rapidly the weight of our dynamos and electric engines in proportion to their power,

so that it is now but one-quarter of what it was five years ago. When man has learned to generate power with say one-tenth the weight of apparatus that is now required, then by making the framework of his flying machine of aluminium tubes, there is no good reason to doubt that he will become master of the air, as he is now master of the water, and will be able to fly from place to place with all the certainty and safety of a bird.

But I will not detain you by further speculations as to the future triumphs of our profession. That they will be great, does not admit of a doubt, in view of the past. That they will be more wonderful and important than we can now comprehend, is probable from the analogy of history. That they will elevate and ennoble man, lift him out of many of his present limitations, and make him the master where now he is the victim, is certain, because that has always been the effect heretofore, and because it is the end for which the engineer is commissioned. This Society is to be a factor in such result. Its every meeting to mingle experiences, to discuss causes, to compare attainments, and to encourage research, is a step toward the final achievement when every force in nature and every created thing shall be subject to the control of man.

CCLXIII.

NOTES ON RESULTS OBTAINED FROM STEEL TESTED SHORTLY AFTER ROLLING.

BY EDGAR C. FELTON, STEELTON, PA.

(Member of the Society.)

Two years or more ago some interesting facts were noticed in connection with certain tests then being made by the Pennsylvania Steel Co. on structural steels for a well-known bridge across the Ohio River. These facts were as follows: The inspector employed by the Bridge Company for whom the work was being done, being in a great hurry to leave our works, had tested several heats on the same day that the test bars had been rolled, and had rejected each heat so tested; the cause of rejection in each case being a low percentage of reduction of area. Several days later, pieces cut from the same bars were tested and found to fill the requirements of the same specifications perfectly, the percentage of reduction of area in the last test pieces broken being much higher than in those broken on the same day that the bar was rolled. Here was evidently something worth investigating. Knowing the very misleading conclusions which can be obtained by reasoning from an insufficient number of experiments where the conditions are as complicated and variable as in the making of a steel bar, it seemed best to get as large an accumulation of data as possible together, and then endeavor to read their story and deduce a theory which would explain what they told. Accordingly, at such times as it has been possible during the past two years, experiments have been carried on by testing bars of steel immediately after rolling and again after several days have elapsed, and the data so accumulated are now thought to be sufficient to arrange and discuss intelligently.

The steel tested was all of a structural quality and made by the open hearth process, the conditions of working being kept uniform as nearly as possible. After melting, the steel almost without exception passed through the following stages of manufacture: an ingot 14 inches square and weighing about 2,850 lbs. was poured, reheated and rolled into a bloom 7 inches square. This 7-inch square

bloom was next reheated and either hammered or rolled into a 4-inch square billet. The billet was reheated and in the case of the "guide" rounds was rolled at once into a $\frac{3}{4}$ inch round bar; in the case of the "hand" rounds, was broken down to about $1\frac{3}{4}$ inches square, reheated and rolled into a $\frac{3}{4}$ inch round bar. For the better understanding of those unfamiliar with the practical work of the rolling mill, I will explain the difference between the "hand" and "guide" round. The hand round is made by rolling the billet into a bar of rectangular section, the shortest side of the rectangle being a trifle longer than the diameter of the round into which the bar is to be rolled. This piece of rectangular section is formed by several grooves in the rolls into a round slightly larger than the finished bar is to be, and this round is then passed through the finishing groove of the rolls several times, the bar being rotated after each pass through about 90°. A new portion of its surface is thus brought in contact with the rolls in each succeeding pass. The reductions at each pass in this method of rolling are very small and the heat at which the work is done is very low. The guide round, on the other hand, is rolled as follows: a bar of rectangular shape is first made and then rolled into a bar of elliptical section, the conjugate axis of the ellipse being slightly less than the diameter of the round which is to be made from it. This elliptical bar is next thrust between guides which are made to fit it closely and hold it firmly with its transverse axis in a vertical position, into the finishing groove of the rolls which forms it at one pass into a round bar. Almost the entire surface of the bar is in contact with the rolls in this finishing groove, and the amount of reduction is very much greater than is the case with the hand round; the heat at which the work is done is much higher also. The principal differences in the methods of rolling the two are these: that the hand round is finished at a much lower temperature by passing through the finishing groove in the rolls a considerable number of times with very slight reductions each time. The guide round, on the other hand, is finished at a much higher temperature by being squeezed at one pass from an ellipse into a round, the amount of work done in this pass being considerable. It will thus be seen at a glance by any one familiar with the working of steel that the work done in finishing the hand round takes effect on the surface principally, while that done in finishing the guide round affects the whole area of the bar. The work done on the hand round is approximately the same as that done on the universal plate mill, now so largely

used in bridge and girder construction, while that done on the guide round is very much like that to which an angle bar is subjected in rolling. It was thought well to go thus particularly into the difference between the two methods of rolling the test bars, since a considerable difference was found in the results obtained from hand and guide rounds.

In the following tables the results obtained from hand and guide rounds are grouped separately. In both tables the averages of results obtained from pieces tested within twenty-four hours of the time of rolling are compared with the averages of results obtained from pieces cut from the same bars but tested more than twenty-four hours after rolling. The tables are further subdivided into groups according to the ultimate strength of the heats included in them. The data contained in the tables were obtained by breaking 446 test pieces from 102 separate open hearth heats. The tests were all made on one testing machine running at a constant rate of speed, and all conditions were made as uniform as possible. The elastic limit was in all cases determined by the dropping of the beam of the machine, and the percentage of elongation was taken on an original length of 8 inches.

TABLE I.

RESULTS FROM HAND ROUNDS.

Reduction of area. Per cent.	Elongation in 8″. Per cent.	Elastic limit, lbs. per sq. in.	Ultimate strength, lbs. per sq. inch.	
Group 1. *Averages from* 5 *heats of* 60,000 *to* 65,000 *lbs. ultimate strength.*				
50.09	25.79	45,689	61,821	Av. of 10 tests made within 24 h. of rolling.
53.93	26.90	44,482	62,206	Av. of 5 tests made 24 h. or more after rolling.
+ 2.14	+ 1.11	− 1,207	+ 385	Increase or decrease.
Group 2. *Averages from* 5 *heats of* 65,000 *to* 70,000 *lbs. ultimate strength.*				
51.86	25.87	48,277	67,897	Av. of 32 tests made within 24 h. of rolling.
52.23	25.62	47,806	67,717	Av. of 12 tests made 24 h. or more after rolling.
+ 2.07	− .25	− 471	− 180	Increase or decrease.
Group 3. *Averages from* 6 *heats of* 70,000 *to* 75,000 *lbs. ultimate strength.*				
38.17	22.61	47,946	72,053	Av. of 21 tests made within 24 h. of rolling.
41.12	23.27	48,248	72,250	Av. of 20 tests made 24 h. or more after rolling.
+ 2.95	+ .66	+ 302	+ 197	Increase or decrease.
Group 4. *Averages of* 3 *heats of* 75,000 *to* 80,000 *lbs. ultimate strength.*				
39.90	22.55	52,998	77,380	Av. of 10 tests made within 24 h. of rolling.
46.66	23.81	52,189	77,487	Av. of 8 tests made 24 h. or more after rolling.
+ 6.76	+ 1.06	− 809	+ 107	Increase or decrease.

Group 5. Averages of 1 heat of 80,000 to 85,000 lbs. ultimate strength.

Reduction of area. Per cent.	Elongation in 8″. Per cent.	Elastic limit, lbs. per sq. in.	Ultimate strength, lbs. per sq. inch.	
49.82	23.21	57,020	83,821	Av. of 7 tests made within 24 h. of rolling.
50.26	23.50	57,233	83,857	Av. of 3 tests made 24 h. or more after rolling.
+ .44	+ 29	+ 213	+ 36	Increase or decrease.
				General averages of 20 heats.
45.97	23.96	50,386	72,544	Gen. av. of tests made within 24 h. of rolling.
48.84	24.52	49,992	72,703	Gen. av. of tests made 24 h. or more after rolling.
+ 2.87	+ .56	− 394	+ 109	Increase or decrease.

TABLE II.

RESULTS FROM GUIDE ROUNDS.

Reduction of area. Per cent.	Elongation in 8″. Per cent.	Elastic limit in lbs. per sq. in.	Ultimate strength in lbs. per sq. inch.	
Group 1. Averages of 3 heats of 55,000 to 60,000 lbs. ultimate strength.				
58.13	28.27	39,116	58,193	Av. of 6 tests made within 24 h. of rolling.
57.17	28.92	38,397	58,630	Av. of 10 tests made 24 h. or more after rolling.
+ .96	+ .65	+ 719	+ 437	Increase or decrease.
Group 2. Averages of 6 heats of 60,000 to 65,000 lbs. ultimate strength.				
49.94	26.28	41,985	63,674	Av. of 10 tests made within 24 h. of rolling.
51.07	26.93	41,532	64,270	Av. of 22 tests made 24 h. or more after rolling.
+ 1.13	+ .73	− 453	+ 596	Increase or decrease.
Group 3. Averages of 18 heats of 65,000 to 70,000 lbs. ultimate strength.				
50.65	25.70	44,267	67,660	Av. of 22 tests made within 24 h. of rolling.
52.10	26.03	44,097	68,042	Av. of 36 tests made 24 h. or more after rolling.
+ 1.45	+ .33	− 170	+ 382	Increase or decrease.
Group 4. Averages of 15 heats of 70,000 to 75,000 lbs. ultimate strength.				
47.80	24.41	46,498	71,630	Av. of 24 tests made within 24 h. of rolling.
48.94	24.85	46,332	72,268	Av. of 36 tests made 24 h. or more after rolling.
+ 1.14	+ .44	− 166	+ 638	Increase or decrease.
Group 5. Averages of 18 heats of 75,000 to 80,000 lbs. ultimate strength.				
42.22	23.01	50,045	77,767	Av. of 35 tests made within 24 h. of rolling.
44 55	22.20	49,731	77,968	Av. of 47 tests made 24 h. or more after rolling.
+ 2.33	− .81	− 314	+ 201	Increase or decrease.
Group 6. Averages of 14 heats of 80,000 to 85,000 lbs. ultimate strength.				
40.64	20.79	51,322	81,385	Av. of 16 tests made within 24 h. of rolling.
41.88	21.21	51,157	82,152	Av. of 30 tests made 24 h. or more after rolling.
+ 1.24	+ .42	− 165	+ 767	Increase or decrease.
Group 7. Averages of 8 heats of 85,000 to 90,000 lbs. ultimate strength.				
36.87	19.41	54,145	86,787	Av. of 8 tests made within 24 h. of rolling.
37.49	19.87	54,237	87,312	Av. of 16 tests made 24 h. or more after rolling.
+ .62	+ .46	+ 92	+ 525	Increase or decrease.
				General averages of 82 heats.
46.61	23.97	46,768	72,442	Gen. av. of tests made within 24 h. of rolling.
47.60	24.29	46,498	72,949	Gen. av. of tests made 24 h. or more after rolling.
+ .99	+ .32	− 270	+ 507	Increase or decrease.

From a study of the averages contained in these tables it will be seen that, with some few exceptions,* the tests made twenty-four hours after rolling, when compared with those made within twenty-four hours of rolling, show an increase in the percentage of reduction of area, an increase in the percentage of elongation, a decrease in the elastic limit, and an increase in the ultimate strength. The percentage of change is as follows:

	Increase in reduction of area.	Increase in elongation.	Decrease in elastic limit.	Increase in ultimate strength.
Hand Rounds	6.2%	2.4%	0.8%	0.15%
Guide Rounds	2.1%	1.4%	0.6%	0.07%

These changes occur so uniformly throughout the different groups in which the results are arranged that it seems very probable that this "seasoning" of steel, as it may be called for want of a better term to describe it, always takes place after rolling, although its effects may be so modified in particular cases as not to be noticed. To find an entirely satisfactory explanation of these changes is a difficult matter.

A first glance at the results recorded above would seem to indicate that they are to be explained by and perhaps help to prove the theory of a gradual change after rolling in the form of the carbon contained in the metal. This theory maintains that whatever graphitic carbon is included in steel at the time it is finished in the rolls is continually and slowly passing from the graphitic state, in which it is disseminated in fine particles through the metal, into the combined state, in which it either forms a definite chemical compound with the iron or else is united with it in the same way that metals unite to form an alloy. If this change of graphitic to combined carbon does take place we should expect it to increase the ductility of the metal, that is to increase its percentage of reduction of area and its percentage of elongation. Exactly these changes are seen to take place, in the tests recorded in the tables. But the same change from graphitic to combined carbon does not satisfactorily explain the lowering of the elastic limit, although it may explain the increase of the ultimate strength, both of which are also characteristic of the "seasoning" of steel. Nor does the change of carbon theory explain why the hand rounds

* These exceptions are: The percentage of reduction of area in Table II., Group 1; the percentages of elongation in Table I., Group 2, and Table II., Group 5; the elastic limits in Table I., Groups 3 and 5, and Table II., Group 7; and the ultimate strength in Table 1, Group 2.

show the effects of "seasoning" to a so much greater extent than the guide rounds do. In fact, it cannot be made to give a satisfactory solution of the changes noticed above in the physical characteristics of steel. An entirely satisfactory explanation of these changes I have not been able to find, but it seems to me that it is to be looked for in a modification of the relations of the molecules of the metal brought about by the mechanical treatment to which it is subjected in rolling, rather than in a chemical change such as the graphitic and combined carbon theory calls for.

It is certain beyond any question that during the process of rolling the molecules of the bar are forced very violently over one another, although this is done while the interior of the bar is at a temperature high enough to admit of the molecules moving over one another with tolerable freedom and without weakening to any great extent the power which holds them together. The treatment to which the molecules of the surface or "skin" of the bar are subjected is different in several important particulars from that which the molecules in the interior portion of the bar receive. The former are at a much lower temperature than the latter, being cooled by the surrounding air, and while the work of reduction is put on them, are in contact with the cold mass of the roll. There is, consequently, a much greater resistance to movement over one another among them than among the molecules of the interior. At the same time they are subjected to the powerful wiping action of the surface of the rolls. When it is considered that each point of the surface of the roll in contact with a round bar is traveling at a different rate of speed from its neighbors, owing to the change in the diameter of the roll at each point of contact, it will readily be seen what a complication of movements the molecules are trying to execute at the same time that they are being compressed by the squeeze of the roll upon them. The result of this wiping action of the roll upon the comparatively cool molecules of the surface of the bar must be a very violent and forcible movement among them, and a consequent partial destruction of the power which holds them together. Other things being equal, the cooler the surface is and the more times the wiping operation is repeated, the more loss of cohesion among the molecules we should expect to find. Consequently the hand rounds, which are subjected to the wiping action of the rolls oftener and at a lower temperature than the guide rounds, should show the effects of "seasoning" in an exaggerated degree. Furthermore, our theory must now assume that

the attraction between the molecules, which has been rudely disturbed and weakened by the action of rolling, gradually grows stronger as time elapses. In other words, the molecules must be supposed to regain their lost grip upon each other after they are allowed to rest.

Now, an increase in the attraction which holds the molecules of a bar of steel together means an increase in the ductility, elasticity and strength of the bar. Going back to the tables of tests, we find that the tests made twenty-four hours and more after rolling, when compared with the tests made within twenty-four hours of rolling, show an increase in the percentage of reduction of area and percentage of elongation; that is, an increase in ductility; a lowering of the elastic limit, that is, an increase in elasticity; and an increase in the ultimate strength, that is, an increase in strength. Furthermore, comparing the results obtained from the hand rounds with those obtained from the guide rounds, we find that the hand rounds which have been subjected to the wiping action of the rolls a greater number of times, and at a lower temperature than the guide rounds, have gained more in ductility and elasticity, but not in strength, by their rest of twenty-four hours. The percentage of gain in the two is as follows:

	Increase in reduction of area.	Increase in elongation.	Decrease in elastic limit.	Increase in ultimate strength.
Hand Rounds	6.2%	2.4%	0.8%	0.15%
Guide Rounds	2.1%	1.4%	0.6%	0.7 %

Excepting the case of the ultimate strength, this is what we should expect, since the rougher the treatment is to which the molecules are subjected the more their power of holding together is lessened, and the greater their gain would be on being allowed to rest and renew their lost hold on each other. The exception to the general rule, shown by the very slight gain in ultimate strength, by the hand rounds, I am unable to explain. Whether the average of a greater number of tests would show a greater gain in ultimate strength, or would further confirm the exception, can only be told by making further experiments. It is certainly true that the exception, as it stands, weakens the testimony of the other results toward establishing the theory.

To sum up: The tests made upon the three-quarter inch round bars seem to prove quite conclusively that after rolling steel increases in percentage of reduction of area, in percentage of elongation and in ultimate strength, and decreases in elastic limit. I

have endeavored to account for these changes by supposing that the rough mechanical treatment of the metal by the rolls has temporarily weakened the hold of its molecules upon each other, and that this hold is regained when the metal is allowed to rest.

DISCUSSION.

Mr. Jacob Reese.—Mr. Felton has called our attention to a very important matter. Steel is now wrought in so many structural forms, and used in so many places where the lives of persons are in danger, that it becomes our duty as engineers to search for the cause of every phenomenon exhibited in its manufacture or its use. And in this matter now under consideration, I think Mr. Felton has pointed in the right direction when he says the phenomena in question were in some manner due to the movement of the molecules composing the metal.

Steel is a compound of iron and carbon, with more or less manganese, silicon, phosphorus and other objectionable matter; the iron and other elementary bodies *per se* are inert; their energy is derived from the imponderable physical forces by which they are endowed; by virtue of these forces the atoms form molecules, and the molecules assume a physical structure, in which neither the atoms in the molecule nor the molecules in the physical structure touch each other at any time, nor are they at any time in a state of absolute rest. When we rupture a bar of steel by tensile strain, we do not rupture the iron *per se*, but rather rupture the molecular resultant force which holds the molecules together in the section under strain.

The physical force that draws the molecules together is inherent in the molecules, hence its effective power diminishes with the distance of its object.

When the molecules are most close to each other they are most quiet, and when farthest apart they are the most active.

Temperature is the measure of molecular activity as weight is the measure of matter; hence an increase of temperature indicates an increased molecular activity, an increased distance between the molecules, and a corresponding reduction of the molecular resultant force.

Assuming that molecules possess a long axis at their equators and a shorter axis at their electric poles, in the act of rolling, the movements of the molecules will be in line of their equatorial

axes; and as the bite of the rolls on a bar of steel is different at every point, there will be produced a differential movement of the molecules, and when the bar is finished the molecules will be left in an abnormal attitude to each other.

Now there are two distinct causes operating on the bar: 1st. As the temperature lowers, the activity of the molecules diminishes; they assume a closer attitude to each other, and the molecular resultant force increases, which would exhibit an increased tensile and compressive ability and a reduction of ductility. 2d. The molecules are in an abnormal attitude, and must be brought to their normal relation to each other before the bar will assume its *most* healthy tone; this can only be done by annealing, that is, by holding the bar for a given time at the same or a slightly higher temperature than that at which it was rolled, thus giving the molecules time and ability to adjust themselves to their normal healthy attitude to each other.

The phenomena exhibited in the tables of Mr. Felton's paper are due to a cold flow of the molecules while adjusting themselves *towards* their normal relation. Now, assuming that the normal relation of the molecules is when their electric or short axes are parallel with the length of the bar; in the act of rolling the molecules are drawn into a right angle to their normal relation to each other, with their long axes parallel with the length of the bar, and their short electric axes across the bar. When the rolling ceases the molecules commence to adjust themselves towards their normal relation to each other, but as their activity is less than when they were drawn into the abnormal relation, they are unable to resume their normal relation, but this movement *towards* their normal relation goes on even after the temperature has fallen below sixty degrees Fahrenheit.

When this cold flow takes place, the short axes take the place of the long axes (differentially), and it consequently increases the distance between the molecules (on that line), increases their ductility, elongation and reduction of area; decreases the elastic limit, and if the flow had been perfect as in annealing, there would also be a corresponding reduction in ultimate strength.

In hand rolling, the work is done at a lower temperature and with a greater number of passes, hence the molecules are left in a worse condition than in the guide rounds. In the act of annealing there is always a greater change in hand rounds than in guide rounds.

I think that molecular physics will explain many of the wonderful, mysterious phenomena which bother the engineers of the present day.

Mr. Wm. Hewitt.—Mr. Felton, in explanation of the greater gain in elasticity and ductility of the hand rounds, remarks, "that it is natural to expect that the rougher the treatment is to which the molecules are subjected, the more their power of holding together is lessened, and the greater their gain would be on being allowed to rest and renew their lost hold on each other." It is not clear to me that the material which undergoes the least reduction in the finishing pass, even though subjected to this pass a number of times at a low temperature, receives a rougher treatment than that which undergoes a much heavier reduction in one pass at a higher temperature. I should suppose it would make a considerable difference at what temperature the metal left the rolls, but without more definite information on this point I do not see how any very satisfactory conclusions can be drawn. I know in rolling rods we always endeavor to finish them as hot as possible. Very much, no doubt, also depends on the molecular structure of the bars when they left the rolls, and the temperature at which they are finished might have considerable influence on this. Some very interesting facts on this point will be found in the discussion of the paper on "The Injurious Effect of a Blue Heat on Steel and Iron," by Mr. Strohmeyer, which I called attention to in the discussion of the paper by Mr. Coffin. This discussion, which was rather a remarkable one in many respects, elicited considerable information in regard to the effect of working steel at various temperatures, and some apparently conflicting statements. The general conclusion which I drew from it, however, was that if steel was worked at a low temperature, say a dull red or a blue heat, while the carbon or any portion of it was in that state described in the paper on "Steel Car Axles" as hardening carbon—the state which it assumes when heated to a high temperature and when it possesses the property of imparting a temper to the metal if suddenly cooled—the effect is injurious and the bar or metal is permanently weakened. On the other hand, if the carbon exists in the non-hardening state and the structure of the metal is free from crystallization, the effect of working at a blue heat is imilar to that of cold rolling; the strength and elasticity of the material are considerably elevated at a slight sacrifice of ductility. I call attention to this paper with the idea that it may sug-

gest an explanation of the difference in the effect of hand rolling and rolling in guides. In reading the remarks of Mr. Anderson on the paper to which I have referred, it appears that it does not always follow that the rougher the treatment that the metal receives the less will be the cohesive force between the molecules. Such treatment may often assist in breaking up crystallization, as instanced by the experiments of Mr. Chernoff alluded to by Mr. Anderson. A specimen of boiler plate which was heated to a bright red heat and allowed to cool very slowly exhibited a crystalline structure, and instead of being benefited by this careful annealing proved to have received a positive injury, whereas a similar specimen heated to the same temperature and then hammered lightly and rapidly till the temperature had fallen to a blue heat was sound and good.

As to the gradual recuperative properties of bars after rolling, the experiments are very interesting and instructive; the phenomenon is probably one due to the gradual subsiding of the molecules, which were no doubt in a high state of agitation immediately after the bars left the rolls. The flow of metals is a subject which appears to have received but slight attention at the hands of engineers, and the query arises, does this flow ever entirely cease?

I have noticed that bars which have been thrown aside as defective, and have lain in the yard a long time, so long indeed that they have become very rusty, improve in quality with age, and the older and rustier they get the better they seem to be. Some may be inclined to accept this statement with a good deal of allowance as a fact existing to a large extent in the imagination, but I am very glad to note that the statement is corroborated by the observations of such high authority as Prof. Thurston. (*Materials of Engineering*, Vol. 2, page 576.)

I would like to ask Mr. Felton what advantage there is in finishing from a groove of elliptical section? In rolling rods we always finish from what we term an oval; that is, the shape formed by the intersection of two circles of equal diameters.

Mr. Felton.—A few words in answer to Mr. Hewitt. He remarks that the explanation given of the greater gain of the hand rounds in elasticity and ductility seems insufficient, since it is not clear that the molecules of the hand rounds have received rougher treatment in rolling than those of the guide rounds. In describing the difference between the methods of rolling the hand and

guide rounds, I should have explained more fully my reasons for supposing that the molecules of the hand rounds were more roughly treated than those of the guide rounds. It is true, as Mr. Hewitt says, that the guide round receives a much heavier reduction in the finishing pass than the hand round receives in the several passes which are required to finish it. But it does not follow that the molecules of the guide round are therefore treated as roughly as those of the hand round. The guide round receives such a heavy reduction at so high a temperature in the finishing pass that there is a uniform and easy flow throughout its entire mass. The molecules at its center are moved and drawn over each other in very much the same manner as those nearer its surface, and the bar may be spoken of as nearly homogeneous, looked at from the standpoint of the relations of its molecules to each other. The hand round, on the contrary, receives many slight reductions at a low heat,* and consequently the flow of its molecules is principally in the neighborhood of its surface. The molecules at its center tend to remain stationary, and those at and near its surface tend to move in the direction of the length of the bar. The molecules in one part of the bar are consequently under a different strain from those in another part of the bar, and the bar may be spoken of as unhomogeneous as far as the relations of its molecules to each other are concerned. It is thus clear, I think, that the force which holds the molecules together has been more severely strained in the hand rounds than in the guide rounds, and we may speak of the molecules of the former as having been more roughly treated than those of the latter.

Mr. Hewitt further says that the difference in the results obtained from the hand and the guide rounds may perhaps be explained by the facts brought out in the discussion of Mr. Strohmeyer's paper on the "Effects of Working Steel at Blue Heat." The effect of the different temperatures of finishing in the two kinds of rounds is undoubtedly very great, as is explained above. But as the steel in the case of both kinds of rounds was heated to a working temperature—the amorphous structure of Chernoff—and then rolled, the finishing heat in no case approaching anything like a blue heat, it is plain that no difference in the state of the carbon in the two kinds of rounds is possible, and that consequently we

* That the hand rounds were finished at a lower heat than the guide rounds is conclusively proved by the higher elastic limit shown by them for a given ultimate strength.

cannot explain the differences in the results of the tests in the manner suggested by Mr. Hewitt.

In answer to the question as to why the guide rounds were formed from a bar of elliptical section rather than from an oval, I would say that experience has taught us that a bar of this shape fills out the round more perfectly than an oval. I think Mr. Hewitt's experience has been with much smaller rounds than were used in my experiments. It is the usual practice, I know, in rolling wire rods to form the round from an oval such as is spoken of by Mr. Hewitt. In making larger rounds the uniform practice is, I think, to roll from a bar of elliptical shape.

CCLXIV.

EXPERIMENTS AND EXPERIENCES WITH BLOWERS.

BY HENRY I. SNELL, PHILADELPHIA, PA.

(Member of the Society.)

Running a fan blower to the best advantage is a problem not generally solved, and one that is often cast aside as being too small a matter to require the attention of the mechanical engineer of the establishment, who thinks his genius and acquirements can be better employed in determining the relative merits of "cut offs" or "combustion of fuel." There is a greater distinction in being able to talk learnedly, and calculate on economy which will produce a horse-power with a consumption of less than two pounds of coal per hour, than there is in being able to select and put in operation a blower with at least one-half the power which one-half the blowers are using in this country to-day.

In this paper it is merely proposed to open the way for the discussion of this neglected subject, and to do something toward removing the prejudice and ignorance concerning it, by giving the Society some of the experiences which have been met, and in making some deductions from them and suggestions regarding them. The object of this paper is not to give rules and ideas in relation to the best construction or proportion of blowers; but data of power required, pressure obtained, and quantities of air delivered which have been attained, and should be expected from commercial blowers already in the market in general use.

Fan-blower calculations can be made which will be realized in practice with as much certainty as any other engineering calculation, but in making them all the elements of the problem must be considered and no guesswork allowed. With a good groundwork to start from, comparison and association of ideas and experiences are much to be preferred to the unintelligent use of elaborate formulæ, especially when the formulæ contain, as they often do, constants dependent in their values upon variable thermometrical, barometrical, hygrometrical, or any other metrical condition. In earlier days it was thought necessary to carry all calculations out to the seventh

or seventeenth decimal to be sure of getting just the correct answer. In some of my old note-books the value of π was carried out to 126 decimals; now I am generally content to call it $3\frac{1}{7}$. These are the faults, if they are faults, of youth and a too liberal education, and are generally corrected as old age and experience creep over us. When letters similar to this are to be answered: "How much power will it take to run a fan to deliver 11,386 cubic feet of air per minute under a pressure of 1.156 oz. per sq. inch?" inclinations prompt me to reply "Not much," but courtesy and a desire to make a sale let them down easy.

I propose first to give some tables relating to the velocity of air under pressure, then some experiments upon blowers under various conditions, and finally to discuss the results obtained.

For the purposes of discussion farther on the following tables have been calculated, giving velocities of air discharging through an aperture of any size under the given pressures into the atmosphere. The volume discharged can be obtained by multiplying the area of discharge opening by the velocity, and this product by the coefficient of contraction: .65 for a thin plate and .93 when the orifice is a conical tube, with a convergence of about 3.5 degrees as determined by the experiments of Weisbach.

Table No. 1 gives velocity in feet per minute, for very light pressures, varying from .01 to .10 oz. per sq. inch.

Table No. 2 gives velocity for pressures varying by 16ths, from one 16th up to one inch, and by 8ths up to 2 inches water pressure.

Table No. 3 gives velocities for higher pressures, varying by $\frac{1}{4}$ oz. up to 1 lb. per sq. inch, which may be considered the practical limit to be attained with a fan blower.

As these tables will be absolutely true for only one condition of things, it may be stated that they are calculated at a barometrical pressure of 14.69 lbs. equals 235 oz., and for a temperature of 50 degrees Fahr. from the formula $V=\sqrt{2gh}$.

Allowances have been made for the effect of the compression of the air, but none for the heating effect due to the compression.

At a temperature of 50 degrees, a cubic foot of air weighs .078 lbs., and calling $g=32.1602$, the above formula may be reduced to

$$V_1=60\sqrt{31{,}5812\times(235-P)\times P},$$

where V_1 velocity in feet per minute.

P=pressure above atmosphere, or the pressure shown by gauge, in oz. per sq. inch.

TABLE NO. 1.

Pressure in ounces per sq. inch.	Velocity in feet per minute.
.01	516.90
.02	722.64
.03	895.26
.04	1033.86
.05	1155.90
.06	1266.24
.07	1367.76
.08	1462.20
.09	1550.70
.10	1635.00

TABLE NO. 2.

Pressure per sq. inch in inches of water.	Corresponding pressure in ozs. per square inch.	Velocity due the pressure in feet per minute.
$\frac{1}{32}$	.01817	696.78
$\frac{1}{16}$	.03634	987.66
$\frac{1}{8}$	.07268	1393.75
$\frac{3}{16}$	.10902	1707.00
$\frac{1}{4}$	.14536	1971.30
$\frac{5}{16}$	.18170	2204.16
$\frac{3}{8}$	.21804	2414.70
$\frac{7}{16}$	.25438	2608.41
$\frac{1}{2}$	.29072	2788.74
$\frac{9}{16}$	.32706	2958.12
$\frac{5}{8}$	.36340	3118.38
$\frac{11}{16}$	.39974	3270.84
$\frac{3}{4}$	.43608	3416.64
$\frac{13}{16}$	.47240	3556.56
$\frac{7}{8}$	.50870	3690.62
$\frac{15}{16}$	.54500	3820.33
1	.58140	3946.17
$1\frac{1}{8}$	.6541	4186.25
$1\frac{1}{4}$	.7267	4362.62
$1\frac{3}{8}$	.8000	4671.10
$1\frac{1}{2}$	.8721	4836.06
$1\frac{5}{8}$	.9448	5034.32
$1\frac{3}{4}$	1.0174	5224.98
$1\frac{7}{8}$	1.0901	5409.26
2	1.1628	5587.58

TABLE NO. 3.

Pressure in oz. per sq. inch.	Velocity in feet per minute.	Pressure in oz. per sq. inch.	Velocity in feet per minute.
.25	2582	8.25	15100
.50	3658	8.50	15334
.75	4482	8.75	15566
1.00	5178	9.00	15795
1.25	5792	9.25	16021
1.50	6349	9.50	16224
1.75	6861	9.75	16465
2.00	7338	10.00	16684
2.25	7787	10.25	16900
2.50	8213	10.50	17113
2.75	8618	10.75	17324
3.00	9006	11.00	17534
3.25	9379	11.25	17841
3.50	9739	11.50	17946
3.75	10085	11.75	18149
4.00	10421	12.00	18350
4.25	10748	12.25	18550
4.50	11065	12.50	18747
4.75	11374	12.75	18943
5.00	11676	13.00	19138
5.25	11970	13.25	19331
5.50	12259	13.50	19522
5.75	12541	13.75	19712
6.00	12817	14.00	19901
6.25	13088	14.25	20088
6.50	13354	14.50	20273
6.75	13616	14.75	20458
7.00	13873	15.00	20641
7.25	14126	15.25	20822
7.50	14374	15.50	21002
7.75	14620	15.75	21182
8.00	14861	16.00	21360

The experiments were made as follows: * The power was measured by means of a recording, rotating dynamometer placed between the driving shaft and the blower. The variations of speed were made by changing the pulleys upon the counter-shaft. The revolutions were noted by means of a speed counter, on the counter-

* As these experiments were made at the expense of B. F. Sturtevant, of Boston, to whom the world is indebted more than to any other man for the improvement and development of the blower, it is but just that the credit of them should be given to him, and it will be an assurance also of the care with which they were made and the accuracy of the results.—H. I. S.

shaft, the ends of the blower-shaft being covered so that no other means could be used; by multiplying the speed of counter shaft by the ratio of pulleys on it and on the blower, the speed of the blower was obtained. A slight error in the speed of the fan was possible through slipping of belt, although comparing the speed of belt, which could readily be obtained, with calculated speed of circumference of pulleys on both counter-shafts and fan, none of importance was detected until the high power used was greater than belts are usually expected to convey.

The pressure of the air was determined by the usual water column, and the time of the experiment by a stop watch. Each experiment was of five minutes duration, thus obtaining the revolutions per minute with greater accuracy. Each blower-wheel was carefully measured; and the discharge openings had been carefully made and measured previous to any of the experiments, and were so arranged as to be readily adjusted in place.

TABLE No. 4.

No. of Experiment.	Revolutions per minute.	Area of Discharge in sq. ins.	Observed Pressure in ounces.	Volume of air discharged per min.	Horse-Power.	Actual number of cu. ft. of Air delivered per. H.P.	Theoretical vol. per min. that may be discharged with 1 H.P. at the corresponding Pressure.	Efficiency of Blowers as per Experiment.
1	1519 *a*	0	3.50	0	.80		1048	
2	1479 *b*	6	3.50	406	1.15	353	1048	.337
3	1480 *a*	10	3.50	676	1.30	520	1048	.496
4	1471 *a*	20	3.50	1353	1.95	694	1048	.66
5	1485 *a*	28	3.50	1894	2.55	742	1048	.709
6	1485 *a*	36	3.40	2400	3.10	774	1078	.718
7	1465 *a*	40	3.25	2605	3.30	790	1126	.70
8	1451 *a*	44	2.88	2686	3.50	767	1277	.601
9	1468 *a*	44	3.00	2752	3.55	775	1222	.635
10	1415 *b*	44	2.75	2636	3.30	799	1333	.60
11	1415 *b*	48	2.75	2873	3.45	833	1338	.613
12	1500 *b*	48	3.00	3002	3.80	790	1222	.646
13	1471 *b*	48	2.88	2938	3.70	794	1277	.626
14	1426 *a*	89.5	2.38	3972	4.80	827	1544	.536

[NOTE.—The experiments marked *a* were repeated 3 separate times and those marked *b* only once.]

Discussing first a set of experiments made with a blower for the purpose of determining the changes in pressure and power, resulting from increasing the size of the opening for the delivery of the air; the inlets into the fan remaining the same, throughout the series,

and the revolutions the same as nearly as possible. The fan wheel was 23 inches in diameter, $6\frac{5}{8}$ inches wide at its periphery, and had an inlet of $12\frac{1}{2}$ inches in diameter on either side, which was partially obstructed by the pulleys, which were $5\frac{9}{16}$ inches in diameter. It had 8 blades, each of an area of 45.49 sq. inches. The experiments were carefully made, and in many cases repeated a number of times; the object being to get at the facts, and not to prove any favorite theory.

The discharge of air was through a conical tin tube with sides tapered at an angle of $3\frac{1}{2}$ degrees. The actual area of opening was 7% greater than given in the tables to compensate for the *vena contracta.*

In the last experiment, 89.5 sq. in. represents the actual area of the mouth of the blower less a deduction for a narrow strip of wood placed across it for the purpose of holding the pressure gauge. In calculating the volume of air discharged in the last experiment I have called the value of the *vena contracta* .80, an amount certainly large enough.

The slight apparent variations in the calculated percentages of efficiency may be attributed in part to the difficulty of reading with certainty the height of the water column nearer than $\frac{1}{8}$ in., on account of its capillarity, and to difficulties of measuring the horse-power closer than .05.

I will now give a table of some experiments undertaken for the purpose of showing the results obtained by running the same fan at different speeds with the discharge opening the same throughout the series.

The discharge pipe was a conical tube $8\frac{1}{2}$ inches inside diameter at the end, having an area of 56.74, which is 7% larger than 53 sq. inches, therefore, 53 sq. inches, equal to .368 sq. feet, is called the area of discharge, as that is the practical area by which the volume of air is computed.

One interesting feature will be observed in a study of this table, viz.: As the revolutions of the wheel are increased the velocities due the pressure of air increase in greater ratio than the velocities of the periphery of the wheel; in other words, the air will escape from the discharge opening faster than the outer edge of the wheel travels. Remembering the controversy in Chief Engineer Isherwood's time over "negative slip" with propeller wheels, I do not care to open a similar one about blowers. I merely state the condition of things as I saw them. The increments referred to amount

to about 1 per cent. for each ounce pressure increase. For instance, the circumference of the wheel was 6.0214 feet. At 600 revolutions the velocity of the periphery will be 3613 feet per minute, the pressure ½ oz., and the velocity of air due that pressure 3,658. At 1,800 revolutions the velocity of wheel 10,839 feet, pressure 4.8 oz., and the velocity of air due that pressure 11,435 feet, or about 5 per cent. greater.

TABLE No. 5.

Revolutions per minute	Pressure in oz.	Vol. of Air in cu. ft. per min.	Horse-Power.
600	.50	1336	.25
800	.88	1787	.70
1000	1.38	2245	1.35
1200	2.06	2712	2.20
1400	2.75	3177	3.45
1600	3.80	3670	5.10
1800	4.80	4172	8.00
2000	5.95	4674	11.40

The best useful effect appears to be attained when the discharge opening of the blower about equals the capacity of the wheel; the inlet full open and unobstructed. The same relation reversed, I have found holds true with exhaust fans, *i.e.*; the greatest efficiency when the discharge is free and open and the area of inlet openings equal the capacity of the fan wheel; in this connection bear in mind, long pipes or those with bends, enlargements or contractions in them, by the friction of air through them, have the same effect as partially closing the inlet or outlet of the fan, and where air or material must be conveyed long distances, allowances should be made for this in their diameters.

By calculating the speed of the periphery of the fan wheel in the above experiments and comparing the results with the table of velocity due pressure on page 53, it will be seen that the speed of the periphery of the fan wheel approached near to the velocity due the pressure of air when the area of discharge opening is equal to the area of the blade; but I have compared these experiments with

others of a similar nature, undertaken for the purpose of determining this point, as for instance, using wheels of a greater width, but of same diameter in the same blower casing as used above; and have observed that a nearer approach to the equalization of the velocities of the outer circumference of the wheel, and those due the pressure of the air, can be obtained when the opening of discharge equals the diameter of the wheel multiplied by one-third the width of same at its circumference, and this may be called the "capacity" of the fan, and should be considered the starting point from which all calculations begin.

These conclusions depend greatly, of course, upon the proportions of the fan, and are based upon those used in the "Sturtevant," when run as a blower, as that is the one upon which all these experiments were made.

I have never found any practical difference in the efficiency between blowers with curved blades and those with straight radial ones. I think a careful examination, into the action of a particle of air through a wheel, will disclose the fact that while a curved float passing through a heavy inelastic body, as through water, may show a marked advantage over a straight one, it will not appear in a fan blower, where a large quantity of air in proportion to that passing through the wheel is rotating at as great, or even greater velocity, than the wheel, and packing itself upon the concave surface of the blade; and there are other causes, among them the increasing density of the air as it passes along the blades, and becomes a maximum, after leaving the wheel.

Even if close mathematical calculations can formulate a theory on the basis that particles of air have some weight and that the efficiency of the blower must depend upon the form of its blades, in practice it will require very careful and accurate experiments to find any difference in the power required to run it, or in the quantity of air delivered. I have found, in my experience, cases where parties, using blowers with wheels having curved floats were running them backward, some insisting that was the proper way, that they obtained better results that way than the other, having tried the other, and thinking it wrong, had changed it. There may be one advantage in the *construction* of a blower, using curved floats when it is intended for the purposes of great speed and high pressure, and that is stiffness, but I do not think there is any practical advantage in efficiency.

It is neither proper, nor the object of this paper, to extol one

blower or decry another; but it is so evident that the power required to run one is such a bugbear to most mechanical shops, and as advantage is taken of this fact by some manufacturers to advertise *theirs* as taking only one-half the power of others, it may be well to examine into the question of how this power is used and how it is wasted.

From experiments already given it will be seen that we may expect to receive back 65 to 75 per cent. of the power expended and no more. In the general running of any machinery we should not expect more, and when we read or hear of experiments giving an efficiency of more than 100 per cent. or closely approaching it (see table in Vol. VII. of Transactions, page 547, and some manufacturers' catalogues), we should take them, if we take them at all, with several unusually large grains of salt.

The great amount of power often used to run a fan is not due to the fan itself, but to the method of selecting, erecting, and piping it.

I will give some cases where the application was wrong:

In Fitchburg, Mass., a fan was examined applied to a cupola furnace which was driven by an engine 254 feet away by means of a wire rope; the fan was placed in a pit in the foundry, and discharged its air through an underground pipe about 100 feet long; this pipe had gradually filled with dirt until it probably had less than one-half its original area, and the strength of blast had become strong enough to prevent any further deposits. The power to run the fan was measured by an indicator on the engine, and showed about twice that given in the manufacturers' catalogue: the difference of course, must be attributed to loss by friction of transmission and by friction of air in pipes requiring a higher speed of fan to maintain the proper pressure at the end of the pipe, or at the cupola.

At an iron mill in Troy, New York, were three fans discharging into one system of blast that took I don't know how many times more power than they should, and as it may prove interesting and useful, I present a sketch of the arrangement. (Fig. 1.)

It will be seen that three fans were discharging into a cylindrical pipe 4 feet in diameter, about 15 feet long, attached to one end of which was a pipe 3 feet in diameter and some 30 feet in length with one easy bend; this led into an irregularly shaped receiver from which three pipes of different diameters radiated in the direction of the different furnaces, and from these pipes branch pipes

8, 10, and 12 inches in diameter led to and were connected with the furnaces. Some of these branch pipes were probably over 00 feet long.

The blowers supplied blast for 14 single puddling furnaces using 6,800 lbs. of coal each 24 hours;

1 double puddling furnace using 13,600 lbs.;

1 scrap furnace using 6,800 lbs.;

5 heating furnaces using 10,000 lbs., and 5 forges.

The pressure in the 4-foot main pipe was $4\frac{7}{8}$ oz. about 3 feet

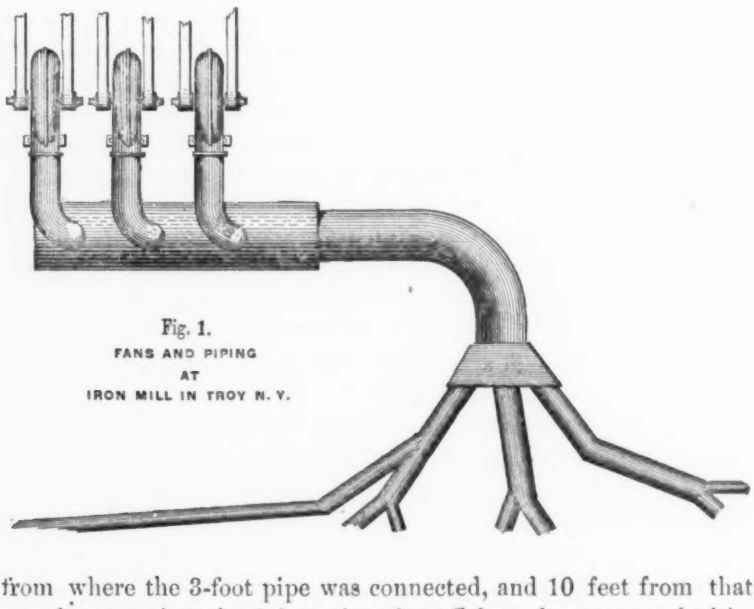


Fig. 1.
FANS AND PIPING
AT
IRON MILL IN TROY N. Y.

from where the 3-foot pipe was connected, and 10 feet from that, or, after entering the 3-foot pipe about 7 feet, the pressure had become reduced to $3\frac{1}{8}$ oz., showing a loss of $1\frac{3}{4}$ oz. Following along the system the pressure in the 8-inch pipes near the various furnaces varied from $2\frac{3}{8}$ oz. to $1\frac{1}{16}$ oz. The power developed by the engine which drove the fans was obtained from indicator cards taken May 2, 1873, but these having been mislaid I am unable to give the amount actually used, but I can make a comparative examination of the power used, and what should have been used if the power had been properly applied by the selection of proper fans and proper pipes, which will illustrate the case, and how such calculation, near enough for practical purposes, may be made.

These furnaces burning say 168,000 lbs. of coal each 24 hours, or

7,000 per hour will require 26,250 cubic feet of air per minute at the rate of 225 cubic feet per lb. coal, a fair and average allowance when blowers are used.

This becomes under a pressure of 2 oz., which I will take as the mean pressure of discharge, to be on the safe side, a volume of 26,030 cubic feet, and under a pressure of $4\frac{7}{8}$ oz. the pressure at which the fans were running equal 25,700 cubic feet.

1,833 cubic feet of air per minute, theoretically, may be discharged under a pressure of 2 oz. with 1 H. P., and 752 cubic feet per minute at a pressure of $4\frac{7}{8}$ oz. Consequently $\frac{26,030}{1,833} = 14.20$ H. P. in the case of the 2 oz. pressure, and $\frac{25,700}{752} = 34.2$ H. P. with $4\frac{7}{8}$ oz. pressure, and adding 50% to each result, making the efficiency $66\frac{2}{3}\%$, we have 21.30 H.P. and 51.30 H. P. respectively, and the difference, or 30 H. P., represents the loss due a poor arrangement of pipes, etc.

This state of things was found in one of the largest concerns in the country more than 14 years ago and may be running in the same manner to-day. It may be an interesting problem to calculate the cost of manufacturing power in that establishment on the liberal estimate of $50 per horse-power per year, $1,500, which would represent a capital of $25,000 at 6% to maintain an improper application of power that could probably be remedied at an expenditure of about one-tenth that sum.

This has not been selected as an example of exceptionally bad work, for others have been found equally bad, if not worse, and all caused by want of a little investigation of the subject. The above comparison has been made on the assumption of the quantity of air required for the combustion of coal used, which must be nearly correct, but should the volume be greater or less the same relative value would be obtained.

When complaints are made "of great consumption of power" I first see if a blower is working at its proper efficiency, and then look for the cause in improper arrangements or application. Small changes in circumstances effect great changes in the result. While writing this article I was called to point out the reason why a pressure blower on a cupola was melting only about one-half the quantity of iron which it was recommended to do, and found it was one about which an article had been published in the *American Machinist*, some five years ago, detailing some experiments then

made, when the results were in excess of the recommendation and excelling those obtained in other foundries. Asking what changes had been made in the arrangements, I was informed none had been. "We are running the same speed, using the same blower and same piping, and charging the cupola in the same manner." Testing the pressure I found all that was due to the speed, and the blower using no excess of power. It was run about 2,150 revolutions by an 8 by 16 vertical engine at 70 revolutions which was also running a small machine shop at the same time. The blower was working under a pressure of 12 ounces all through the heat. The conundrum is, what is the cause of the change? Perhaps there was a change in charging that has been overlooked in five years. I prefer not to express my opinion until the proprietors have made a little investigation themselves.

I recollect some five years ago a paper was read before the American Institute of Mining Engineers, at their Washington meeting, by H. M. Howe, "On the Comparative Efficiency of Fans and Positive Blowers," in which he was convinced against his will that fans gave better results than positive blowers even at high pressure. In this paper I shall not enter into my experiments upon that point, but will merely say that I am strongly of the opinion that for all purposes of blast required in manufacturing establishments up to and including cupola furnaces, the fan is more economical in power and cost of repairs than any "positive blower." I only use the term positive blower technically, for up to the pressures required for the above work, or say to 1 lb. per square inch the fan blower is more positive than piston blowers or those with revolving pistons. I will take the liberty of extracting from the above paper two conclusions which an examination of this paper will confirm:

"No. 9. For a given speed of fan any diminution in the size of the blast orifice decreases the consumption of power and at the same time raises the pressure of blast, but it increases the consumption of power per unit of orifice for a given pressure of blast.

"When the orifice has been reduced to the maximum normal size for any given fan, further diminishing it causes but slight elevation of blast pressure, and when the orifice becomes comparatively small further diminishing it causes no sensible elevation of the blast pressure, which remains practically constant, even when the orifice is entirely closed.

"No. 10. Many of the failures of fans have been due to the low speed or too small pulleys, to improper fastening of belts, or to the

belts being too nearly vertical, in brief to bad mechanical arrangement rather than inherent defects in the principles of the machine."

DISCUSSION.

Mr. Daniel Ashworth.—Long before I was interested in placing commercial blowers practically in the field, I had had some experience and had begun to consider that in sowing a wind we were likely afterward to reap a whirlwind in the blower business. In one case several years ago a Sturtevant blower was placed with a thirty-inch wheel running at a speed of fifteen hundred per minute, and it was represented by those who put it in as requiring six horse power. At that time it was intended to place some electric lights in the establishment, and an engine was secured having sufficient power not only to drive this fan but also to perform the duty of driving the dynamo. A fifteen horse-power engine was procured for that work, and upon getting to work with the fan alone it was found that the engine labored exceedingly. Careful tests were made, the pipe was correct, and everything mechanical was thoroughly complete. That fan required fifteen and three-tenths horse power. The Rochester Tumbler Company have a blower with a forty-four inch wheel of the same make, being driven at 1,200 per minute. This was represented by the makers to absorb twenty horse power at the farthest. Upon getting right down to work with this fan it developed forty-eight and nine-tenths horse power. This test was doubted by the parties, and a short time afterward the Hartford engineering people were in that section of the country. They were not informed as regards the previous tests at all; they were merely engaged to make a thorough test as to its power, and to remove all doubts. Their test exactly coincided with the first, and since that time a number of tests have been made. Usually the first point to be made by the manufacturer of the blower before arriving upon the ground, is that there must be something radically wrong with the mechanical arrangements, or that the pipe must be imperfect. Those gentlemen were promptly on the ground, and acknowledged that the piping was correct, that the mechanical arrangements were perfect in detail, and finally acknowledged that there was no loophole to get out of. That fan to-day is absorbing fifty horse power.

A blower was put upon the market some time ago which was to

run with half the power of any other blower. I found that one of those fans, 42-inch wheel, at 1,000 revolutions, required $42\frac{1}{20}$ horse power. The same fan at 900 revolutions is absorbing $27\frac{10}{100}$ horse power. A Boston blower, 43-inch wheel, with 900 revolutions, is absorbing $18\frac{16}{100}$ horse power.

In illustrating the enormous power absorbed by the bad arrangement of piping mentioned in this paper, it is attributed to the closing up of the discharges, and as therefore doubling the power necessary to rotate the wheel. The Westinghouse Machine Co. received from us a fan for the purpose of experimenting as regards the power to be developed by a rotary engine, and at the same time there was another test on hand to investigate a method of ascertaining the power required by a fan, by the use of a mercurial gauge. I was upon the ground to witness that test, and we had a sliding door placed over the mouth of the fan so as gradually to close the aperture entirely. The result was that as we closed that aperture so would the load be removed from the engine, and as we closed it entirely the engine would race and the mercury would go right down; showing that the mere closing of the aperture in this case and in several other cases does not absorb more power than before; in fact it decreases it rapidly.

In placing our blowers on the market as we do, we would expect to reap a hurricane were we to place a blower which was rated on the data furnished by most of the catalogues of to-day. We stand manfully and squarely up to people, and instead of letting them down easily we say—No, sir; that blower will take so much power. We have refused to take orders for blowers by promising people that they will take so much horse power when we conscientiously believe they will not do anything of the kind, and our competitors have placed them in and have had to reap the whirlwind by taking them down or increasing the power of the engine. A special class of piping to which I would call attention has been applied to glass works. In a majority of cases there have been good sized blowers put in, and the piping has been large—18 and 20 inches diameter. This pipe would be arranged annularwise on a circle about 50 to 70 feet in diameter, interspersed at points of about eighteen inches to two feet apart by vertical pipes at right angles to this. There has been no sparing of pains to make every detail perfect. We have followed that up thoroughly. We have abandoned the idea of all angles, in these pipes, so as to produce the best result. It was the common practice some years

ago to have this main in a series of straight pipes divided into an octagon. We condemn that practice, but use the continuous curve to give the best efficiency to any blower that may be attached to the plant. But we understand that the data furnished by the trade catalogues as regards power are scarcely worth the paper they are printed on for our purpose. That we have learned by actual experience. To show how the trade at large is getting down to that thing, a New England company meets that point in an ingenious way. They do not say it will take so much horse power at all; but they say, We guarantee each blower to be perfect in construction, to run with as little noise as any built, and that *no similar machine doing the same amount of work can be run with less horse power!*

Prof. J. E. Denton.—Before the last speaker's remarks, I was tempted to say that possibly two senses of the word blower might account for some of the trouble related regarding fans in practice. (Laughter.) Mr. Snell states that instead of troubling ourselves about the economy of the engine, we should save more by making one-half of the blower do the work. The fact is that only half the blower is too often proposed to do a given amount of work. It seems to me that Mr. Ashworth has had a very unfortunate experience, but I think the theoretical calculations in the end ought to extricate him, by close enough attention to what is the real cause of the troubles he has described, and which Mr. Snell has shown himself able to solve by the instances cited in his paper. I would remark that the same law of variation of power with speed shown by Mr. Snell's results was found many years ago by a Mr. Buckle of England, whose results are published in Bourne's large treatise on the steam engine. Except in the matter of elbows and abnormal obstructions such as soot or crushing of pipe, we are in possession of the means of determining the resistance of fan blowers to entire satisfaction.

I have found that both the formula of Weisbach and the tables of Sturtevant give frictional resistances entirely in agreement with actual measured amounts for lines of pipe 4,000 feet long with elbows of four diameters radius. Naturally the rating of blowers in catalogues is based upon their performance when unconnected with piping, and the only way to avoid dissatisfaction regarding the realization of the catalogue promises is to take into account the resistances of the piping as far as possible by calculation and then make a very liberal allowance in addition for the

abnormal resistance of friction due to the accumulation of soot, sharp angles, etc., which abound in connection with the use of the fan in many instances. Looking at the matter in this light, it strikes me that Mr. Ashworth need hardly complain of fan-blowers in general, because cases of practice have arisen where a fan used 50 per cent. more power than the catalogue may have stated in connection with the instrument. The fan-blower is certainly an admirable apparatus when properly used. A single fan can, as Mr. Snell has stated, work against pressures up to 12 ounces with excellent efficiency, and within a few years three times this pressure has been successfully worked by using three fans, the first delivering air into the second, and the second into the third. The first cost of such an arrangement would probably be within that of a positive blower such as the Baker, capable of delivering the same amount of air, but it would still possess the one point of weakness in the fan, which it seems to me must always cause the positive blower to be preferred where the pressure to be worked against is liable, through accident or design, to increase considerably beyond 12 ounces, viz., the fact that for a given speed of engine after the pressure against the blower passes above a certain amount, surplus power is of no avail in maintaining constant the supply of air desired from the fan—whereas in the case of the positive blower, as long as the driving power can be increased, the supply of air will be constant, and generally the extra power thus spent is of less consequence than the annoyance and trouble arising from the inability of the fan to permit itself to be forced up to the requirements of the work. Regarding the relative cost of repairs of fans and positive blowers, I am of the opinion that the repairs of blowers of the Baker and Root types having iron revolvers is so little, even under rough usage, that while the fan blower may equal, it cannot excel the positive blower in this respect.

Mr. W. F. Mattes.—The last speaker has raised a point with reference to the comparative cost of maintenance of positive blowers and fan blowers. I have had a little experience in that line with reference to the application of the Sturtevant fans and the Baker positive blowers, so called, to the furnishing of blast power to the cupolas of Bessemer steel works. At the Lackawanna Works, when they were first erected, the cupolas were supplied by two, I think, No. 9, possibly No. 8 Sturtevants. After an experience of perhaps four or five years it was decided to try

the Baker blower. A blower building was erected and four of these blowers put in. We then had another term of experience of four or five years. The result of this is that we have gone back to the Sturtevant fans. It is true that in the mean time the Sturtevant fan has greatly improved. It is also true that we have put in larger sizes, coming up to No. 10; but we think that on the whole we get more efficient work from the fan, although we have not been able to make positive tests of the matter, because the work of the two interlace, the blowers and fans working into the same pipes; but we find this—that the repairs of the Baker blowers were a very serious item. This possibly is due in part to the hurry and drive and rather rough character of the repairs that can possibly be made about such a concern running night and day. But nevertheless the repair item was a very serious one. The interruption of the work was serious, and the parts to be removed were heavy, cumbrous and difficult to handle. On the other hand, the parts to be handled on a Sturtevant fan are all very light and easily carried about. In setting the last fans we have made improvements in the arrangement of pipes, which possibly will account for much of the improvement over the former experience with the fans. By using the fan with an upward discharge, we get rid of one elbow at the start. Then by arranging pipes of ample area, making bends as easy as the locations will permit, we get a much easier delivery. Another advantage results from the action of the fans, in giving automatically an increased volume, when another cupola is put on, or the pipes leak. It may be said that we should not have leaks; but in carrying pipes to six cupolas, varying in internal diameter from seven to eight and one-half feet, with ten tuyeres each and numerous valves, irregular leakage is inevitable. We run with an average of twelve to fourteen ounces. From either cause a slight reduction of pressure ensues, and the fan responds at once with increased efficiency. With a positive blower the loss of pressure is greater and permanent.

In all cases, where the question lies between the adoption of a fan or a blower, the burden of proof is on the blower, because of its greater first cost and general unhandiness.

Prof. Denton.—I would be interested to know if that was the general experience about the Baker blowers, for I inquired very carefully regarding them before investing, and I found a most remarkable record regarding freedom from repairs.

Mr. Geo. Schuhmann.—I should like to call attention to the fact that when there is a large variation in the volume of blast required, and where the speed of the blower remains constant, the fan blower has a big advantage over the positive blower. In order to be on the safe side, we have to put in a blower large enough to supply the maximum amount of wind required. If for a while we require only one-half of the maximum amount, we can throttle down a fan blower and use only little more than one-half of the maximum power, while a positive blower must deliver the full volume, and we have to put in a safety valve to let the surplus escape; so, while we are using only one-half of the blast, we are still using the full power to drive the blower. When the blast is entirely shut off on a fan hardly any power is required to drive it, while with a positive blower it still requires the same power as when doing maximum duty.

Mr. E. C. Felton.—I would like to say a few words in relation to the use of the positive Baker blower in furnishing blast for iron cupolas in steel works. Mr. Mattes, of the Lackawanna Iron and Coal Co., has said that in the works with which he is connected they have gone back to the use of the fan blower for the cupolas connected with their Bessemer works. Now, it may be interesting to note that our experience at the Pennsylvania Steel Works is exactly opposite to this. We began by using two Sturtevant fans, coupled together to get the necessary pressure, but have now done away with them and put in the Baker blowers exclusively in both of our Bessemer plants. We find that they give us entire satisfaction, and that the item of cost of running and maintenance between the two is decidedly in favor of the Baker blower. We have two Baker blowers in our No. 2 Bessemer plant, which have been running since 1881. They are driven direct from a horizontal engine against a pressure of about twelve ounces, and the cost of repairs on them has been so slight that it would be hard to name it. This has been our general experience, and I was surprised to hear from Mr. Mattes that the opposite has proved true at Scranton. I think, however, that the Baker blower is used l most exclusively in all the steel plants I know of.

Prof. R. H. Thurston.—There is one remark made in the paper that strikes me as especially interesting. The writer says that he discovers no difference between the straight and the curved blade. I imagine that the reason of the non-discovery of that difference is a very simple one. If he will take the trouble to lay down the

proper form of blade in order to give the maximum efficiency of fan, he will find that the best form for an ordinary fan is a very nearly straight one. Some years ago I had occasion to put in a heavy fan, and wanted one of high efficiency to do a great deal of work, supplying the blast for a marine engine; and the Sturtevant people made me a fan which was very carefully proportioned. For that case the curvature of blade was, as near as I recollect it, about like that shown in Fig. 82. Now, the difference between that and a straight blade is practically not very great, and I imagine that if a comparison were made between this straight blade and a highly curved one, it would be found that the straight blade would give as high efficiency as this particular curve; and as illustrating the fact that

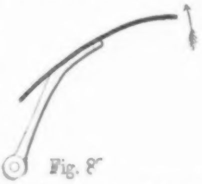


Fig. 8

the curvature is an important element, I can relate an incident that occurred in connection with the fan just described when we were setting it up. When set up and in place we gave it steam, and under full steam we discovered no effect. The air in the fire-room was as placid as before the blower was started. There was no perceptible blast. The connections with the engine were all right. The connections of the blower were all right. We experimented about the machine for nearly half a day, and, finding no apparent defect, came to the conclusion that we should be compelled to send the fan back. But it occurred to me to overhaul the machine and see in what direction it was running and how the valve was set. I found the valve was so arranged that the engine could be driven in either direction, and all that was necessary to secure the reversed direction of movement was to take out a little pin and put it in another hole. Putting that pin into the new hole, on starting we found the engine running in the opposite direction, and *then* we had a gale of wind in the fire-room. The fact was that this curved blade, instead of running as intended, had been running in the reverse direction, and it then had a vastly less efficiency, as a matter of course, than a straight vane would give. The tendency was to scoop in the air at the periphery and to thus give us a reversed blast. So I think more than likely that the reason of the non-discovery of an increased efficiency by the adoption of a curved form may in many cases have been due to some such fact as this, or to the putting in of a curve which is not the correct curve for the purpose. On fast-running fans, when I have sometimes laid out these blades, I have found them

to come nearly to a straight line. Of course, if it is a radial blade, there is a certain loss of efficiency from the impact of the leading edge; but if properly inclined, such a straight blade will give very nearly the efficiency of the curved one; the fact being that the curvature may be too slight to produce an effect observable in the operation of the fan.

Mr. Crane.—What are the components that determine the form of that curve?

Prof. Thurston.—The curve should be given such form that it would take the air on the vane without shock. The edge of the vane would follow the line of the current, taking it into its channels without producing any shock. The design of the blade should be such that the terminal portion should be so shaped and should so move in a peripheral direction as to avoid the production of shock; and the total curvature of the blade is thus determined very largely by the speed at which we may choose to run the fan. It is very easy to design a fan which shall be fast running or slow running. The speed is determined somewhat by the pressure which it is proposed to carry. But with a given rate of flow of air through the fan, a sharp curvature makes a fast-running fan, a less curvature means a comparatively slow-running fan and low pressure.

Prof. Denton.—The gentlemen will find the formula for the inclination of the blade in Rankine's *Applied Mechanics*, under the head of "Pressure of a Forced Vortex," etc., Art. 650, Formula 3.

Mr. H. I. Snell.—Replying to some of the remarks brought out in the discussion of this paper, I think Mr. Ashworth deserves attention first, as he has had the most experience, though I must think him very unfortunate.

If he had read more of the paper, he would have seen one of the objects of the paper was to do something toward removing the prejudice and ignorance concerning blowers. I think he fails to comprehend that no table of the power, etc., of a blower under all conditions of outlet and inlet and at every temperature and condition of barometer can be made. A catalogue may be right which says a thirty-inch wheel at 1,500 revolutions will take only 6 H.P. if the discharge opening be 50 or less square inches; but it does not follow that that catalogue says a thirty-inch wheel takes 6 H.P. at 1,500 revolutions under all conditions of discharge! In the paper I say in Table No. 4, on page 55, that a blower with a twenty-three inch wheel running 1,519 revolutions per minute only

took $\frac{80}{100}$ of 1 H.P. to drive it, and yet in the same table I prove that a twenty-three inch wheel running only 1,426 revolutions a minute took $4\frac{80}{100}$ H.P. It is this ignorance concerning the effect produced by change of conditions in the same blower which causes the great hue and cry against them. If you are advised by your physician that morphine is very good to quiet your nerves, he expects you to use it only as he directs; so with catalogues of blowers; they are guides only under the conditions stated, and if you do not understand them, the best plan is to call upon the manufacturer, as it is his business and interest to start you right.

This paper was not an advertising one, and was not intended as such, although discussion has made comparisons between fan blowers and so-called positive blowers. I shall not take any responsibility for bringing them in, and will take this occasion to say that in my opinion they are all good, and each has fields of usefulness which the other cannot successfully fill.

In my paper I made the claim that a properly proportioned and constructed fan blower was more positive than any piston blower, or blower with revolver, for purposes of blast in manufacturing establishments requiring 1 lb. pressure per square inch or less, and I base that claim upon the principles governing their action. The pressure of air from the fan blower is due to centrifugal force, and is as certain as the law of gravitation upon which it is based. The pressure from the positive blower is wholly dependent upon the accuracy of its construction. Any leakage due to faulty workmanship, or wear, renders its positiveness dependent upon the amount of that leakage, and when we consider the velocity at which air will flow through any aperture under a pound pressure (the table on page 54 gives it at 21,360 feet per minute), and consider the small cubic capacity of these blowers, we can readily see that very small leakages will so greatly reduce the pressure of the confined air as to confirm my claim that the fan is the more positive. As mechanical engineers we all know how impractical it is to make tight revolvers, and if we made accurate fits, that expansion would prevent movement. We are also aware that the unavoidable dampness, dirt, and grit necessarily taken in all blowers would destroy our accuracy. The manufacturers themselves are aware of this, and do not claim their blowers to work tight—they make them as close as possible, and overcome leakages by increasing the speed. My experiments prove that a fan blower speeded to give 12 ounces pressure with a given discharge will give 12

ounces all the time until it is destroyed; while a "positive" blower that will give it to-day may in a year give considerably less under the same conditions of speed and discharge. In the experience of Mr. Mattes, of the Lackawanna Coal & Iron Co., of Scranton, and in those of Mr. Felton, of the Pennsylvania Iron & Steel Co., at Harrisburg, we find directly opposite conclusions as to the relative merits of the fan and positive blower. Their discussion was very interesting to me, and I reasoned from it that the fan blower was best for Bessemer steel plants. Here is the case in point—two large concerns with similar plants have similar experiences up to certain points, but Mr. Mattes carries his investigations beyond those of Mr. Felton, and finds continued improvement by returning to an improved Sturtevant blower put in under more favorable conditions of size, adaptability to work, and improved piping. I would suggest that this question be investigated farther to see to what extent the fan blower and its usual arrangement to-day may be in advance of what it was years ago. Since the time when the first trial was made at Harrisburg, to my knowledge Mr. Sturtevant has spent many thousand dollars in experiments and in perfecting his appliances.

I can hardly reconcile my experiences with those of Prof. Thurston relative to the results obtained when running a wheel backward. In my paper I have taken quite a strong position that the results obtained by reversing the direction of motion of the wheel vary but slightly. There are reasons why there should be some improvement in action if the wheel runs in the direction of the egress of the air, but with straight float radially arranged the pressure within the fan case must be the same, as the centrifugal force is the same; and even with curved blades the centrifugal force is the same running backward or forward, and only a slight difference can be accounted for by the difference in friction between the concave and convex surfaces of the blades. I cannot conceive, and never saw a case, where the centrifugal force of the air was changed into a centripetal by the curvature or "hooking" of the blade. As I disclaimed in my paper any reference to the construction of a fan, I do not think it necessary to discuss the proper form of blade or its angularity with a radial line, knowing many of the mathematical members of the Society could make that part of the subject more interesting than I could, and I hope that may be undertaken at some future meeting.

Mr. Ashworth calls attention to the case at Fitchburg, Mass., mentioned in my paper, and contests my explanation of the cause of extra power as due to the partial filling of the main pipe from the blower to the cupola; overlooking my experiments, he confirms them, by saying his experience has shown him that a partial closing of the pipe diminishes the power. On that point we agree, but that does not fit the Fitchburg case and many others similar to it. At Fitchburg we required a certain pressure at the cupola say 12 ounces, which is the usual pressure for cupolas of the size used there; if the pipe becomes partially closed, in order to realize 12 ounces at the cupola, through the full size of the pipe or tuyeres, it must be forced through an obstructed pipe of diminished area at an increased pressure, or say about 16 ounces, which requires increased speed of fan to maintain that pressure with corresponding increase of power.

Regarding the durability of the fan blower, I can only say I have known of their running daily and in some cases continually night and day for many years at no expense for repairs; with automatic oiling arrangements upon the journals, the only part subject to wear, and these journals of the best material and very large surfaces for the work done, why should they wear? The wheel may be corroded by gases or dampness and be destroyed, but it can be replaced at very small trouble and cost. I should consider I would have a bonanza if I could have a contract to keep all Sturtevant blowers in repair at $\frac{1}{10}$ of 1 per cent. per year of their cost.

Mr. Ashworth.—What would be the result in reversing in your experience with exhaust fans?

Mr. Snell.—The result would be the same. The exhaust fan is merely a fan blower with an enlarged mouth.

Mr. Ashworth.—The reason I asked the question was that we had a matter of that kind. We found that a man had put it wrong side up, and it was turning the wrong way. We changed that location, and the material to be removed disappeared very quickly.

CCLXV.

INTERNAL FRICTION OF NON-CONDENSING ENGINES.

BY R. H. THURSTON, ITHACA, N. Y.

(Member of the Society.)

At a meeting of the American Society of Mechanical Engineers, at New York, November 30, 1886, the writer presented a paper in which he endeavored to give in succinct form, and in as clear and simple a manner as possible, the data derived from certain investigations made under his direction, to determine whether the method of variation of the internal friction of the common non-condensing steam-engine is precisely as stated by standard writers on the subject (especially following De Pambour) in assumptions made without apparently any attempt to check them by experiment. It had been customary among engineers conversant with the operation of the steam-engine to take the "friction-card" produced by applying the indicator to the unloaded engine as representative of the friction of the engine at all times, whether loaded or unloaded; but it had been, in theory, usual to accept, nevertheless, the formula of De Pambour,

$$R = (1 + f)\ R_1 + R_0$$

as given by Rankine, for example, in which R is the total resistance, R_1 is that of the net work of the engine, its "useful" load, and R_0 is the work of friction of the parts of the machine itself. This formula is based upon the very reasonable assumption that the total friction must be a minimum in the unloaded engine, and that the imposition of external work upon it must, by increasing the pressure on its running parts, add to the total by the amount of the friction there arising. That this is naturally the case there can be but little doubt; the question which it has been sought to solve has been, not whether this is a correct assumption, but whether this undoubted increase of waste energy amounts to so much as to become observable, or to be practically important in the operation

of the engine—whether the engineer is right in theory, or practically correct in his practice.

In the paper referred to, it was shown that several distinct lines of experiment had indicated that no serious error is introduced by the assumption that the variation of friction in the engine with varying load is too small, in the class of engine here considered, to be taken into computations of efficiency. It was concluded, as the result of these investigations, that the friction of the non-condensing "high-speed" engine of the better class is sensibly constant, and practically may be so taken, for any given speeds, at all loads, and, at different speeds, also, as independent of the magnitude of the load, from zero of load up to loads exceeding the nominal power of the machine. It was further concluded that this friction varies with the speed of engine, as some function of that speed yet to be determined, increasing as the speed increases and decreasing as the speed decreases. It was also found that the friction increases with the increase of steam pressure.

Finally, it was concluded that the formula,

$$R = R_1 + R_0,$$

may be taken as sensibly correct for such cases, rather than that taken by De Pambour, the value of the co-efficient f being too small to be sensible in presence of other disturbing elements, in common practice, with this form of modern engine.

The writer ventured, in the paper here referred to, to suggest the possible reasons for the insignificant values of this co-efficient. It had been shown by the writer, in the course of a very long series of researches in this field, that the co-efficient of friction of lubricated surfaces usually decreases with increase of pressure, and so rapidly, in some cases—in many cases, in fact—as to make the total work of friction almost constant for all pressures within the range of safe practice. Thus the friction of the engine at the crosshead pin, the crank pin, or the main journal, probably remains sensibly constant, or nearly so, whatever the load thrown upon these parts; while every other part of the engine subject to friction is unaffected by this variation of work done by the machine.* It has never yet been ascertained by direct experiment just what part of the friction of engine is due to the pressure on the connections

* Trans. Am. Soc. Mech. Engrs., Vol. VIII., p. 86. Jour. Franklin Inst., Dec., 1886.

through the line of parts along which the power of the piston is transmitted; but it is evident, from the fact that the co-efficients obtained by the writer, and by others since his investigations were made, under similar conditions of speed of rubbing, pressure and temperature, and with similar surfaces, were usually less than one per cent., instead of being five per cent. and upward, as commonly taken by earlier writers, that the total must be very small, and its variations may well be insensible.

Referring to the variation observed in the friction of the engine with variation of steam-pressure, the fact was noted that the type of engine selected for experiment invariably controls speed by the variation of the point of cut-off, and of the ratio of expansion, by the direct action of the governor, and it was remarked "that such variation results in the readjustment of the set of the valve in such manner as to cause the greater proportion of the nearly constant amount of work performed to be done more nearly at the commencement of the stroke, at a point in the orbit of the crank-pin at which the work is mainly lost by friction, and to reduce the proportion of total work done at or near the 'half-centre,' where it is principally useful. The proportion of useful to lost work is thus varied in such manner as to give a mean final result which is the less favorable as the steam-pressure is higher, and the cut-off shorter, giving a higher ratio of expansion. It is also evident that, if this explanation is correct, the difference here noted will be less as the point of cut-off approaches and passes the half-stroke position of piston and cross-head. Could the valve be set with negative lead for all positions at the point of cut-off, as is considered right by some experienced engineers, the work would be more nearly performed at positions removed from the 'dead points,' and the variation here described would be thus reduced, while the efficiency of the engine would be increased."

Since the presentation of that paper and its publication, the relation of internal friction of engine to the speed has been made the subject of investigation in the Mechanical Laboratory of the Sibley College, Cornell University, and the conclusions already derived, and stated as above, thus corroborated. These experiments were conducted by Professor R. C. Carpenter, of the Michigan Agricultural College. The engine was similar to that employed in the research last described, and was six inches in diameter of cylinder, twelve inches stroke, and the apparatus used with it was that formerly used for the same purposes. The method of operation was

to place a load on the brake such as would bring the engine with an open throttle to a certain speed, and then, when the speed had become constant, to make the observations and record the data. The following table gives the figures of the log so produced at the last and best trials. The brake worked very smoothly and perfectly, and the engine held its speeds very accurately:

Number on Indicator card.	Revolution of Engine.	Indicated Horse Power.	Brake Horse power.	Friction Horse Power.	Number on Indicator card.	Revolution of Engine.	Indicated Horse Power.	Brake Horse power.	Friction Horse Power.
1.........	127	9.193	8.636	0.557	12........	130	7.718	6.695	1.023
2.........	136	9.583	8.500	1.083	13.......	171	9.447	8.208	1.239
4*........	165	11.244	9.570	1.674	14........	217	12.238	10.199	2.039
5	165	10.703	9.735	0.968	15........	219	12.545	11.059	1.486
7*........	192	12.101	10.944	1.157	16........	229	11.662	10.534	1.128
8	80	4.982	4.240	0.742	17........	252	11.836	10.332	1.504
9.........	48	2.979	2.592	0.387	18........	241	11.574	9.640	1.934
10	100	6.555	5.600	0.955	19.... ...	266	11.626	9.709	1.917
....	120	7.647	6.360	1.287	20........	315	13.621	11.182	2.439

* Cards No. 3, 6, lost.

In the next series of experiments, the engine was set at a constant ratio of expansion (2) *i.e.*, with cut-off at one-half, no governor being used, and the speed was regulated by the throttle valve. At five speeds the results were the following:

Rev.	I. H. P.	B. H. P.	F. H. P.
20½	0.449	0.369	0.080
28	0.801	0.504	0.297
152	3.556	2.888	0.768
175	5.650	3.150	0.963
215	4.113	3.970	1.680

A much less satisfactory series of trials gave data as below. They are irregular, and evidently subject to correction; yet they give a general confirmation of the experiments already considered. In these last trials, the brake worked irregularly and unsatisfactorily, and the log is only quoted as giving such general conformatory testimony, and also as showing the dangers which attend an attempt to handle a brake for purposes of this character. It is usually subject to such variations of resistance due to irregularity of lubrication, that it is only by special provisions or extraordinary care that it can be made to give absolutely reliable indications.

Illustrations of this fact are seen in the records for speed of eighty-three and one hundred and forty-three revolutions, where a sudden change of speed shifted the weight slightly, and thus gave the anomalous results recorded. Variations of steam-pressure due to causes beyond the control of the observers, contributed to make the last set of data comparatively unreliable. The speed was controlled and adjusted by hand. The indicator diagrams obtained were satisfactory, but the table is principally of value as showing the liability to irregular working arising in this method of operation:

Revolution of Engine.	Indicated Horse-power.	Brake Horse-power.	Friction Horse-power.	Revolution of Engine.	Indicated Horse-power.	Brake Horse-Power.	Friction Horse-power.
35	1.2733	0.665	0.6083	143	4.416	4.004	0.412
60	2.829	2.700	0.129	150	4.933	4.200	0.733
83	3.217	3.320	0.103	160	5.019	4.000	1.019
95	4.074	3.515	0.559	220	7.716	6.682	1.045
127	4.409	3.810	0.599	244	7.446	6.832	0.914
130	5.064	4.810	0.254	252	8.149	7.434	0.715
138	4.564	4.002	0.562	282	9.211	7.543	1.668
141	4.694	4.935	0.241				

But even these irregularities are capable of being made serviceable as evidence. A general tendency to increase of friction resistance as the speed increases is seen, and by plotting the data, the curve of variation is roughly obtainable. This is not needed, however, as the experiments and data already obtained serve to give the law of variation with a very satisfactory degree of accuracy. The line most closely corresponding with the data which have been found most reliable has very exactly the equation (from the first table),

$$y = 0.008\,x;$$

and the internal friction of this engine is in horse-power about 0.8 per cent. of the number of revolutions per minute.

In engines of this class, therefore, it would seem that the internal friction varies directly as the speed, or sensibly so, other things being equal, and thus that it is directly proportional to the power exerted, and may be taken as a constant fraction thereof, whenever the steam pressure and ratio of expansion, and the wastes, remain unchanged with varying speed.

In this case the engine was operated with a wide-open throttle

valve. In the second case, in which the load was applied and the engine then speeded by handling the throttle, opening or closing it to obtain the desired speed, the friction is seen to have been proportional neither to speed, to power generated, nor to their product. Earlier experiments, already described, had shown that in this engine the friction was not sensibly variable with the load on the piston or brake, and the law is here seen to differ from that controlling it in the first of these trials in which the friction was found proportional, very nearly, to speed. It is thus evident that the method of distribution of steam and application of load must probably have an important influence in some cases, upon the magnitude of the least work of friction of engine. The irregularities observed are, however, supposed to be largely due to variation of effectiveness of cylinder lubrication, and possibly to a less extent to variation in the efficiency of lubrication of other parts of the machine.

It was suggested, in the course of these investigations, that it might be possible that, as indicated by the writer in the earlier paper, the method of steam distribution might have an important influence in determining and modifying the law of variation of internal friction, and that it might not be true in other engines that the friction is independent of the power exerted, when the distribution of steam is effected by another system of valve-motion, as for example, by the older arrangement of regulation by a throttling governor. To determine this point, an investigation was undertaken with the same engine, but with the automatic gear removed and a fixed cut-off, the regulation being effected by a governor of the class last mentioned working a throttle valve in the steam pipe. The work was undertaken by Messrs. A. W. Buchanan and W. D. Gillis, who were aided as occasion offered by Assistant Professor A. W. Smith, in charge of the laboratory. A Gardner governor actuated the throttle-valve in the steam pipe at its point of attachment to the steam-chest. This is one of the most common methods of regulation of the less expensive classes of engine, and the governor selected was taken as representative of the better sort in that class. Its operation was perfectly satisfactory.

The difference in action of the two classes of governor is a very important one, not only as a matter of kinematics, but also as an element of efficiency of engine, and of economy in the use of steam and of fuel. The automatic system, so-called, adjusts the point of

cut-off, and the ratio of expansion with every change of load, in such manner as at every instant to make the energy expended by the engine precisely equal to that demanded to do the required work at the intended speed; while the older system, as illustrated wherever the throttle-governor is used, produces the same result by altering the pressure of steam in the engine without variation of the point of cut-off. The one holds the initial pressure unaltered, varying the extent to which expansion takes place; while the other continually varies the initial pressure, retaining a constant ratio of expansion. The two methods thus produce radically different modes of variation of the distribution of pressure and work on the points of connection of the train of mechanism carrying the work through the machine and applying it to its purpose, and thus, presumably may alter in different and appreciable degrees the total amount of internal friction at every point in the stroke, as well as its total for a full revolution or succession of revolutions. In the first case, the proportion of work done usefully, or in friction, at the early part of the stroke, and while the crank is swinging through a comparatively large arc on either side the "centre," in which arc the work expended is mainly wasted, is being constantly varied; in the second, this proportion remains practically constant whatever the action of the regulating mechanism. The object of this later investigation was thus to ascertain whether such difference in action of the governor can affect the apparent constancy of loss by friction observed at all powers in the "automatic" engine; whether the fact that, in the automatic system, the work is so largely done during the period of minimum efficiency of machine, while in the throttling system it is done in greater proportion near midstroke, and thus in the period of greater efficiency, produces any sensible effect upon the magnitude of internal and wasted work.

The study of the following tables, in which are given all the data obtained during this series of trials, as observed and computed by each of the two observers independently, and thus checked, will probably be considered to settle the question considered very thoroughly. The same variations are to be noted here as in all previous investigations, variations presumed to be mainly due to the impossibility, under usual conditions of working in ordinary engines, of securing perfect or uniform lubrication; but the variations in the amount of internal friction due to variation of power developed are seen to be practically inappreciable, as in the first investigation

reported by the writer. The tables give the results obtained when working with a fixed cut-off at $\frac{1}{8}$, $\frac{1}{4}$, $\frac{3}{8}$, and at $\frac{1}{2}$ stroke. All the figures obtained from the several indicator diagrams are presented, and the revolutions, the indicated and brake-power, the load on the brake-arm, friction horse-power, per cent. of friction, and mean effective pressure on the piston, are all recorded for every observation. It will be thus seen that the variations noted are not due to alteration of load on the engine; but they cannot be traced to variation of steam-pressure, to change of speed, or to any other element of which record could be made. It would seem an unavoidable conclusion that these variations must be attributed to an inconstancy of the co-efficient of friction of lubricated parts, such as is familiar to every one who has given much time to the investigation of the laws of friction of lubricated surfaces. It was endeavored to evade this cause of uncertainty by the use of a "sight-feed" in the steam-chest, with an unusually free flow, but with less success than was hoped for. The conclusions are not, however, less positive and certain on that account.

The speed of the engine was not as perfectly held in this series of experiments as in the first; this method of regulation is never expected to be equal to that introduced with the automatic engine. It here often varied ten or fifteen revolutions from the desired standard as the steam-pressure and the load changed. Following these variations, a variation of the friction-work may be detected according to the law established in the last described set of experiments, but often obscured by the unidentified cause of still larger variation. It is also to be noted that there is a slight but observable gradual increase in the amount of internal friction as the point of cut-off and ratio of expansion are changed, confirming the prediction or suggestion previously made by the writer in discussing earlier work. The friction horse-power ranges from an average of 1.31 at $\frac{1}{8}$ stroke to 1.55 at $\frac{1}{2}$ stroke, at the same speed. (This engine, it will be remembered, is 6 inches in diameter of cylinder, and one foot stroke of piston.) The total power exerted ranged from zero on the brake, and about one and a half horse-power by indicator, up to 5.54, 11.07, 12.60, and 17.40 by brake at the several points of cut-off chosen respectively. The steam-pressure ranged between 85 and 95 pounds at the boiler, its variations being due to causes beyond the control of the experimenters; but they were not sufficient to affect seriously the data or the conclusions.

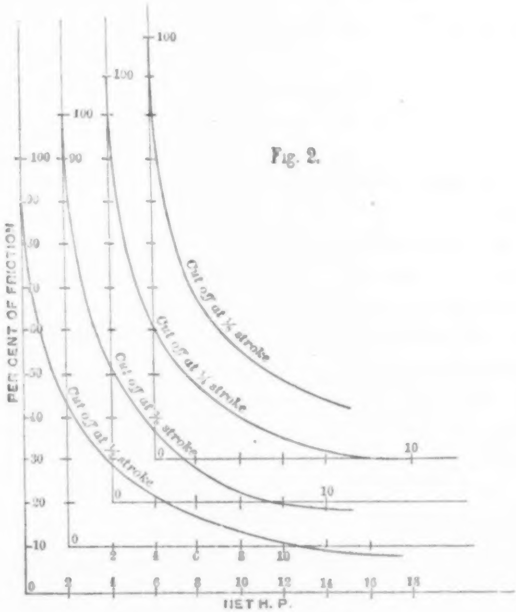
INTERNAL FRICTION OF ENGINE.

SIBLEY COLLEGE, CORNELL UNIVERSITY, JUNE, 1887.

Number of Cards.	Crank End. Length.	Crank End. Area.	Head End. Length.	Head End. Area.	Mean Ordinate. Crank End.	Mean Ordinate. Head End.	Mean Effective Pressure. Crank End.	Mean Effective Pressure. Head End.	Revolutions.	H. P. Indicated. Crank End.	H. P. Indicated. Head End.	I. H. P. Total.	Weight on Brake.	Brake-Arm Ins.	H. P. by Brake.	Loss by Friction. I. H. P.—B. H. P.	Per cent. Friction.	M.E.P. necessary to overcome friction.	Boiler Pressure.
CUT-OFF AT ⅛ STROKE.																			
1	3.13	.08	3.13	.21	.026	.074	2.05	5.36	263	.44	1.19	1.63	0	0	0	1.63	100.	3.71	85
2	3.12	.33	3.13	.60	.106	.192	8.48	15.36	258	1.80	3.36	5.16	15	63¾	3.92	1.24	24.	2.87	85
3	3.14	.38	3.14	.78	.121	.248	9.68	19.84	252	2.00	4.24	6.24	20	63¾	5.08	1.16	18.6	2.75	85
4	3.12	.47	3.11	.95	.151	.305	12.08	24.40	220	2.19	5.55	6.74	25	63¾	5.54	1.20	17.8	3.26	92
CUT-OFF AT ¼ STROKE.																			
1	3.14	.11	3.13	.18	.035	.058	2.80	4.60	262	.60	1.02	1.62	0	0	0	1.62	100.	3.69	89
2	3.12	.40	3.15	.51	.128	.162	10.24	12.96	262	2.20	.68	4.83	15	63¾	3.97	1.11	22.9	2.58	80
3	3.12	.66	3.15	.80	.211	.254	16.88	20.30	248	3.45	4.27	7.72	25	63¾	6.25	1.47	19.	3.54	81
4	3.13	.84	3.14	1.01	.267	.321	21.40	25.7	254	4.48	5.54	10.02	35	63¾	8.96	1.16	11.6	2.71	85
5	3.10	1.14	3.14	1.32	.368	.420	29.44	33.6	244	5.92	6.95	12.87	45	63¾	11.07	1.80	13.9	4.41	89
CUT-OFF AT ⅜ STROKE.																			
1	3.12	.10	3.14	.14	.032	.045	2.56	3.60	270	.57	.83	1.40	0	0	0	1.40	100.	3.10	93
2	3.14	.42	3.14	.47	1.34	.150	10.72	12.	262	2.29	2.67	5.96	15	63¾	3.98	1.98	33.2	4.52	93
3	3.12	.51	3.15	.63	1.63	.200	13.04	16.	254	2.73	3.45	6.17	20	63¾	5.12	1.05	17.	2.47	93
4	3.13	.80	3.14	.85	2.56	.271	20.55	21.68	255	4.30	4.68	8.98	30	63¾	7.71	1.27	14.1	2.98	94
5	3.13	.99	3.14	1.09	3.16	.347	25.28	27.76	250	5.20	5.89	11.10	40	63¾	10.08	1.02	9.2	2.43	93
6	3.13	1.24	3.14	1.40	3.96	.446	31.68	35.68	250	6.53	9.56	14.09	50	63¾	12.60	1.49	10.6	3.65	90
CUT-OFF AT ½ STROKE.																			
1	3.13	.12	3.14	.15	.038	.048	3.04	3.84	272	.67	.88	1.55	0	0	0	1.55	100.	3.43	95
2	3.14	.50	3.14	.54	.159	.172	12.72	13.76	258	2.70	3.01	5.71	15	63¾	3.92	1.79	31.3	4.15	90
3	3.12	.60	3.15	.68	.192	.216	15.36	17.28	256	3.24	3.75	6.99	20	63¾	5.16	1.83	26.1	4.27	90
4	3.13	.80	3.13	.87	.256	.278	20.5	22.24	253	4.27	4.77	9.04	30	63¾	7.65	1.39	15.3	3.28	85
5	3.13	1.06	3.14	1.11	.339	.353	27.12	28.24	252	5.63	6.03	11.66	40	63¾	10.16	1.50	12.8	3.56	85
6	3.14	1.33	3.13	1.42	.423	.453	33.84	36.24	250	6.97	7.68	14.65	50	63¾	12.60	2.05	13.9	4.90	85
7	3.13	1.50	3.13	1.59	.479	.508	38.32	40.64	246	7.77	8.47	16.25	60	63¾	14.87	1.38	8.49	3.40	82
8	3.12	1.72	3.13	1.83	.551	.584	44.08	46.72	241	8.75	9.55	18.30	70	63¾	17.00	1.30	7.10	3.18	87
9	3.11	1.93	3.10	2.06	.620	.665	44.96	53.2	216	8.83	9.74	18.56	80	63¾	17.40	1.16	6.2	3.25	90

A column is introduced into the tables giving the percentage of the total power developed which is expended in the overcoming of the internal friction of the engine. This column exhibits, perhaps even more clearly than either of the others, the gain to be secured by throwing upon an engine the maximum amount of load consistent with maximum efficiency of fluid and with highest total efficiency of engine in other respects. General experience indicates

that a cut-off at less than one-fourth stroke, at such pressures, is usually uneconomical in use of steam in such engines, and this investigation shows that the friction of engine is an important element of loss for small powers, and that a throttling governor and light loads mean serious defect in efficiency and economy. Such an engine should, as far as possible, be adapted to its work in such manner that it should be at all times as fully loaded as possible. It would probably be even sometimes advisable to have two engines, the one for periods of light work, the other for times of full power, in order to secure maximum commercial economy.



The accompanying engraving (Fig. 2.) exhibits graphically the results of these investigations, as obtained by plotting the data so obtained, taking the net horse-power for the abscissas and the corresponding amount of friction in per cent. of that power for the ordinates. It is at a glance seen that the curves so obtained are nearly, if not precisely, parabolic, and that, therefore, the internal friction must have a constant value. If we were to plot the results here given, taking the internal friction as the abscissas, and the total powers as the ordinates of the curves, the line would be rectilinear and parallel to the axis of abscissas.

CONCLUSIONS.

(1.) The conclusion derived by study of the above is evidently that the internal friction of an engine of this class, operated under the conditions here described, with a constant speed secured by the action of a thottling governor, is sensibly constant for all loads, and that the variations occurring at the points of connection in the train transmitting the work of the engine, when the power varies, form too small a proportion of the total friction to have important or sensible effect on the total, or to be observed in presence of other usual causes of irregularity.

(2.) The conclusion reached by a comparison of the results of this investigation with those which have preceded is as obviously that the internal friction of this class of engine (the non-condensing) is sensibly independent of the magnitude of the load and of the method of steam distribution, or of the power developed; but that it is variable with speed and with efficiency of lubrication in a very observable degree.

(3.) In the engine here used, the total friction was considerably less than in that employed in the investigation last reported, probably in consequence of its having been longer in service, and its bearing having thus come to a better condition. We may thus readily find here confirmation of a fact well known to engineers of experience, that the operation of a well-cared-for engine will continuously, and for a long time, appreciably reduce the internal friction of the machine.

DISCUSSION.

Prof. J. E. Denton.—We have now had presented to the society by Prof. Thurston, several instances of this paradoxical showing regarding the friction of engines. In connection with his previous paper in addition to the examples of the author, Mr. Barrus presented results from a one hundred horse power Corliss engine in which the friction was similarily constant. There was some query in Prof. Thurston's previous paper as to whether the principle would hold for all classes of engines and various conditions of running of the same engine.

He has added now a very interesting and laborious set of experiments further answering such query.

I have put in the table below three engines which have come under my notice, the results regarding which still further increase

the probability of the hypothesis that the friction of all steam engines with cranks is practically constant for all loads.

Indicated Horse Power.	Friction in Horse Power.	Style of Engine.
84 loaded. 10 unloaded.	7 loaded. 10 unloaded.	Westinghouse 12 x 11. 300 revolutions.
23. loaded. 5.1 unloaded.	5 loaded. 5.1 unloaded.	Buckeye 7 x 14. 280 revolutions.
347 185	44 40	Compound Condensing. Power varied by throttling.
181 137	19 25	Compound Condensing. Power varied by expansion.

The case of the Westinghouse engine shows considerably *less* friction loaded than when unloaded. The result was obtained with the use of a Prony brake which was under perfect control, water being maintained against the inner surface of the brake wheel rim by centrifugal force without any splashing of water upon the rubbing surface to interfere with the uniform lubrication of the latter.

In the case of the Buckeye engine, the power was absorbed by a dynamo adjusted upon a Brackett's "cradle dynamometer." Connection was made through a vertical belt between the engine and dynamo.

The variations of power were made without practical variation of the slipping of the belt, and hence the total strain upon the main shaft bearings was not affected by the change of load upon the engine, so that the showing of constant friction is in this case proved to be independent of the lessening of strain upon the main shaft bearings, due to the greater upward reaction of the Prony brake arm as the load upon the engine is increased. (This point was pertinently referred to in the discussion of Prof. Thurston's previous paper by Mr. Hawkins.)

The cases of compound condensing, throttling, and expansion engines are selected from the Alsatian engine experiments of Hirn and Hallauer made in 1876. A Prony brake was used to absorb the power.

I think that, taking into account all the facts thus far presented on this subject, there is no doubt that the friction of all modern crank engines is practically independent of the load, and Prof. Thurston is clearly entitled to the credit of calling special attention to the fact.

I presume this fact regarding the constancy of the friction has been often met by others, but no particular notice has been taken of it,* and although, as Mr. Wolff remarked in discussing the previous paper, most steam experts have used the friction indicator cards of engines as representing the friction of the engine for all loads; on the other hand, much time and money have been occasionally expended in attempting to insert dynamometers between engines and their work, on the assumption that the engine friction varied with the load to an important degree. The usefulness of Prof. Thurston's labors on the subject is, therefore, not at all problematical, inasmuch as all such attempts at dynamometry may be eliminated from programmes of engine testing.

Regarding the explanation of the cause of the friction remaining practically constant, I am not satisfied with that proposed by the author, which I understand to be substantially that, since the Thurston oil experiments have shown it possible to secure less than one per cent. coefficient of friction with journals, the total journal friction of an engine is a small quantity compared with the friction of its piston and valve gear, and hence as the latter is not liable to increase with increase of load upon the engine, the only variable friction is that due to the journals, which because of the reduction of coefficient of friction with increase of pressure, do not vary sufficiently in their friction with increase of load to affect the total friction sensibly. Now the following facts are opposed to this explanation:

1st. Coefficients of friction of one per cent. and under are only obtained with bearings in an exceptionally smooth condition, supplied with an impractically large supply of lubricant, and operated under conditions which afford a constant longitudinal motion of considerable amplitude. None of these conditions are obtained in engine bearings in practice.

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* Tests of 16 x 30 Porter-Allen engine made at American Institute, New York, 1871, gave results as follows (power absorbed by Prony brake):

Indicated H.P.	27	56	84	109	142
Friction by Prony Brake	9.1	9.5	8.4	8.7	12.7

2d. The piston rod of a 7″ × 14″ engine being freed from the cross head, and steam of 60 lbs. pressure let into both ends of the cylinder, the piston moved rapidly toward the cross head.

Area of piston-rod, $\frac{1}{38}$ of piston.

Area of piston packing rings, 10 inches.

Assuming steam to act underneath the ring with full pressure, the coefficient of friction, neglecting the stuffing-box friction, is about 7 per cent. Take this coefficient at 10 per cent., and subtract the power absorbed by the piston and valve, on this basis, from the total friction of the engine to which the paper refers, and it will be necessary to assume upward of 10 per cent. coefficient of friction at all bearings to account for the remaining friction of the engine.

3d. If an engine with crank and fly-wheel be run without a governor, against a constant resistance, no practical variation of speed can be caused by varying the lubrication of the cylinder, whereas a rock drill or steam hammer, or direct acting pump, has its speed largely affected by variations of cylinder lubrication. Hence it appears that the friction of the piston and valve is not the main internal resistance of a crank engine.

4th. Although it is true that the coefficients of friction of journals decrease with increase of pressure, such reduction is never sufficient, with constant feed of oil, to prevent some increase of the absolute friction. Hence the cases cited in which engines show *less* friction loaded than unloaded are not at all accounted for by this reduction of coefficient with pressure.

The following hypothesis regarding the cause of the constant friction phenomenon appears to possess probability:

It is based upon the fact that the most sensitive factor in the friction of a bearing is the amount of oil supplied. If this be increased where the supply is restricted, as in the case of practical bearings generally, it is quite possible to reduce the absolute friction at a given pressure to *less* than that obtained at a lower pressure under the *less* supply of the lubricant.

If, now, a bearing—for instance, a crank pin—has attached to it an oil cup capable of supplying lubricant continuously, the rate at which the oil is fed from the cup to the pin may be largely affected by the suction produced by the release of pressure on one side of the pin at the instant of passing the dead center; and the harder the engine is worked, the greater will be this suction, and the greater the quantity of lubricant applied to the bearing, and

hence a possible reduction of friction might ensue consistent with the results obtained by Prof. Thurston's tests.

Such action would apply to all circular journals, and possibly to the slides.

Oil cups for shafting, and some forms of locomotive crank pin cups, are adjusted so as to feed a drop only when pressed upon the hand and quickly withdrawn, so that a slight suction is created; and it is assumed that a similar but greater suction acts through the motions of the bearings, to cause the cups to feed.

The above hypothesis is consistent with this idea.

Mr. H. R. Towne.—If the curious results reported by Prof. Thurston are true of a new engine, or engine that has not been run long enough to bring it down to its bearings, they would certainly seem paradoxical; if applied to an engine which has been a considerable length of time in use, and has got into its best working condition, I can see that there is a possible explanation for a portion, if not all, of the results referred to. It would be this—that an ordinary steam engine, under usual conditions, is run with a tolerably constant load. It is conceivable that its bearings and parts adjust themselves to the conditions obtaining under that load and attain their best working condition there. If, therefore, the load is taken off, the elastic qualities of the materials employed in constructing the different parts of the engine would tend to produce slight disturbances in the relationship of the parts, which changes might be quite sufficient so to alter the bearings of one on the other as to produce this curious result—an increase of friction with a diminution of load. I would ask Prof. Thurston if his experiments have covered ground which would make this point clear—whether any difference has been noticed in these results in experimenting with an engine which is new as compared with one that has been running a considerable time.

Mr. Samuel Webber.—I arrived at precisely the same conclusion on the transmission of power through a transmitting dynamometer some years since, or in 1871, which Prof. Thurston has arrived at with the engine. I found slight differences in the friction due to velocity, but I also found that after I had determined the velocity and friction, any addition to the load placed on the Prony brake at one end of the transmitting dynamometer simply required an equal weight to be placed on the lever of the dynamometer to balance it on the other. We started with a small fraction of a horse power and went up to four or five or six horse

power, with some weight added at the other end immediately balancing it. I found the same trouble which Prof. Thurston suggests for many reasons in getting at these results, before I could get the Prony brake to work with an equal and smooth friction. I believe that is the experience with every one who has ever attempted to use one. To work a Prony brake properly requires about three people—one to handle the screws, one to attend to the lubrication, and one to count the revolutions; but I am happy to see that these results, arrived at a dozen or more years ago, allow me to say that I agree entirely with the conclusions arrived at by Mr. Thurston. These data were entrusted to a friend in 1871, and were mislaid, and only came to light after his death, some three years ago.

Mr. Wm. Kent.—I would like to ask if Mr. Webber has reduced his figures to the coefficient of friction.

Mr. Webber.—I did, sir, and the coefficient of friction diminished very rapidly, and almost exactly in agreement with the figures given by Mr. Woodbury in the paper read some three or four years since before the society.

Prof. R. H. Thurston.—I do not think, Mr. President, that I have very much to add to what has been said. Certainly nothing in contradiction to what has been stated here. I have not thought that it was very much of a paradox; the real question with me was whether it was correct to say, as DePambour has said, that the total load on an engine, or on any machine, is equal to a certain constant plus a certain percentage of the added load. The custom of engineers all through the days of my early experience has been to assume that the internal friction of an engine is a constant quantity, and these experiments were made simply to ascertain which were right—De Pambour and those who followed him, or those more experienced, if less accurate, engineers who had come to the conclusion that it was practically correct to say that the internal friction was constant whether the engine be loaded or not.

In regard to the working of journals, I am not at all sure yet * what are the coefficients to be expected in the ordinary working of engines. I do not think it is correct to say that there is no end-play in the journal, because we usually intend that there *shall* be. On a journal of six or eight inches in diameter, and twice that length, I would give an end-play from an eighth to a quarter of an inch, and should always endeavor to see that it took it up.

* Investigations in progress, and to be reported later, may settle this point.
R. H. T.

In some cases builders—my impression is that Prof. Sweet does it—so construct the journals of the main shaft that the engine not only has end-play there, but actually takes it up, by a slight endwise motion of the crank-shaft at every turn of the engine. However, I have seen engines working for a great many years in which there was no observable end-play, and with journals so magnificently smooth that no testing-machine jorunal, as ordinarily used, could approach them. I remember one such old engine well—one with which I was familiar when a boy. It had settled down to a regimen which was absolutely constant, and the face of the journal had assumed a beautiful brown, mirror-like polish, and even the minute scores made in the surface by an occasional bit of grit had not been rubbed out by the endwise motion ; the condition of the journal was almost perfection. I have no question that ample end-play gives this perfection with much greater certainty. I see no reason why the operation of the engine should not give coefficients very similar to those obtained on a testing machine. It is true that in the ordinary testing of metals on testing machines there is often a large surplus of oil used ; and yet, where we have used the ordinary wick feed, we have obtained these same low coefficients.* The lowest coefficients, even, that have been obtained by this latter method, so far as I know, have been improved upon vastly by the method of flooding journals by the "oil bath," illustrated in all the experiments of Beauchamp Tower, which were made for the British Institution of Mechanical Engineers.† But I make no pretense of asserting exactly how the friction of the engine is distributed. The suggestion I made a year ago seems a very plausible one—that in the presence of a considerable amount of friction these quantities may disappear as affecting the total—and I think it is possible that this may cover the case. I am not at all sure of that, however.

I was asked whether I knew anything of the difference between old and new engines. I do not. The engines which are reported here as experimented upon were practically new. Two were newly constructed, and the other, although an older engine, had been in use a less proportionate time. I should assume that the coefficient of friction of journals would be lower in old engines that had been running a long time and well cared for than in new.

* Coefficients of one-half of one per cent. are often obtained under conditions which seem to me less favorable than in good engine-shaft and pin journals.

† Trans. Inst, Mech. Eng'rs, Great Britain, 1883, p. 632. R. H. T.

CCLXVI.

A NEW PRINCIPLE IN STEAM PISTON PACKING.

BY JOHN E. SWEET, SYRACUSE, NEW YORK.

(Member of the Society.)

So far as is known to the writer, wherever pistons have been packed to prevent the escape of steam (except in the case now to be described) the object has been accomplished by forcing the hemp or rings, or whatever was used, against the surface of the cylinder. That this system has been perfectly satisfactory in thousands of

Fig. 3

cases is undeniable; that it has not always been satisfactory is equally undeniable. That nearly all sorts have been satisfactory in certain cases is probably true, but that any one sort has always been satisfactory, no one, I guess, believes, except makers of that particular sort. That the principle embodied in the new plan will always work right, is not at all likely, but it will work, in many cases, I believe, where others have failed.

The reason the simple small square rings turned eccentric and sprung in the cylinders do not always prove successful is due to the difficulty in establishing a proper balance between two conflicting conditions; sufficient initial tension so as not to be stuck fast by

bad oil or abrasion, or liability unduly to wear themselves and the cylinders if given too much set.

Fig. 3 shows all there is really new about the scheme I have to present, and it is what it appears to be, a common eccentric ring hooked together by a clamp which forms a part of the ring itself, and this hook clamp limits the expansion of the ring and changes the whole principle of its action.

The rings are cast heavy, rough turned very much larger than the cylinder, a piece cut out, sprung together and fitted with the hook clamp or shoes, left slightly larger than the cylinder and then re-turned to a tight fit. It will be noticed that the rings can com-

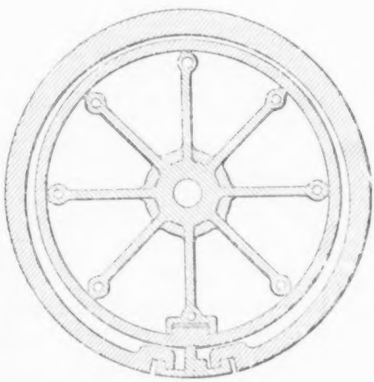

Fig. 4.

press to a limited extent, but cannot expand. In use they act, or are supposed to act, as follows:

When the engine is first started and the hot piston moves to the cold end of the cylinder, the rings compress and allow it to go free; but when both cylinder and piston get up to working temperature, the rings just fit and work without any pressure and very little tendency to wear.

Filing out the hooks compensates for wear when it has taken place. It will be seen that the hook clamp is longer at one end than the other. The object of this is to break joints when two rings are placed side and side in the same groove, and thus cut off the leak which would otherwise take place through the gaps. The hook clamps or shoes are placed at the bottom of the piston, in the horizontal engines, and secured by leaving them a tight fit and allowing the follower to bind them fast.

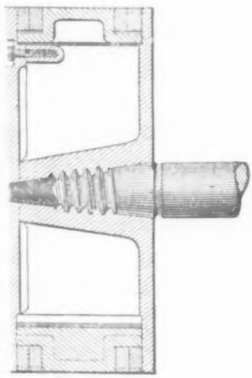

Fig. 5.

Figs. 4 and 5 show the arrangement as used in a large piston with spider, bull ring and follower, and the method of lining up the rod with liners between bull ring and spider.

The objection to the plan is that it is only applicable, with any prospect of success, to parallel cylinders, a thing not always obtainable, but one that is more easily obtained as machinery and methods improve. With a parallel cylinder, and the job properly fitted up, I know of nothing in the line more satisfactory.

DISCUSSION.

Prof. J. B. Webb.—I would like to ask Prof. Sweet whether the invention cannot be described as a solid piston with an arrangement to prevent it from sticking in the cylinder—whether, so far as the working quality is concerned, it is not essentially a solid piston?

Prof. De Volson Wood.—I wish to ask to what use has this ring been put? It so happens that many good things which appear very good at first, prove to be defective with use. As long as a thing is really new, if it is fairly good, we do not know the objections which may arise afterward. I would like to know whether this device has been tested in practice to an extent to show its merits and demerits.

Mr. Geo. S. Strong.—I would like to ask Mr. Sweet, or any one else who has had experience, why cast-iron rings where they are sprung in, as they are in ordinary locomotive practice, or Ramsbottom rings, have a tendency to break in service. They are ver good as long as they last; but I find they very often will break, and the engine will run for some months before you find it out. You take the cylinder head off and find your ring in twenty or thirty pieces, sometimes cutting the cylinder to pieces. I am very much interested in the question of finding a good piston packing which will stay together and last. Perhaps Mr. Sweet's device will overcome the difficulty I speak of. I cannot account for it myself, and I have not met any one who can account for that tendency. Sometimes it will last for a year, and again

you will have to put in a dozen in a year. I do not know whether it is water in the cylinder, or unequal pressure, or what it is that causes these rings to go to pieces.

Mr. F. H. Ball.—I would like to ask Prof. Sweet if he has not found that parallel cylinders wear larger in the center, and how his ring would do in cases of that kind. The elastic ring, of course, would follow the cylinder in all positions of the piston.

Mr. Oberlin Smith.—I think that when a ring breaks, it is from the same cause that makes other metallic structures break—it has to move too near to the elastic limit of that particular piece of metal. Probably the qualities of cast-iron which are homogeneous and have no flaws in them last better than the poor ones. But, perhaps, if rings that do break were proportioned differently—made thinner in a radial direction, they would not give way so often in the way Mr. Strong speaks of.

I would like to ask Prof. Sweet in regard to the wear of the outside of the ring and the bore of cylinder. The paper says that he files away the hook portion, when it is worn too loose, so as to allow it to expand a little larger after so filing. How often does this usually have to be done?

Mr. T. R. Almond.—Wouldn't it appear that the increased velocity of the piston would be the cause of the extra wear?

Mr. H. deB. Parsons.—In reply to the question just put, I would say that it has always been considered by me (granting the cylinder is worn in the middle), that it was due to compression in the packing of the stuffing box. When the crank at mid-stroke is at right angles to the center line of the engine, and the connecting rod at its greatest angle, the latter exerts a vertical pressure on the end of the piston rod, which would be enough to throw the piston slightly out of parallel and thus unduly wear the cylinder.

Mr. Angus Sinclair.—I think if that deflection was sufficient to lead to the breakage of a piston ring that it would be felt much more on metallic rod packing for instance, and that it would injure the packing so much that it would leak badly before it would lead to injury to the rings inside.

Mr. Almond.—I would like to ask Mr. Sinclair what he thinks about the action of the piston in the cylinder in traveling across the center and the quantity of motion there. The increased motion of the piston as it passes the center, in my opinion, would cause the increase of wear at that point.

The President.—Increased velocity, you mean?

Mr. Almond.—Greater velocity in passing the center or at the center. The greatest velocity must be, of course, at the center.

Mr. Sinclair.—I can scarcely see it. I think I can hardly agree with that.

About that matter which Mr. Strong spoke of in regard to the breakage of Ramsbottom piston rings, my experience has been that some roads use these very successfully and have no reason to complain of the breakage. The breakage is not by any means greater than with the ordinary steam piston rings, and I have come to the conclusion that the material used has something to do with it, and I think that occasionally the rings are injured in springing them on. I can see no other causes which would lead to the destruction of which he speaks.

Mr. O. C. Woolson.—I want to know if any of the gentlemen here have observed, in opening the steam cylinder of a pump, or the cylinder of an engine, that the center of the cylinder had the appearance of being much less lubricated than the ends. It is a matter that suggested itself to me in years past, that if there is any difficulty at all in lubricating a cylinder it occurs more in the center than at the end. I think that would account for the wear somewhat. In addition to that, it has also occurred to me that in the steam packing rings there is an opportunity of getting a greater force on these rings in the center of the cylinder than at the ends. In other words, unless we have a large cushion, the packing rings do not get to business quite as quick, and I think there is a greater force on those rings, as it starts away from each end, than directly at the end, and in that way I think it helps to wear the center, when there is evidence of such wear.

Mr. Strong.—Since I spoke before, I have had my attention called to a ring they are using on the Pennsylvania Railroad, in which, in turning the grooves in the piston, they slip the center off about one-sixteenth of an inch, turning in just room enough to allow the ring to bear against the bottom of the groove and against the face of the cylinder, so that they carry the piston on the ring, and they turn the ring without any eccentricity, and they leave the ring loose in the groove, so that the ring is free to turn, and in practice they find that the ring does turn so that it wears perfectly even all the time. It does not wear thinner at the bottom or at any one point, and the piston itself never rides on the cylinder, but rides on the ring altogether, and they are getting very good results, so far, on these rings. I think probably this break-

age I speak of is due in a good many cases to the fact that the rings are fastened on the piston so that they cannot turn, and they wear thinner where they ride on the bottom of the cylinder. They become weaker there than at any other point and break.

Mr. Geo. H. Babcock.—The time which is appropriated to this subject has expired. Permit me to add a word in regard to the wearing of steam cylinders. It is not a regular thing for a steam cylinder to wear larger in the middle. I have known many of them to wear larger at the ends, at one time when it was fashionable to put in a "steam" packing, as they called it, in which the pressure was admitted under the rings. In engines with short cut-off, it was found that the cylinders would wear considerably faster at the ends than in the middle. I do not remember that I ever noticed a cylinder which had worn larger in the middle than at the ends; but it is not at all uncommon to find a cylinder smaller just at the end. Where the piston does not override the counterbore, it is very common to find a ring at the end smaller than the rest of the bore, which may lead to the impression that it is worn larger in the middle. Speaking of the wear of steam cylinders, I remember an old steamboat on the Hudson, the *Rip Van Winkle*, which had a horizontal cylinder laid down upon the keel working through a triangular beam. It was said when she was built that the cylinder would wear out of round because of the weight of the piston running in the horizontal cylinder, but after that engine had been running twenty years it was very carefully measured, and it was found that the horizontal diameter of the cylinder was actually greater than the vertical direction; the difference being the spring of the metal. Owing to the fact that this cylinder was bored vertically, when it was laid down upon its side it sprung slightly, from its own weight, and twenty years of constant use had not been sufficient to wear the amount of that spring out of it.

Mr. Daniel Ashworth.—It has occurred to me in close observation upon the boring of cylinders, that in a majority of cases where there is a difference in the texture of the iron, that the porous portion, or the less homogeneous portion, is mostly in the center of the cylinder. We have a clearer, more solid texture at the ends, and it seems to me that, under such circumstances, there would be, at the very start, a tendency to wear more at the middle of the cylinder than where the iron is of a closer texture. This is something which I have frequently observed in boring

cylinders. That may have something to do with a cylinder wearing larger in the center than at the ends.

Mr. T. R. Almond.—Some experiments were made in my shop by Mr. Hobart in turning out a hole in a large coupling, the hole being four inches in diameter. He found that the power required on one side of the hole when the tool was boring was very considerably greater than on the other side, and the casting was a very good-looking casting, solid throughout, and, to all appearances, it hardly showed any variation in porosity or density. The case was rather surprising to me, although I had noticed for a long time all those little difficulties that occur because of the varying densities of materials. But this was specially noticeable because it took so very much more power on one side of the hole than on the other—perhaps as much as 20 per cent. more. There were a great many experiments made in that connection at that time, and in every instance we noticed such a difference in the density corresponding to, perhaps, the bottom or to the top of the casting.

Mr. John E. Sweet.—In reply to Prof. Webb, I would say that, according to my notion, the piston acts exactly like a solid piston after both piston and cylinder become heated to the same temperature; and to Prof. Wood, that it cannot be told what may be the result after some years of use, because it is not over two years since the first one was made. Aside from my own experience, I will say that rings put in a pair of 26″ cylinders, and run six months without stopping, were reported by the engineer to be as tight as when first put in; and in the case of a 24″ Buckeye engine at Beaver Falls, the chief engineer writes that after running six months he put 95 lbs. of steam in one end, and his head in the other, without inconvenience.

In regard to the breaking of the small cast-iron rings referred to by Mr. Strong, I know of but one reason, not mentioned, which may lead to this breakage: when the grooves are deeper than the thickness of the rings, and the rings overrun the counterbore, they may be blown in, so to speak: then, after leaving the counterbore, snap out against the cylinder, and in that way break themselves. That they do so blow in and snap out, we have proven to our own satisfaction to be a fact.

To Mr. Oberlin Smith I would say that we cannot tell how often it will be necessary to file out the hooks, as we have not, as yet, had to file out any.

Regarding the cylinder wearing larger at the ends than in the middle, or larger in the middle than at the ends, so far as this method of packing is concerned, there is no experience to indicate what the final result is to be, but reason would indicate that it is impossible for the ring to wear the cylinder unevenly, whatever other influences may do. That the increased velocity should wear the cylinder faster in the middle than at the ends, I do not believe; nor should I expect a cylinder to be more porous in the middle than at the ends, nor inefficient lubrication to show itself any sooner there. Imperfect alignment is far more likely to cause trouble by making a fulcrum of the piston-rod packing, as suggested by Mr. Parsons.

CCLXVII.

AN ECONOMICAL METHOD OF HEATING AND VENTILATING AN OFFICE AND WAREHOUSE BUILDING.

BY HENRY I. SNELL, [PHILADELPHIA, PA.]

(Member of the Society.)

VARIOUS methods have been devised and are in use for heating large rooms, manufactories, and public buildings. Some of them take into consideration the ventilation of the buildings as well.

I will describe briefly a method which I have had in use in my store in North Third Street, Philadelphia, Pa., for the past two winters, which has been very satisfactory. It has been very economical, and dependence could be placed upon its efficiency at all times, no matter what the condition of the weather might be.

A reference to the accompanying sketch will clearly give a correct understanding of the arrangement. (Fig. 6.)

An exhaust fan driven direct by a small upright engine is connected with a "patent air-heater" placed in the basement at the front of the store by an 18-inch galvanized pipe.

An upright boiler in the basement furnishes steam to run the engine; the exhaust steam from the engine is delivered through the exhaust pipe into the base of the air-heater on one side, and the drip and condensed steam is conveyed away through a pipe at the other.

The exhaust steam of the engine furnishes all the heat usually used, but as a precaution, and for use early in the morning, in extremely cold weather, or for use in very moderate weather, in the middle of the day, when it is unnecessary to run the engine, a small live steam pipe is connected with the base of the heater. The fan runs at a very low speed, and is perfectly noiseless. In my case, no conducting pipes for the distribution of the air are necessary, and the variations of temperature in different parts of the store are not observable with the ordinary commercial thermometer. By examining the sketch, it will be seen the store itself becomes one large

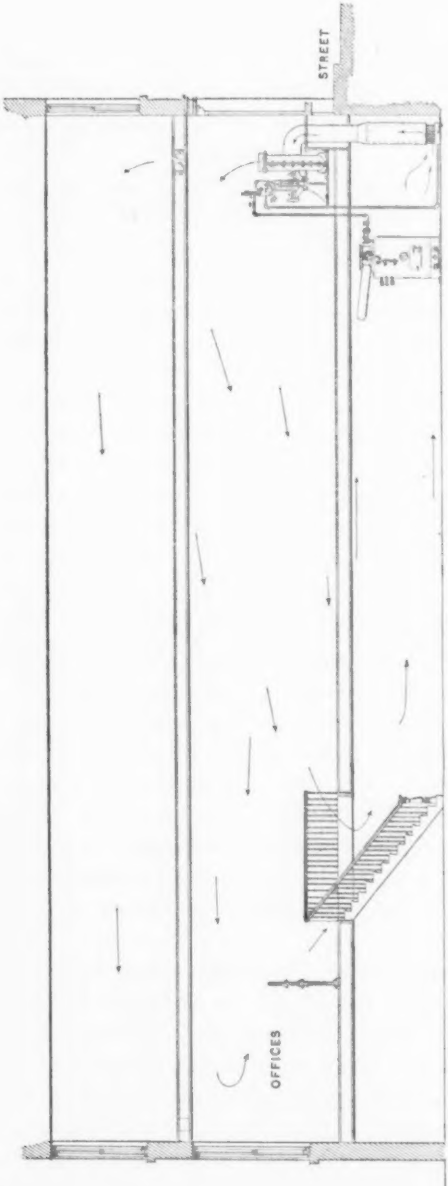

Fig. 6

conducting tube—and the air is used over and over again, enough fresh air coming in through openings around the windows and

through doors constantly being opened. An opening near the bottom of the heater has been provided, and can be used for supplying fresh air when greater ventilation and less heat are required.

The following data are submitted :

Length of store, 95 feet; width of store, 24 feet; height of basement 8 feet 6 inches in the clear; height of first or main floor 13 feet in the clear; height of second floor, 11 feet 6 inches in the clear.*

The side walls are brick, inside walls plastered, and have no openings.

The front is almost wholly of glass, and the large windows are hinged, and open or close like a door. They are not very tight. About three-fourths of the wall surface of the back end is composed of glass; the rest of brick.

The building is five stories and basement, and I only occupy and heat the first and second stories and basement, but I think I could easily heat the whole with my apparatus at a very little increase of cost in fuel.

The engine that drives the fan is three inches in diameter, and has three inches stroke. The wheel in the fan is 36 inches diameter, and $13\frac{3}{4}$ inches wide at the outlet of wheel; the area of discharge of blower 1.76 square feet and the inlet is same size. The heater is about 3 feet wide, 6 feet 6 inches high, and 20 feet deep, and filled with 588 feet of one-inch steam pipe. I am so well satisfied with the results I get from this apparatus, that I have not made any close and accurate experiments of what I can do with it. I know from the cost of my fuel that the expense of heating all I occupy is about the same as I formerly paid when I only heated the offices which were partitioned from floor to ceiling and heated with open grate. I might return the condensed water from the heater to the boiler and make a greater saving. This is not done at present.

Possibly before the meeting of the Society we may have some cold weather, requiring the use of the apparatus, and if this paper produces any discussion by the members, some careful experiments upon its performances may be laid before them ; but at present I can give only the results of one imperfect and incomplete observation, made during December, when the outside temperature was 45°.

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* This floor I only heat occasionally, as it is used principally for the storage of machinery. When necessary to heat it, I open the damper shown directly over the mouth of the blower, and sufficient heated air will be driven through the opening to heat it comfortably in a few minutes.

Temperature of the air on its return and just before entering the heater, 59°.

Temperature of air issuing from the mouth of the blower after passing through the heater, 112°.

Average temperature of air in the room of the main store, on first floor, 75°.

Pressure of steam in the boiler by gauge, 40 lbs.

Revolutions of blower, 113 per minute.

I have made arrangements by which I can measure the evaporation of boiler, condensation by the heater, steam pressure at cylinder, and temperatures at various places in the rooms, but to put them in operation I require another element, which I can only obtain later in the season, viz., cold weather.

I have no apparatus which will give more heat units than contained in the steam, but I do think I have one which will utilize those obtained to the greatest advantage, and one that will work when I want it to, and as I want it to, independent of atmospheric condition even when the winds blow where they list. One self-evident advantage must be apparent from the better distribution of the heat throughout the room, having a much less difference between the temperature at the top of a room similar to the one above described and at the bottom. A room 13 feet high heated with furnaces or radiators will at times be 25° warmer at the top than near the floor. When this air is more uniformly mixed as it is with a blower, a less expenditure for fuel will be the result and more agreeable conditions realized.

DISCUSSION.

Mr. C. J. H. Woodbury.—The method used most extensively in heating mills is that widely known in connection with the work of Mr. Sturtevant, which consists in blowing large quantities of air through a building, passing the air from a blower through an iron space filled with coils of steam pipes, heating it at a temperature not over one hundred and forty, and then by galvanized iron pipes, or if they have put up the building with proper preparations, by flues in the plastered walls, distributing the air over the building, controlling its ejection from the orifices by means of dampers, as may be required. The cost of this method of steam heating is exceedingly satisfactory, because it requires in the first place a very small amount of pipe, relative to the ordinary method of piping a building—something like one-sixth, as a mat-

ter of recollection. Second, it is entirely controllable. It is not necessary to warm portions of the building by radiation from the return heating pipes, &c., where you have no reason for use of the heat in such places. Third, there is no extensive depreciation of the apparatus by freezing of the condensed steam in pipes which have been unskillfully put up, leaving pockets.

I never learned the source of this method of heating, but believe that it antedates any method of heating by direct radiation from steam-heating pipes, and has come into use a second time more successfully by reason of improvements in blowers and engineering skill in its installation.

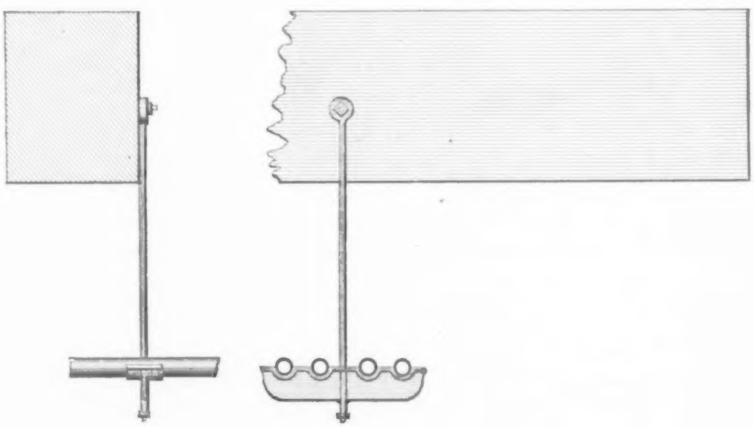

FIG. 74.

The next method of heating buildings which has met with the greatest success is that of the overhead steam-heating pipes. I suppose if the whole history of the art of steam heating of industrial property were ever written in all its truth it would read more like a contest, on account of the lives which it has cost. In former times, when they used very large cast-iron pipes, about six inches in diameter, a single coil was hung about eight or nine feet above the floor, and the heat from above radiated on the top of the head was probably pernicious to health, as it certainly was to comfort, in the highest degree. Then for some reason, whose history I could never learn, the pipes were placed in the usual and conventional manner around the sides of the room, where air could not have easy access of circulation around the pipes, and of later years there is a method of hanging the pipe about three feet

from the ceiling and from the walls of the room in a manner which received at first a great deal of ridicule, but which has been followed by approval in practical work. A mere yoke holds four pipes by means of a half-inch rod in the manner illustrated (Fig. 74), and a vertical steam pipe running up through the building supplies branches running off to each room, as may be required, and thence by separate returns condensation comes back. The opposition to such a method of heating of course is purely conventional. They recall the hypothesis about a room being heated by convection, where the air is represented as rising up in the middle of the room, being cooled at the ceiling and sides and falling down, forming a couple of vertical whirlwinds. Now suppose that we apply the test of direct experiment to that, as was done in the mill at Hartford, where the elevated steam pipes were rubbed with oily waste before steam was let in upon them. The heat of the oil with which they were smeared very rapidly showed, by appeal to both smell and sight, that the smoke was driven off from the pipes filling the room uniformly; that it was following the laws of increase of temperature of the room, which was nearly uniform. I think at a previous meeting of the society I alluded to a letter of inquiry which was written a few years ago upon the subject of heating factories located near Washington and farther south up to the Province of Quebec on then orth and from the Atlantic coast to the western part of New York State, seeking information relative to the present overhead system of heating, and of the forty-two letters received there were forty favorable replies and only two which were unfavorable. The favorable replies of course had different degrees of approval. Those where the approval was partial were, I believe, in every instance, where the rooms were eleven feet or under in height.

Mr. John Walker.—Having had some experience recently in applying the hot air system to our workshops (The Walker Manfg. Co., Cleveland, O.), I would say that we are much pleased with the same; and referring to the paper by Mr. Snell, I have been, therefore, interested in the data given. I find in making a few calculations, that he has about 200 square feet of heating surface in the heater, and 49,000 cubic feet of space in the basement and first floor; this gives about 4 square feet of heating surface in pipes per 1,000 cubic feet of space. When the upper room has to be heated, there is a total of about 75,000 cubic feet in the three rooms; so that with the same 200 square

feet of heating surface, this reduces the square feet of heating pipe in heater to $2\frac{2}{3}$ per 1,000 cubic feet of space, to maintain 75 degrees of heat on first floor. This I should consider a good showing with 40 pounds of steam in the boiler, and no doubt can be accounted for in the case which he presents by the rooms being protected, the sides having no openings at all, and the windows and doors being at the ends of the rooms only.

In designing our plant, we had to consider buildings exposed on every side, and at a considerable distance apart; the north side of all of the buildings being exposed to the cold winds from Canada via Lake Erie. Our foundries have ordinary roofs, with ventilators the entire length. Taking these conditions into account, we thought that probably 10 square feet of heating surface per 1,000 cubic feet of space might be required; we found, however, that 6 square feet of heating surface per 1,000 cubic feet of space was sufficient, with steam at from 70 to 75 pounds pressure per square inch.

No. 1 foundry contains 90,000 cubic feet of space.

No. 2 foundry contains 165,000 cubic feet of space.

Melting house, casting cleaning shop, etc., contain 58,000 cubic feet of space; making a total of 313,000 cubic feet of space.

With these conditions we were enabled to keep the shops at the comfortable temperature ranging from 60 to 75 degrees, according to the severity of the weather.

The benefit to our foundry cannot be too highly appreciated; as the double advantage of heating the shops and displacing the smoke and gases incidental to a foundry was accomplished.*

Mr. W. B. Le Van.—I would like to say a word in regard to the heating and ventilating of buildings by the use of a fan. It is of more importance than is at first thought given to this plan.

In the first place it not only heats the building properly, but also ventilates it perfectly, which other systems do not do, and which I consider a feature of primary importance as regards the heating of buildings.

Mr. H. I. Snell.—This paper having been read in connection with my paper upon "Experiments and Experiences with Blowers,"† there is but little left to reply to, as all the remarks seem to concur with what I have said.

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* See Topical Query 259-39 on removing gas and smoke from shops. Trans., Vol. VIII., p. 697.

† See page 51, of present volume.

I will venture the remark that wherever properly designed for the work, and, except in extreme cases where the arrangements of the rooms are such as to require expensive and extensive piping for the conduction of the heated air, the cost of the apparatus ready for the work is less than the system of direct and indirect radiation.

I have been informed that in the case of the Western Penitentiary of Pennsylvania, at Allegheny City, when the estimate was made for heating by the direct and indirect system, it called for and there was appropriated $53,000. The plan which I have described was adopted instead of the other, and the work and all material was furnished for about $25,000 (or less than one half), giving better results and satisfaction than any other system would give.

Replying to the remarks of Mr. Walker relating to proportion of pipe to the cubic contents of the space to be heated, I will say that in the Women's Homœopathic Hospital in this city (Phila.), I have 2,000 feet of one-inch pipe to heat 250,000 cubic feet of space, and it is doing very satisfactory work in heating, and ventilating as well; this equals one square foot of pipe surface for about 350 cubic feet of space, or less than three square feet for 1,000 cubic feet. I might say in this case the fan is located in a separate building about 100 feet from the hospital, and the air, after being heated to about 135 degrees, is conveyed through an underground brick duct with a loss of only five or six degrees in cold weather.

This system is by no means a new thing, as it was used 14 years ago to heat and ventilate the Boston Rubber Shoe Co.'s works at Malden, Mass., where after heating and ventilating their rooms the waste heat was conducted to the attic rooms and made to do duty as a means of drying their rubber.

CCLXVIII.

STANDARD SECTION LINING.

BY FRANK VAN VLECK, ITHACA, NEW YORK,
(Member of the Society).

FROM a consideration of the lack of uniformity prevailing, and of the positive needs of engineers, it is proposed to bring to the attention of the Society some recommendations which, it may be hoped, will lead to the establishment and general use of standard sections, to be used in all engineering drawings, to represent the conventional sections of iron, steel, brass, lead, etc.

To the modern engineering works, the blue-printing room has become almost as indispensable as the existence of the drafting department itself. This ferroprussiate, or blue-print process, has but gradually worked its way into this elevated position, yet its possibilities and its exactions have well nigh revolutionized many of the old draftsmen's methods. The requirements of the photo-engraving process, being nearly identical with those of the blue-print, have also lent their aid in effecting this result. These changes have been the abandonment of all colors, save black—if that can be called a color. No more is the favorite "wash" spread abroad over the tracing, or those picturesque red or blue dimension or center lines permitted. All must be white or black, with no semi-tones permissible. It is true that a very few still use colors as washes on tracings; but this number is gradually decreasing, as the result is almost always unsatisfactory, for the reason that colors so used give by transmitted light through the transparent tracing different results according to the difference in actinic effect of the colors, and this effect has no direct correspondence with the color used. The density with which the color is laid on has also a most important effect, so that the same color as applied to the tracing may have, at different times, entirely different effects in the blue resulting. The time of printing is also a factor of vast importance. In a print overdone nothing of the wash may be observed, while in a quickly made print it may be very apparent.

As the blue-print must of necessity be in monotone, it is practically useless to seek for sectioned effects by the use of color on the tracing.

Until within very recent times it was never the custom to send out or exchange duplicate copies of drawings to any extent, because such a proceeding usually involved the making of a tracing. Notwithstanding that the execution of the tracing was always relegated to the office boy, yet it always remained an item of considerable expense. Therefore, each shop and drawing-office had its own special methods; nor was it a matter of any moment what these methods might be, as long as they were understood and covered all the local requirements. The introduction of the blue-print process has led to the dissemination of reproduced drawings broadcast throughout the land. Especially is this so in relation to railway work. The office of the head mechanical engineer or superintendent of motive power often originates all the mechanical details of a road or a system of roads; the drawings of these details are then forwarded to all the shops of the company and to shops of affiliated companies. In these cases, of course, it has been necessary to adopt some uniform system of sectioning and linework. The systems adopted have been almost as numerous as the corporations adopting them; and thus, when the drawings of one company were put into the hands of a foreign company, the difference of methods would be very noticeable, sometimes confusingly so.

A well-executed blue-print tracing, being available for the purpose of the photo-engraving process, has led to the multiplication of copies of drawings in the illustrated engineering press. Often these engravings are made directly from the tracings, thus carrying into other establishments any system which they may bear; sometimes they are resectioned and otherwise altered to correspond with the system adopted by a special journal. Here, again, is confusion.

These being the facts, would it not be well for the American Society of Mechanical Engineers to stand as the sponsor of a system of sectioning which should be known throughout the country as the standard formally approved by them?

Would not such a system, if recommended by this body, soon come into very general use, and be the means of saving much annoying confusion, and also yield those many other advantages which the adoption of any standard will insure?

For analogous standard conventionalizations in drawings, might

be mentioned the subject of heraldry. Here a system of lining or sectioning for colors is used which is the admiration of all for its simplicity, its conciseness and its applicability. Not only has it been employed some centuries, but its use is universal. A coat of arms with the field engraved (or sectioned) with its parallel vertical rulings is known at once as the color red, and by all nations—save, perhaps, by democratic Americans, who have a right to glory in their ignorance of monarchical trappings. Why should not the vastly more vital subject of mechanical engineering have as well digested a system of lining as such a purely æsthetic subject as heraldic insignia? Why should we not indicate our irons and steels with as much precision as the colors and tinctures on coats of arms?

In the fields of science, we have the example of the geologists, who are now casting about for some standard method of indicating sections of differing strata on geological profile maps. At the meeting in Berlin of the International Congress of Geologists, plans were proposed for a geological map of Europe, which should employ, for the grand geological divisions, colors and symbols which could be readily accepted by all geologists as a *universal sign language*; * and for this purpose an international color-scale was adopted.

In adopting standards for engineering use, what conditions will best fulfill the purposes for which such standards were designed? In a system of sectioning, what desirable features should be sought and what undesirable ones avoided? The object of a system of sectioning is to make drawings more quickly "readable." If complicated systems are used, or if, as is often the case, the name of the material is written upon a plain section, then a certain, and a by no means inappreciable, amount of mental effort is required before a complete comprehension of the drawing is arrived at. The working drawing then, instead of at once being a diagram of the object, becomes a problem whose details must first be studied before the true mental conception of its construction can take place. We are aware that some large corporations designate either by initials or writing the name of the material sectioned; but, on the whole, would it not be more highly satisfactory to all concerned to adopt systems of sections which, perhaps, would call for a little more labor and time on the part of the draughtsman, but which would be a boon to all who have to read such drawings.

* Report of American Committee of International Congress of Geologists. Trans. of American Association for the Advancement of Science, 1886.

The most useful advantages to be sought in a system of sectioning might be summed as follows:

(1). Ease and rapidity of execution.

(2). Clearness or legibility.

(3). Simplicity.

(4). A good or an even effect.

(5). Adaptability to the material defined.

Ease and rapidity of execution must be secured at all hazards, as in these practical days everything must be reckoned by its cost of production. "Time is money," as well in the draughting-room as in the shop.

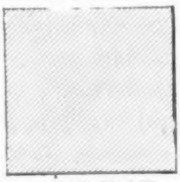
Clearness or legibility may be best secured by adopting sections which may vary greatly from each other, and which, therefore, the eye can easily distinguish at a glance. A prime essential is *simplicity*, as upon this depends not only the neatness of appearance, but also the rapidity with which the section can be put in. A *good* or *an even effect* can be best attained by avoiding, as much as possible, broken lines or dotted lines. From the difficulty of putting them in, it is almost impossible to maintain a series of broken lines all equally spaced. *Adaptability to the material defined.*—This should be so arranged that the material most often recurring should have the simplest method of sectioning; while, if complicated sections must be used, they should only be applied to the rare subjects.

At the suggestion of the Director of the Sibley College of Cornell University, Prof. R. H. Thurston, the accompanying system of sectioning was adopted for the students of mechanical and electrical engineering in that institution. At the outset it may be well to state that no great amount of originality is claimed for the sections here shown, most of them having been variously used.

Referring to the figures (7 to 27), cast iron is represented by plain parallel rulings. This being the simplest and most easily executed of all sections, it is employed for the metal most often occurring. Simple and appropriate as this may appear, yet it seems that a large number of companies employ the section shown by Fig. 8. Why this more difficult section should be used for cast iron, no one seems yet to have satisfactorily explained. Fig. 8 is then reserved for wrought iron. Fig. 9 is steel. It will here be observed that these three sections, which occur oftener than all the rest, are formed by the employment of solid right lines—no broken lines occurring. Many, it is known, use for these metals sections

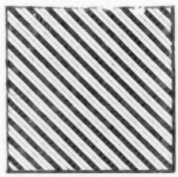
having broken lines in connection with full lines. These may look very nicely, but they are expensive, since it requires at least five or six times as long to ink in a broken line as it does one full one. In

Fig. 7.



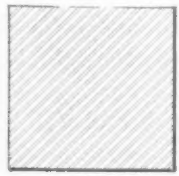
CAST IRON

Fig. 8.



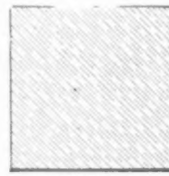
WROUGHT IRON

Fig. 9.



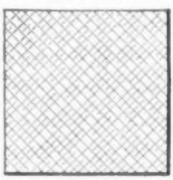
STEEL

Fig. 10.



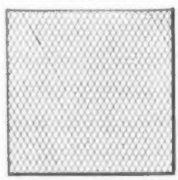
BRASS

Fig. 11.



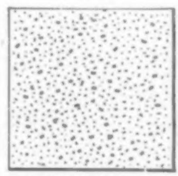
COPPER

Fig. 12.



LEAD OR BABBITT

Fig. 13.

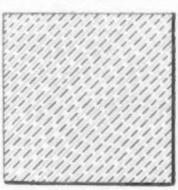


LEATHER

Fig. 14.

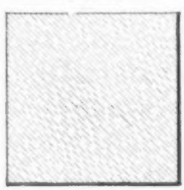
GLASS

Fig. 15.



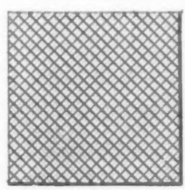
STEEL

Fig. 16.



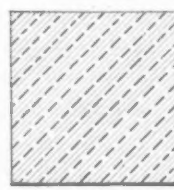
STEEL (Unwin)

Fig. 17.



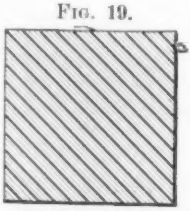
STEEL (German)

Fig. 18.



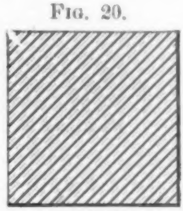
CAST STEEL

Fig. 19.



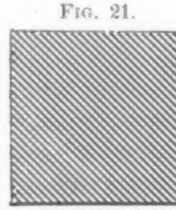
MALL. IRON

Fig. 20.



BRONZE

Fig. 21.



UNDESIGNATED

the other sections, as shown, this, perhaps, is not a matter of so much concern, as these sections are but occasionally used.

Fig. 10, the brass section, has the appearance of being composed entirely of broken lines, but in reality it is made up of alternate full and broken lines. The usual section for brass has been one

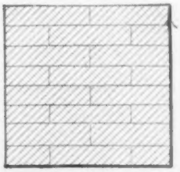
composed entirely of broken lines, but this section as recommended secures, therefore, the same effect with a less expenditure of labor. Copper and brass are naturally related and so should be their sections, but as copper is not in so general use as brass, it is then not necessary that its section should be as simple. Therefore, the copper section adopted and shown in Fig. 11 is the same as brass with a cross ruling of full lines. Lead or babbitt is represented by a sectioning which is already in very general use for that purpose. It consists of full lines cross-hatched, preferably at an acute angle, as shown in Fig. 12. Fig. 13 is in common use for leather and packing sections.

The demands of electrical draftsmen have also brought into use two new sections which are coming to be more or less recognized. One of these is a sectioning to represent such an insulating material as vulcanite, vulcanized fiber, etc.; the other is a form suggesting a section through a coil of wires, as for instance a section through an electro-magnet coil.

In Fig. 24 are shown two methods of sectioning through wood. It is often desirable to indicate approximately the direction of grain. In this case it can be readily accomplished as represented.

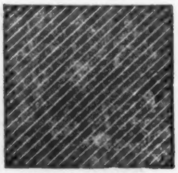
In order to observe what sections have been used and proposed from time to time, a number are shown in Figs. 15 to 21. Fig. 15 has been very commonly used for steel, but it has a grave defect in

FIG. 22.



BRICK

FIG. 23.

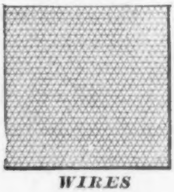


VULCANITE

FIG. 24.

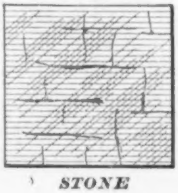
WOOD

FIG. 25.



WIRES

FIG. 26.



STONE

FIG. 27.

EARTH

that it uses broken lines. No. 16 is used by Unwin* for steel. It is open to the same objection, only intensified. No. 17, also used for steel by the Germans, is very pretty, but the similarity to a Scottish plaid could be better exchanged for a section which would consume only half the time to draw. No. 18 has been recommended for cast steel.† Whether it is necessary to have a separate designation for cast steel remains a question, especially as the term cast steel is in many quarters rather vague. No. 19 for malleable iron and No. 20 for bronze have been recommended, but whether there is a real need for them remains to be seen. No. 21 is simply a suggestion, not yet assigned to any material, but having the advantage of being composed entirely of full lines.

DISCUSSION.

Prof. F. R. Hutton.—I am much interested in the subject of this paper, and hope that its presentation will result in some recommendations by the Society as embodying their experience and preferences.

I am well aware of the arguments which can be urged against the use in the drawing-room of a busy shop of any conventional system of section lining. The best of these is the necessity of having the men who are to read the drawings in the shop educated also to the use of the standard section lining, and the difficulties from misunderstanding such ruling as put down. The other objection comes from those whose drawing-room is short-handed, and who think that draftsmen take too long at a job any way, and if all sections had to be hatched throughout to be understood, the difficulty would be so much the worse. Objectors on either ground adopt the plan of writing in the name of the material upon the drawing at points enough to make error unlikely.

These objections are both valid as far as they go, and are good reasons why the engineer advancing them should not adopt a standard system for his particular shop; but they do not touch the question as to the desirability of the existence of a set of standards to be used by such as prefer the use of this dialect for drawings, nor the usefulness of such common standards for other purposes than shop drawings. There will always be a larger number of draftsmen and offices who will use the system of lining sections, and they certainly will be the gainers by uniformity; but the particular point which I

* Elements of Machine Design, p. 16.

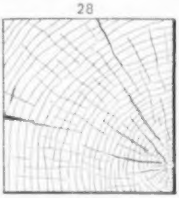
† *The Master Mechanic*, April, 1887.

wish to urge is the advantages in the matter of illustrations of engineering papers and technical literature generally which would follow from the adoption of a system authenticated by those most competent to suggest one, and then generally adhered to. The illustrations in the journals and other publications must be lined for printing, and the method of printing in the name of the material is here either not applicable at all or else is costly out of all proportion, in these days when ruling is done in engravers' establishments by a machine with an adjustable feed motion to produce the spacing of parallel lines. The use of such machines is moreover an added argument against the adoption of broken lines in any conventional section, since the unbroken line is made here also at much higher speed. Fig. 38, made up altogether of broken lines, and produced by such a machine, is an excellent commentary upon this point. The white places are accidental, but have arranged themselves in what appears to the eye like an attempt at a pattern. If the members of this Society, agreeing to follow a certain set of standards, would use them in the illustration of their contributed papers for the Transactions, I am sure that it would be found of advantage sufficient to pay for the little extra trouble it might cost.

Hoping that this paper and its discussion may result in the appointment of a committee of the Society to look into this matter, I took the trouble to write to the nearest technical schools, requesting the officer in interest to send me a reproduction of the standards in use there for presentation in this discussion. Ten years ago, when I first took charge of the details of the construction drawing at Columbia School of Mines, I compiled the series of conventional hatchings which are shown in Figs. 28 to 43. The hatchings were taken from available sources in technical literature at that time, and some which were not found anywhere were originated. We have used them ever since, finding, however, the faults and objections to them which are obvious, and which have been mentioned by the author of the paper. The ruling appears finer in the cuts than we use it, because the originals furnished to the engraver were slightly reduced to make them the same size as those presented by the author of the paper. Our students are never allowed to work so fine as this, since we try to have them use a coarseness which the time-pressure of after life will admit of their adhering to. Several of the other technical schools addressed use no conventional system, but hope to see suggestions made which will result in proposing one. The Massachusetts Institute of Technology uses very much

the same system as at Cornell, publishing them in a pamphlet with other notes on the execution of drawings according to their standard.

I shall be very glad if from all the engineers and manufacturers

28

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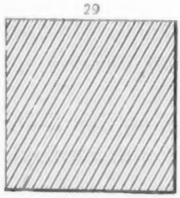
29

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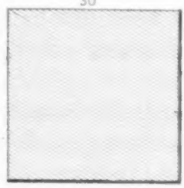
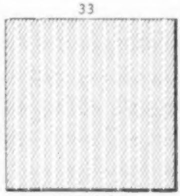

FIRE BRICK

31

BRICK

32

CAST IRON

33

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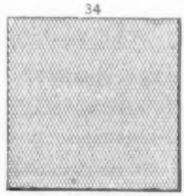
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COPPER

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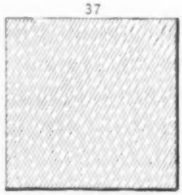
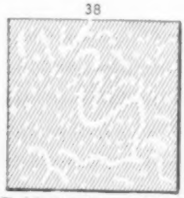
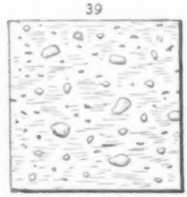
SLATE

37

BRASS

38

BABBITT METAL

39

CONCRETE

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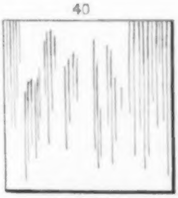
GLASS

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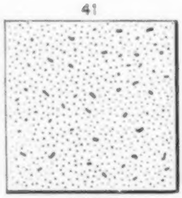
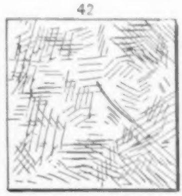

SAND

42

EARTH

43

WATER

who may attend this meeting additional contributions of the sections in use in their shops can be given in the form of tracings or blue-prints, which can be considered as part of the discussion of this topic, and used by the Society's committee (if appointed) in deciding upon the standards to recommend.

It would be a very great help to the technical schools if its officers could say to students that the system which they would present has the recommendation of such an organization as this; and the adoption of a set of standards by the schools, by agreement, would go far on the other hand to secure its general introduction into practice when their graduates came thereafter into the active work of the profession.

Mr. J. J. Grant.—The matter of section lining drawings is one which interests both the employer and employee, from the head draftsman down to the apprentice.

That some standard system of section lining or symbols is badly wanted is evident to any one working from drawings made by different draftsmen, and that it should be as simple as possible is equally evident. The day for elaborate drawings, especially in machine shops, is fast drawing to a close, and in my opinion any system upon which this society puts its stamp of approval should have the element of simplicity in it. There are many of us who have spent days in section lining a drawing when such time might be entirely done away with by some simple letter or symbol, easily understood by any one, speaking either English or a foreign tongue.

Every one would like a standard system of drawing, but what might be called embellishments should be such as can be quickly done, so that in any small establishment, where a man is even at times not only boss but also all hands, he can afford to make his drawings to conform to the standard.

Mr. H. R. Towne.—The last speaker has correctly stated that this subject interests both employers and employees equally. It is important to adopt some standard of this kind which shall make drawings intelligible to the workmen in our shops, and such that a workman, in passing from one shop to another, will not have to learn a new language each time that he changes. The average drawing usually requires, in some places at least, that the character of the material shall be indicated by letters. Many views on a drawing show parts not in section but in elevation, and therefore symbols have to be used. In the establishment under my charge we have already adopted the standard sections which Prof. Thurston has introduced here to-night. We have been using them for some months with great satisfaction. But in connection with them we are also using a system of symbol letters which were briefly described in a paper that I read at the Pittsburgh

meeting several years ago on the subject of a drawing office system.* The letters are as brief as possible, one initial in some cases—never more than two—indicating each of the different materials employed. I would suggest, therefore, if a committee is appointed on this topic and should make a report, that they recommend that in any standard sheet representing the sections proposed there should be printed in the middle of each diagram a symbol, or letter or letters, indicative of the material, so that the two may become known together; so, also, that whenever either is used the other is naturally suggested to the mind, and that each conveys the same meaning. I think that both are required; that the use of section-lining alone does not fully cover the ground, and that symbols should be considered by the committee as well as section-linings. I hope that a committee will be appointed for this purpose as has been suggested.

Mr. J. H. Cooper.—There are many sheets of drawings made without a particle of section-lining, and the material of these must be named in words. It is quite impossible to have a distinctive section-lining for every material. There are too many at present, and the number is increasing. Moreover, the woods and stones of different countries differ so much, as do also the measures, that any international uniformity of delineation must be very complex.

Of several mechanical methods accomplishing any purpose equally well the cheapest is taken, and we may say the same of methods of drawing. If a system does not aid work, it should be discarded.

There is no question about the importance of representative section lining in certain kinds of drawings, and those shown in figures 28 to 43 in Mr. Hutton's discussion of the paper are appropriate and elegant. But the one voice from the constructive department is, "Put things down so plainly that everybody can clearly understand all about them." In the interest of drawing-room economy and machine-shop certainty, I would suggest that section lining be used to show *sections only*, and that the material be named in words.

Prof. F. N. Willson.—I look upon this matter from about the same standpoint as Prof. Hutton, and believe it very desirable that in this, as in everything else, we who are engaged in teaching should feel that we have back of the standards which we set

* Trans. A. S. M. E., Vol. V., page 200.

before our students the sanction and practice of a large portion of the engineering profession. Observing considerable disparity in the various systems of sections which have come under my notice, I have been familiarizing my classes at Princeton with the standard sections adopted several years ago by the Pennsylvania Railroad. If systems of section lining are indispensable to the profession, it certainly would seem most desirable that some one system should be agreed upon and universally adopted, and if a committee is appointed to examine into the matter I would call its especial attention to the following points: First, that the recommendations of both Mr. Van Vleck and Mr. Hutton regarding the use of the continuous line can very easily be carried out for the representation of the ordinary metals, also of glass and of water, by simply selecting from the two systems already presented and that of the Penn. R. R.,—only one or two new designs in addition being required. Second, that the kind of section lining heretofore in general use to indicate the *fact* that the piece represented is in section, should not, as in the various systems now in use, be made the symbol of any particular material.

Mr. F. A. Halsey.—I rise to ask what this is all for. I have looked the matter over carefully, and am unable to find any reason for these conventional lines. Consider what they involve. They must be memorized by principals, draftsmen, and workmen, and then the draftsmen must consume time in using them, and the workmen in deciphering them. When it is all done, the result is imperfect. None of these systems provides for one-half of the materials in every-day use. Consider the infinite number of materials which go by the name of steel, and all the mysterious compounds which come from the brass foundry—and yet we have here only one way for designating steel and one other for brass.

In point of fact, however, the workmen never do learn these systems, and consequently the draftsman prepares a key which he posts up around the shop—or perhaps pastes on the corner of each drawing—and the workmen diligently compare the drawings with the key to learn what the drawings mean. It seems to me that it would be just as reasonable to call for materials in a foreign language, and then send the workmen to a dictionary for the meaning of the words, as to call for them by these conventional lines and then send them to the key for the meaning of the lines. We could submit to all this if it were necessary; but it is not. There is another convention already at hand, which every-

body understands, and which is comprehensive enough to cover all possible materials. It is to simply call things by their names and have done with it. When I want to specify that a certain piece is to be of steel, I letter on the drawing of the piece "Steel." In the same way, to call for brass, I say "Brass." If some particular kind of steel or brass is wanted I say so, and that is the end of it. I would like some one to say why this simple and obvious method does not answer all possible requirements.

I believe that I place as high an estimate on the value of system in work as any one, but this is a case of system run mad—of system for the sake of system, and not for what it will accomplish To be worth anything, a system must save more than it costs; must relieve one's burdens and not add to them; and under this test this system fails. I take it, however, that the majority of the members use some such system as this, and the presumption is that they have some reason for it, and so I will close as I began, by asking what it is all for.

Mr. John Coffin.—In proposing a system of this kind, if a committee be appointed to review the matter, I hope they will have some consideration for the draftsman. Now, after a draftsman has worked for quite a while and studied out the construction which he wants, and is resting himself a little by putting in some of the little details, he comes to a complicated system of section lining. I must say that there is nothing which tires me quite as much as putting in section lines. If different materials are to be indicated by dashes interrupted by dots at certain times, something like the Morse telegraph system, it would be very tiresome. If by continuous lines, I do not see how we would make them fit the cases unless we made them of different inclinations or differently spaced. If they are to be determined by the spacing, I find it very difficult to go over a large section and not get the spacing finer on one side than on the other. If they are to be determined by the directions of the lines, I do not see what we are going to do with the fellow on some other side of the drawing. We might hear him say: "Dick, what is that—brass or steel? I am left-handed on this side." (Laughter.) There is nothing easier to put on a drawing than s-t-e-e-l or b-r-a-s-s, to indicate the material; and when it comes to foreign languages, I do not see but what it is just as easy to put on a little key to the language, a sort of dictionary, as it is to interpret the section system. I remember having an old dictionary in which the flags of

the different nations were represented—the colors—by peculiar lines running in different directions, and I think I turned to the marginal reference to figure out what colors the flags ought to be. I thought it a great satisfaction when they got to putting a colored print in the dictionary, because you know not only what the color is, but you know what the flag looks like.

Mr. H. R. Towne.—In order to bring this method before the society, as apparently contemplated from what has been said, I will move that a committee be appointed by the chair, before the close of this session of the Society, to consider the subject and report at our next meeting; and if I may ask for a moment's further use of the floor, I would like to comment on one point which has been raised in the discussion, which is this—that mere lining is not intelligible to many persons and that it is better, therefore, to write out the word. I think a fair parallel to that is this fact, which we all know, that the old style of drawing, with colored sections, neutral tint for cast iron, blue for wrought iron, and yellow for brass, was perfectly intelligible to all of us. We never wanted any lettering upon it. We always recognized what was meant at a glance. The use of this proposed system of section linings will indicate just as quickly and just as accurately to us what each section means, when we become used to it, as the old system of colors did, and a parallel to that is the habit that we all have now of recognizing pictures which are in line or form only and are without color. The invention of photography and the art of engraving have long familiarized all of us with the reading of pictures which are simply in black and white, without any color, and yet the eye and mind very quickly add to them the significance of color, being guided by long habit and practice. Now, the same thing will result from the adoption of a system of section lining of this kind, in which, without the need of referring to color at all in the mental processes, we will by glancing at the drawing obtain quickly a correct understanding of the different metals represented, just as well as we used to do from the colored and tinted drawings which we had before the invention of the art of blue printing. Certainly something of this kind must be done in order to make the blue print as quickly read and as intelligible to us as the old colored tracing was. I trust that a committee will be appointed for this purpose, and I am sure that the object of its appointment is a useful one to all of us.

[The motion was seconded.]

The President.—Has any one any remarks to make on the subject of the appointment of this committee?

Mr. Jas. B. Ladd.—I would suggest that the first duty of the committee be to ascertain whether or not such section lining is desirable. I side entirely with the speakers who urged that such a thing is entirely undesirable. It is extra work all around and not what we need. I think we should pass first on the question whether we are going to have a standard section lining before we consider what it shall be.

Mr. John E. Sweet.—I should oppose the appointment of this committee for this reason, that this Society has appointed a number of committees, and in certain cases the committees have spent a good deal of their time in preparing their reports, and after they had made their reports the Society has refused to adopt them, or even to recommend them. It seems to me it is imposing a task upon men to ask them to act on committees and then to refuse to profit by the reports after they are made.

Prof. James E. Denton.—I should vote against this committee on the ground that it proposes to devote itself to a refinement upon a question which every one will be able to decide for himself in a way which will secure all the practical ends that a drawing is intended to secure. I think that it is too trivial a matter for this society to put its time upon in the manner proposed. (Applause.)

Mr. Wm. Kent.—It may be very well for the professors in the colleges who are teaching drawing to agree among themselves as to a standard lining which they shall teach their students, just the same as students are now taught the metric system although they may never be expected to use it. It may be well that there should be such a uniform system among the colleges. But I think that the mechanical engineers in general will disapprove of any standard so far as putting it into practice is concerned. I think in general, managers of work have to educate the draftsmen to come down from the refinements they get in colleges and to teach them to make a drawing with a piece of chalk on the door of a blacksmith's shop or in the dust on the floor with a finger. I think this question had better be referred to the professors in the various colleges, and not taken up by a committee in the Society.

Mr. H. R. Towne.—So much has been said already on the question of a vote for the appointment of a committee for a purpose of this kind that I feel justified in asking for a moment's more time in which to put the matter straight before the meeting.

My friend Prof. Sweet has recalled a fact which occurred at our meeting in Atlantic City, if I remember correctly, at which the Society voted against putting itself on record as indorsing the report of a committee in accepting it, and allowing it to go out as the standard of the American Society of Mechanical Engineers.* I was one of those who opposed the action that was proposed at that time and voted in the negative, and I have never regretted it. I think that is the proper position for the Society to take—that we do not, as a body, commit ourselves to the indorsement of any report of this kind, and therefore do not authorize a standard, such as here proposed, to go out under the name of the standard of the American Society of Mechanical Engineers. But that is no reason why a committee should not be appointed, as has been done in many other cases, to consider some special subject of interest to the members of the Society, and make a report thereon. The work of such a committee will of course be better digested and considered than any discussion at a meeting of this kind, and the approval of the members present at the meeting when such a report is submitted is expressed by the vote then taken. That however is a very different matter from what was proposed at the Atlantic City meeting, which was that the report should be adopted and the matter in question go forth to the world as the official action of the Society and as its "standard" in the particular thing referred to. If Prof. Sweet will recall the facts, I think he will see that there is quite a difference between what was then proposed and what is proposed here to-night.

Mr. Wm. Kent.—I happened to be chairman of that committee at Atlantic City on which the discussion took place whether their report should be adopted as the opinion of the Society or not. The verdict of the Society at that meeting was understood as establishing a precedent that the Society would not at any time indorse as its "standard" any committee's report on any subject. I think it was said at that time that it was following the precedent established by the Institute of Mining Engineers ten years before, where some one moved that a certain report be accepted and that the society commit itself to a certain opinion. The president declared the question out of order, saying that the society did not exist for the purpose of having any opinion on any subject as a society. An appeal was taken and the chair was sustained. And that view has always been sustained in that soci-

* Trans. A. S. M. E., Vol. VI., page 877.

ety ever since. As we adopted that plan two years ago, I think it well to stick to it. This, however, I consider as a very different thing from a decision that no matter shall ever be referred to a committee for consideration and report. The report in any case carries the weight which is given to it by the authority of the members who prepare it and the care taken in its preparation, and is none the less valuable because the Society does not deem it wise or politic to indorse it formally as its official opinion.

Mr. Jno. T. Hawkins.—Without touching upon the merits of this subject, I would like to say a word or two about appointing a committee, before we get to that disagreeable stage, as we may do, of hearing an emphatic "No" on this question. I would like to take ground against the position of Prof. Sweet. I hope to see no precedent established in this Society to the effect that we must necessarily be expected to indorse the work of a committee. But on the other hand I should regret a policy or practice that no committees should be appointed because their action possibly would not be sustained or approved of by a majority of the Society. I think the appointment of committees a very desirable means of carrying out discussions of this kind. They have the opportunity to discuss a subject much more fully than we have here in open meeting, and they will generally be men experienced in the particular matter at issue; in fact, we all know that the time which can be given to these discussions in open meeting is much too short properly to exhaust the subjects.

I hope the understanding in appointing committees will be that such action will lead to a better and more thorough discussion of a subject brought before the Society; that a committee should not feel aggrieved if not indorsed by the Society. I think it is wrong to look at it in that light. It appears to me that the work of our committees should be very much like that of Congressional committees; they can more deliberately and thoroughly exhaust a subject than can be done in open meeting.

[The motion to appoint the proposed committee was then put and lost.]

CCLXIX.

ON THE DIVERGENCIES IN FLANGE DIAMETERS OF PUMPS, VALVES, &c., OF DIFFERENT MAKERS.

BY PERCY A. SANGUINETTI, PHILADELPHIA, PA.

(Member of the Society.)

THE object of this paper is to call the attention of the Society to the divergencies existing in the flange diameters of globe and gate valves of different makers, as well as incidentally to touch upon similar characteristics in the flanges of pumps, steam engines and cast iron pipes. The object primarily sought is the taking of such action by the Society as will result in the adoption of a uniform standard by the manufacturers.

In order to show at a glance the divergencies which are referred to, a table has been prepared in which the figures were compiled by myself from printed catalogues issued by the manufacturers, except in one instance where the information was obtained for me. A few dimensions have been also added, taken from drawings of pumps in which divergencies occur. The table might have been more voluminous, had I cared to communicate with other makers than those quoted, but I feared that a consultation with them beforehand might have precipitated a discussion prejudicial to the interests of the Society; so that the list was prepared only from data within immediate reach. I think enough has been given, however, to establish the fact that divergencies exist.

These differences are so slight that very little effort on the part of the Society should be necessary to prevail upon the manufacturers to agree upon the adoption of a uniform standard. The advantages which would arise from such agreement to the users of their products are no doubt too well known to call for any decided advocacy here; it will suffice if I illustrate one or two cases where a want of uniformity would occasion trouble and unnecessary expense. In designing a large pumping plant, the suction and discharge flanges of the pumps selected would under present conditions govern the diameters of all other flanges, as being the most important in the matter of size and expense of pattern making.

After receiving the drawings from the pump men, catalogues must be consulted to see if the flanges of gates, checks, regulating valves and pipes will agree; and if they don't, which is almost a foregone conclusion, steps must be taken to make them agree. Then if the valves which we want to use require flanges larger or smaller than the catalogue dimensions, we could not take them from the maker's stock, but must have them made specially, not only occasioning delay provided the pumps were ready, but if larger, with the chance; sometimes amounting to certainty, of having the following items in the bill: "To extra pattern work, so much;" "to material for same, so much;" "to extra facing of flanges, so much." This is reasonable and business-like under existing circumstances, and I don't think there is an establishment, either private or municipal, which would object to pay for these extra charges; not so much for uniformity of appearance, perhaps, though this is in itself a source of gratification, but in many cases to save the expense of shrinking wrought-iron bands over the smaller flanges on account of the bolt holes in the flanges to which they may be joined having been drilled too near the edge. This is often resorted to where changes are made, either in the substitution of one kind of valve for another, or in a transposition of the pipe system. On some occasions, when time allows, new pieces of pipe with flanges of different diameters at each end are ordered, all of which need not be necessary, and is due to the want of a uniform and universal standard.

I think the pump and valve makers, and perhaps the engine builders (as the latter usually furnish their own stop valves), could together control the working of a uniform standard, as the flange diameters of these articles, so to speak, are governed by somewhat similar conditions, namely, the thickness of metal contiguous to the inlet and outlet orifices, which have to conform pretty nearly to the thickness of main body on account of unequal contraction. This being conceded to be of sufficient importance to give them the lead, it would be a trifling matter to influence the manufacturers of pipes, as they are so often entrusted with orders embracing every range of flange diameters that they would, I am convinced, be very glad to know that their patterns were to be unchanged in this particular. Even in the matter of bolt circles the start is given by the pump men, who invariably ship their goods with studs screwed in the flanges, or holes for bolts ready drilled or tapped.

In ordering pipes with flanges to fit those of pumps or valves,

what at first looks like a serious difficulty presents itself; for while the thickness of valves of good makers agrees closely with that of pumps, it often happens that pipes are wanted of varying thicknesses, to suit different pressures and kinds of service; so that in a thin pipe an unnecessary excess of flange diameter would seem like bad practice. But the variations in thickness of pipes in ordinary use are so slight, being not more than from $\frac{1}{4}''$ to $\frac{1}{2}''$ in pipes of the same internal diameter, that a large fillet or thickening rim fills up any lost space between the outside of pipe and bolt circle; and this is of importance, too, in other respects, as in cases which have come under my notice of the bursting of pipes, the fractures have invariably been at the junction of flanges with the body, which is a good reason in itself for the adoption of generous fillets.

It is not the purpose of this paper to go into the question of bolt circles, or number and sizes of bolts, not because I do not consider these important branches, but the subject is one involving an inquiry into the strength of material and the relative proportion of parts—an inquiry of so much interest that I did not wish to limit it to a necessarily condensed notice here. Besides, until official action has been taken by this Society, we must be content to adopt the proportions given by the pump makers, or else have two or more pitch circles of bolts in the pipe system, which means two or more templates, and the uncertainty afterward as to which was applied at a particular point. As to the correctness of any one system of flange diameter or of bolt circle, I do not wish now to assume the responsibility of determining; it appears to have been done in England by Box, whose proportions are given in the table. He says in his little work on hydraulics that they are the best proportions, an assumption that we might readily tolerate from such a distinguished and careful writer; but even he is forced to admit that they differ from those adopted by many makers.

In these busy times, whatever saves unnecessary thought is of value, and it would, I think, add something to our convenience and saving of expense if we could order our pumps, valves, checks and pipes, and for that matter our steam engines and governors—all necessarily from different makers—with a certainty that their flanges of similar outlets will at least correspond in diameter. If we are once assured of that, an important advance will have been made, and will greatly pave the way to the establishment of uniformity in the diameter of bolt circles and in the number and sizes of bolts.

TABLE I.—NAMES OF MANUFACTURERS OF VALVES.

FLANGE DIAMETERS IN INCHES.

Size of Valves, Inches.	Nelson & Finkle.	Chapman.	Belfield.	Jenkins' Valves.	Jenkins' Gates.	Morris, Tasker & Co.	Eddy.	Ludlow.	Flanges of Pipes. Thomas Box (English).
3	8	7	7½			7½	8	8	6½
3½	8½	8½	8	9	8½	8½	8½	8½	
4	9	9	9	10	9	9	9	9	8
5	11	10	10	11	10	10	10	10	9¼
6	12	11	11	12	11	11	11	11	10½
7		12	13	13	12		12	12	12
8	14	13	14	14	13	13½	13	13	13¼
10	16	16	16	16	16	16	16	16	16
12	19	18	19	19	18	19	18	18	18¼
14	20	21	22				21		
16	23½	23	24			23½	23		
18		25	26				25		
20	27	27				28½	27		
24	31								
30									
36									

PUMP MANUFACTURERS.

Size of outlets. Inches.	Knowles.	Guild & Garrison.
5	Flange diameter 10″	Flange diameter 11″
6	" " 11″ and 12″	14
8	" " 13½″	
16	" " 23½	

DISCUSSION.

Mr. W. O. Webber.—I have had in mind for some time the broaching of this subject, which Mr. Sanguinetti has so ably introduced, and I think that the question of bolt circles, number and diameter of bolts should be also considered at the same time. In our centrifugal pump work, after looking over all the valve and pipe makers' standards, we adopted for flange diameters those used by the Chapman Valve Co., and for diameter of bolt circles,

number and size of bolts, we copied from the National Tube Works as nearly as possible.

To illustrate the divergence in flange diameters, a prominent manufacturer in Boston has to carry the following range differing by half inches:

Size of Pipe.	Flanges from
½″	4 ″ to 8½″ O. D.
¾″	4 ″ to 8½″ O. D.
1 ″	4 ″ to 9 ″ O. D.
1¼″	4 ″ to 10 ″ O. D.
1½″	4½″ to 10 ″ O. D.
2 ″	5 ″ to 11 ″ O. D.
2½″	5½″ to 12 ″ O. D.
3 ″	6 ″ to 14 ″ O. D.
3½″	6½″ to 14 ″ O. D.
4 ″	7½″ to 14 ″ O. D.
5 ″	9 ″ to 14 ″ O. D.
6 ″	9½″ to 14 ″ O. D.
7 ″	11 ″ to 16 ″ O. D.
8 ″	12 ″ to 16 ″ O. D.
10 ″	14 ″ to 16 ″ O. D.

and the styles of drilling vary nearly as much.

If there was some uniform standard, we could send our pumps as we do from Maine to California, Montreal to Chili, and be sure that our pumps, strainers, foot and flap valves all would fit and come out right; as it is now, it is a constant source of annoyance and worry to have to look out for these differences, where the pipe, as it almost always is, is supplied by different parties from those supplying the pumps and valves.

I would therefore submit a motion that the president of this Society appoint a special committee to investigate thoroughly this subject and report, together with recommendations for a standard diameter of pipe, pump and valve flanges, diameter of circle of bolts, number and size of bolts, to be used for the different classes of light, medium, and heavy service, to be adopted and recommended as the A. S. M. E. standards.

Mr. F. W. Taylor.—I would suggest that before it is decided to adopt any one standard series of dimensions for flanges, either in thickness, or diameter, or for bolt circles, it is first necessary to adopt a common standard of packing to be used in the joints, since if you use a packing which consists of a narrow ring of rubber or metal which comes close to the inside diameter of the cylinder, or pipe, on the end of which you propose to make a joint—

that is if you use a ring of packing which is entirely inside of the bolt circle—it is necessary to have a very much thicker flange than if you use a packing which covers the flange entirely. Therefore, the thickness of the flange would be dependent upon the width of the joints which you are going to make between the two parts which you wish to connect.

Then again the diameter of the bolt circle will depend upon the manner in which the flange is fastened to the pipe—that is, whether it is riveted or screwed to the pipe, or cast solid on it.

Therefore, I cannot see that it is possible to establish a uniform standard for the dimensions of flanges, unless a uniform method of packing the joint and a uniform method of attaching the flange to the pipe be adopted, which I think it would be very difficult to do.

Mr. H. R. Towne.—A good many years ago, having constantly considerable pipe work to plan, I had occasion to consider this question, and taking the average practice as I found it at that time, I put it into this set of formulæ below. The diagram (Fig. 80) and the formulæ under it are figures used for brass flanges for copper pipe in ship work, the range of sizes being from pipe of $1\frac{1}{2}$ inches to 12 inches diameter, the same flange, practically, being used, with a little difference in the bore of the center and in the rounding of the corners, whether the flange is loose and simply backs up the flange turned on the pipe to form the joint, or whether the brass flange is brazed to the copper pipe in order to make the joint. The other figure (Fig. 81) represents cast iron flanges used on ordinary gas and steam pipe. I have no doubt but what these figures can be improved upon after further consideration of the subject; but I know, from considerable experience in the use of them, that they are approximately right and can be used safely, and that having a formula of this kind saves considerable trouble and time in planning fittings.

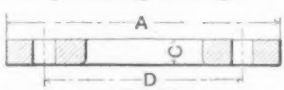


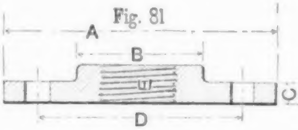
Fig. 80

$\triangle$ = Diameter of pipe.

$$\left.\begin{aligned} A &= 1.21\,\triangle + 3 \\ D &= 1.14\,\triangle + 1.82 \\ C &= \frac{\triangle + 16.42}{35} \end{aligned}\right\}\ \text{All dimensions in inches.}$$

Cast iron flanges for wrought iron pipe:

Fig. 81



$\triangle$ = Diameter of pipe.

$$\left.\begin{array}{l} A = 1.5\ \triangle + 3 \\ B = 1.262\ \triangle + .678 \\ C = .097\ \triangle + .35 \\ D = 1.38\ \triangle + 2 \\ E = .16\ \triangle + .47 \end{array}\right\} \text{All dimensions in inches.}$$

Mr. W. B. Le Van.—The proposer of this resolution forgets that every pump maker, when he sends out his pump, adopts such form and size as will conform to his style of pump. And again the maker also has an eye to business in regard to future wants, and expects to furnish any duplicates which may be wanted.

Mr. L. G. Engel.—There is one point which seems to have been lost sight of, and that is that we have to deal not only with cast-iron pipe but with copper and other pipe. If we adopt a standard flange for pipe, we must necessarily adopt a standard flange for each kind of pipe, because no one would put the same kind of flange on a very thin copper pipe which he would put on a cast-iron pipe.

In regard to light, medium, and heavy pressures as affecting bolt arrangements, I would remind those present of the fact that in every large establishment T's are used indiscriminately for water and steam. That is, they usually adopt some standard T. For instance, if we take an ordinary six-inch cast-iron T with a twelve-inch flange and adopt six holes, which would be plenty for all ordinary purposes where water is concerned, we might find that the six holes were not sufficient for steam pressure, although the steam pressure might not exceed the actual water pressure. If we double the number of holes for the T for steam purposes, as the only way in which it can remain interchangeable with the six-hole T, in case they may be subsequently used together on the same line of pipe, we get so many holes that it is difficult to screw up the bolts. Then the divergence that Mr. Webber has

spoken about in flanges is no doubt due in some degree to the fact that many people join iron pipes to copper pipes, and join large pipes to small pipes, and in that way it is necessary to carry in stock such a divergence in diameters.

Mr. A. H. Raynal.—There certainly is a great convenience in having a standard for diameter of pipe flanges and the number and position of bolt holes, even while there may be occasion at times to deviate from it for special purposes. I have for years worked to such a standard, and find it perfectly applicable in the ordinary routine practice of shops doing a variety of pipe work.

In giving out the detail orders for a complicated line of cast-iron pipe—for instance, with elbows and tees, leading off in different planes—I found it very convenient to mark on the order for each piece how its flange should be drilled. I would mark at each flange "W. S." or "A. S.," meaning thereby that the line passing through two opposite bolt holes should lie either *against* or *with* the seam of the pipe, and that would determine absolutely the proper drilling. The foremen would readily learn and appreciate the system.

There is no necessity for manufacturers of pumps and valves to vary largely from each other in the dimensions of their flanges. I am very much in favor of having this Society propose such a standard, and although we were unlucky in this respect on a previous occasion, would second the motion that a committee be appointed this evening to investigate the subject and propose at our next meeting a standard for the diameter, number and position of bolts in flanges for cast and wrought iron pipe.

Mr. Wm. Kent.—I rise just because I partly opposed the appointment of the last committee, to say that I am in favor of the appointment of this one. I think that the objection raised by Mr. Le Van to the standardizing of flanges held as well with the Whitworth threads in England and the Sellers thread in this country; namely, that the manufacturers did not want to adopt the standard thread because they wished to furnish the repairs. But in spite of the opposition, which was a strong one, standard threads have come into use, and standards for pipe threads have also come into use, thanks to the report of our Society's committee on pipe threads a year ago.

Mr. W. F. Mattes.—I fully agree with the proposition to have

this committee appointed, and I agree with it on the basis of my own experience. In the operation of a drafting room for a large manufacturing concern, where a good deal of pipe and a great many flanges have to be turned out, it saves answering a great many questions to have a standard to work to. Now it is impossible to have a standard which shall never be deviated from in such work as this; but in the vast majority of cases we can work to a standard. I had quite a difficult task in trying to adopt a standard to works which for more than forty years never had one. We had a great variety of pipe made according to the pattern maker's notions at the time, and we had to fit to them as best we could, and at the same time take counsel of the different valve makers' catalogues, and differ from them as little as possible. But we took a happy medium; we corrected such errors as we could, and the result, for four or five years, has been very satisfactory, and I know that my standard has had the effect of modifying slightly the standard of one of our prominent valve manufacturers. I think it is an easy matter for us to get some leading points on this subject, particularly in the matter of cast-iron pipes, where the flanges are cast on solidly, and that it will save us all a great deal of trouble in the long run.

Mr. E. F. C. Davis.—About fourteen years ago, in the shop with which I was connected in the mining regions, where we dealt very largely in pipes and connections of various kinds, we found it necessary to adopt a standard of pipe flanges. We simply took our thicknesses of cast-iron pipe and adopted a flange which would take a reasonable size of bolts for ordinary work, and we laid out that as a standard, and we have stuck to it ever since. Our works have become considerably larger since then, and we have had the opportunity of spreading this system over quite a large territory in our part of the country, and we have all found that the adoption of the system has saved an immense amount of trouble and inconvenience and waste of material. I think that the members will find that any steps in that direction will pay for themselves very handsomely, judging from our experience in the matter. We found that we could work in our standard in almost all cases. I would like very much to see the committee appointed.

[The motion as to the appointment of a committee of the Society on standards for pipe flanges was then put and carried, and

the committee was appointed, as appears on page 17 of this volume.]

Mr. P. A. Sanguinetti (closing debate.)—With regard to the remark that the pump makers want to sell their pumps, I would say that we would ask them to be content with the profit which they would make on the pump itself, and not with regard to what may be attached to it. I find these divergencies so slight, that if we, as a neutral body, were to approach them they would very likely fall in with our recommendation. In fact, if the members would read the remarks in the first paragraph of my paper, they would notice that I was very careful in what I said. "The object primarily set forth is the taking of such action by the Society as will result in the adoption of a uniform standard by the manufacturers. We cannot select a standard which will influence the manufacturers, but we may ask them—and I hope we shall ask them with some emphasis—to modify existing standards and conform to a universal one. I assure the members that it is a matter of very great importance. I have had this trouble under my notice for eight or ten years now, and I think the time is ripe for some reform. We will suppose that we are working to connect up the flange of a large pump to the line of a discharge pipe. We want to attach a check valve to it. We order a check valve from some maker who makes them. If we are pressed for time we have to get them from his stock. If not, we order them with a flange specially designed to suit us. Suppose we are pressed for time—in nine cases out of ten we are and of course the flange of the check neatly overlaps the flange of the pump. Now, you want a stop valve beyond the check. You get a stop valve from some other maker. You use his standard of flange. You get a flange which fits the check-valve flange as much too much the other way. We won't say anything about the pipe men, because they would probably make their flange diameters as we order them. But there is a case where you may have from three makers three different diameters; and in many cases where the bolt holes have to be drilled so near the outer diameter of a large flange, we have had to shrink wrought-iron bands to strengthen the metal at the edge in order to get sufficient material on the outside.

Take another case which has been raised by one of the gentlemen. In many cases we want to attach a copper pipe to a gate-

valve flange. A copper pipe is usually made, as Mr. Towne expressed it, with a flange turned up at the end. Now we want a wrought or cast iron flange which will bind the pipe to the valve. We have to make both the same size. We cannot help ourselves, because the head of our bolts comes pretty close to the fillet of the flange of the valve, and we are compelled to make our pipe flange the same size as that of the valve for the sake of the bolt holes. This thing happens right along.

CCLXX.

STEEL CAR AXLES.

BY JOHN COFFIN, JOHNSTOWN, PA.
(Member of the Society.)

THE adoption of steel axles has been looked at too closely from the point of first cost. The car and railroad companies have tried to get axles which meet their specified requirements as cheaply as possible, and the manufacturers in their turn have aimed to meet these requirements as cheaply as possible, with no concern whatever about the life of the axles after the railroad or car companies had once accepted them. The manufacturer, in this as in other lines of business, should not be content with making his work good enough to fill the specified requirements, but should look to future reputation. He should judge what constitutes good work, and if he thinks in filling a given specification he is making a poor quality of work, he should refuse to work under it. In all cases where steel has been used in the arts, the maker and user have had to co-operate to establish the right quality, and the problem has generally been solved by the maker, the standard of quality of the product being maintained by tests made under the supervision of the buyer.

It is generally admitted that open-hearth steel is a better material for axles than Bessemer metal, because it is more uniform and has more ductility for a given elastic limit and ultimate strength. The chemical qualities and particular mode of manufacture are best determined by the steel maker, and he who gives the most care and study to this part of the business will have the greatest reward.

The ingot should be of sufficient size to insure a thorough working of the metal in blooming. As this paper discusses more especially the treatment of axles after they have been forged, the theory will be reviewed in brief on which the treatment is based.

Carbon exists in steel in two principal states, hardening and non-hardening. These terms are chosen because they do not conflict with any advocated theory of carbon. Hardening carbon is that

form of carbon found in steel which has been hardened, *i.e.*, heated to a high red heat and quenched in water. Non-hardening carbon is that form of carbon found in steel which has been heated to a red heat and slowly cooled.

If steel be heated to a certain temperature, W, nearly all its carbon changes to hardening carbon, and the change is quite sudden. If the steel be cooled slowly from the temperature W, the carbon remains in the hardening state until a somewhat lower temperature, V, is reached, when it begins to change to non-hardening carbon. This change is somewhat slow, so that if the steel be suddenly cooled in water, there is not time for the change to take place, and the result is hardened steel. There is a certain chemical force in the change of carbon which causes a breaking up of the crystals, when the change is from non-hardening to hardening.

The above is the theory of Brinell, ably expounded by him in an article in the first number of Jern Kontoret's *Annalen* for 1885, also translated in *Stahl und Eisen*, Nov., 1885, and again in "U. S. Ordnance Construction," Note 37. The writer reviewed this theory more fully, and gave some corroboratory facts, in a discussion of Mr. Metcalf's paper before the American Society of Civil Engineers, April, 1887.

Further observations which have been made, bearing directly on this subject, are:

1st. The chemical energy of the change of carbon is commensurate with the amount of carbon present.

2d. The work to be done in breaking up the crystals is commensurate with their size, and is somewhat modified by the character of the crystallization. These statements would seem to be axiomatic, yet as each step in our path of reasoning should be lighted and confirmed by experiment, the facts should be given which confirm them. A steel bar containing .50 per cent. carbon was heated to a high temperature and cooled. Its fracture then was coarsely crystalline. It was then heated to temperature W (which is the refining temperature spoken of by Mr. Metcalf) and cooled in water. Its fracture was almost amorphous, no crystal forms being visible by the naked eye. A bar of steel of .20 per cent. carbon was then treated in the same manner throughout; the result was a fracture, fine in places but in general presenting outlines of the crystal forms of the size of the original crystals. Heating this bar again to temperature W and cooling in water, the fracture was as fine as in the first case, the reason of this being that in the second treatment

it had exactly the same amount of carbon energy to act on a less stubborn structure.

3d. While the carbon is changing to its non-hardening state, a force is exerted which has a tendency to break up crystallization. Its observed effect is to cause a darker colored fracture, owing to the crystal faces being dull, each face having small particles adhering to it, rent from the face of its neighbor, showing that there must have been an interchange or an interlocking of molecules, rendering the cohesion between the faces very great. As there are as many striking phenomena manifested in the change of hardening carbon to non-hardening, as in the change from non-hardening to hardening (among them being the reheating, re-expansion and temporary weakening), it would seem that there was as much force exerted tending to break up crystallization as when the change is the other way, and the probable cause of its not doing so as completely, is that more resistance to destruction is offered by the colder crystal.

While steel is cooling, the carbon begins to change to the non-hardening state at a dark red heat, and it would appear from the phenomenon of water-annealing that it had nearly all changed before the steel ceased to show red in the dark; on the other hand, if a piece of steel be hardened and then heated, its carbon begins to change its state at quite a low heat (some change having taken place at a faint straw color), and when a heat is reached which shows red in the dark, perhaps the greater portion of it has changed. So it will appear that the greater amount of carbon may change to non-hardening carbon at two widely differing temperatures; and if advantage is to be taken of this change to assist in breaking up crystallization, and if the previously stated surmise be correct, that the colder crystal is more stubborn, it would follow that the best way to utilize this change would be at its higher temperature.

4th. If steel be cooled *very* slowly from a molten state, cubical crystals are formed, with little cohesion between their faces, each crystal in itself appearing amorphous; but in an ordinarily cooled steel ingot, it is very difficult to observe any well-formed crystals, the irregular plane surfaces appearing more like cleavage planes than like faces of crystals. What appeared to be well-formed prisms have been taken out, with dull faces, but on breaking them they were found to be composed of innumerable smaller crystals, or, it may be, interspersed with irregular cleavage planes running in all directions. The tendency may be toward true crystallization,

and as far as can be judged, the smaller crystals may be perfect, but many of the so-called large crystals are composed of smaller ones with coincident faces, and there seems also to be a tendency to separation between some of the faces. What kind of material (if any) lies between the faces, I do not know; but if they are separated by a material distinct from the body of the crystal, there seems to be a fight for this material. Be that as it may, the relation between different crystals and different faces of the same crystals, seems to be different, for as the cooling progresses to the point where the carbon changes to non-hardening carbon, the effect will be to make only some of the faces dull, the others remaining bright. The amount of carbon present determines the color of the fracture by this action.

5th. All crystallization takes place above the temperature V, for if a piece of steel in which its carbon is in the non-hardening state be heated to temperature V and cooled in any manner, and any number of times, no change will be observed. The exception should be made, however, of that structure which takes place where a moderate heat is maintained for a long period of time extending into weeks or months.

6th. If steel be heated to a temperature above W, its crystallization is in the most part determined by the temperature, and occurs while heating, not because there is no tendency to crystallization while cooling, but at temperatures below a white heat, there appears to be a maximum crystalization for each temperature. This has some exceptions.

7th. At a white heat, steel becomes nearly, if not quite, amorphous; if cooled quickly from this heat, the crystals are fine, for, be it remembered, it takes time for all crystals to form. They become larger as the cooling is slower, up to a maximum size, after which the further effects seems to be to change the relative cohesion between different faces, causing cleavage surfaces; the exception to this being where the steel is heated to a very high white heat and allowed to cool very slowly through a period extending over weeks.

8th. If steel, with a sufficient amount of carbon, be heated to temperature W, and, becoming amorphous thereby, be then cooled, a certain amount of crystallization takes place while it is cooling to V. The slower the cooling the larger the crystals up to a maximum size; the further effect of slow cooling, as in the case just cited, being to change the relation between the crystal faces. This is

illustrated by cooling a piece just quickly enough from W to V to attain the maximum size of crystals, and then further slowly cooling it. The crystal faces will interchange molecules during the change of carbon to non-hardening state, so that all faces will be dull.

Again, cool a piece from the same temperature very slowly throughout, and some of the faces will be bright, showing that there must have been a different condition existing between these faces before the change of carbon commenced.

It was said above that the further effect of slow cooling after the maximum size of crystals was reached, was to change the relation between the crystal faces. While this is believed to be true in the main, it would seem that this changed relation commences before the maximum size crystal is reached. This conclusion has been reached not from direct reasoning from any one fact, but from the circumstantial evidence of many observations, any one of which might have had a different interpretation.

9th. If a small bar of axle steel be heated to temperature W and cooled rapidly as possible in water to temperature V, and then allowed to cool slowly until cold, it will give a perfectly amorphous fracture ; no crystal nor crystal form will be visible under the most powerful glass. It will be very tough and ductile, and have a very high elastic limit. This result is not entirely attainable in an axle, because it is impossible to cool it as rapidly as a small bar; but it can be approached, and the more closely the better the product will be.

The Company with which I am connected has adopted the process of treating its axles as follows : After forging, they are cooled completely, so that the carbon will be in the non-hardening state; they are then heated until the temperature is reached at which the carbon changes to its hardening state, the time of so heating them being a little over an hour; the result of this change of carbon being to break up the crystallization completely, and put the steel in an amorphous state. They are then cooled as rapidly as possible to a temperature somewhat below V, and the subsequent cooling is done in the open air.

The apparatus consists of a furnace which will hold about twelve axles, with a charging door on one side and a drawing door on the other. Every time an axle is drawn, all those in the furnace are rolled forward and a cold one charged at the rear. In front of the furnace is a long bosh filled with water and provided with a submerged jet pipe, over which is suspended a cradle provided with

driven friction wheels; the axle is rolled out of the furnace onto these wheels, the friction of the wheels setting the axle in rapid rotation. The cradle is then lowered below the surface of the water, while at the same time a valve is automatically opened in the water-supply pipe to allow the powerful submerged jets to play on the revolving axle. In a few seconds the cradle is raised, carrying with it the revolving axle above the surface of the water. The axle is then picked up by a little jib crane and deposited on the cooling bed. There is still enough heat remaining in the interior to bring its whole mass to a dark red heat in the dusk. When it is put on the cooling bed, advantage is taken of a wonderful property of steel which has been heretofore explained by the writer, that when its carbon is changing its state a steel bar is very weak and most easily bent. As the axles are delivered on the hot bed, most of the carbon is in the hardening state, for though the temperature is low enough, there has not been time enough for a change. The bed is constructed of "I" beams, bolted rigidly together to conform nearly to the shape of the axle, but with the central part of the bed a little low. The axle is rolled on this bed, and the bends, if any, are detected, when, pressure being brought to bear on the high part by a simple hand lever, the axle is easily straightened. If one of these axles, after it is cold and its carbon is in the non-hardening state, be reheated to the same temperature at which it was previously straightened, it is found impossible to bend it with the same pressure, because there is no change of carbon.

In the *Iron and Steel Institute Journal*, 1879, is a paper by Mr. Thomas Wrightson, in which he shows that by repeatedly heating a bar of wrought iron, and cooling it in water, it continually shortened, and its specific gravity was continually decreased until, after fifty heatings and coolings, it had changed 1.02 per cent.

Prof. G. G. Stokes, F. R. S., advanced the theory for this phenomenon which has been universally accepted, that in the first stages of cooling, the shrinkage of the crust over a center whose body only slightly changed brought about a condition in which a smaller surface amount of crust had to hold the same amount of material, and it merely put itself into the best shape to do so, shrinking in length and enlarging in diameter; and that when the center began to contract, the outside, being cold and rigid, resisted its contraction and held it somewhat in permanent expansion.

It is to be regretted that we have no data from similar experi-

ments on steel, for it might be that the existence of slag or other foreign matter made such a behavior possible. I call attention to this paper and the theory of Prof. Stokes merely to show that it has no bearing on the axle treatment described, for while in the first stage of cooling, the result may be the same; the axle may get a little shorter and a little larger; but in the second stage of cooling, the outside and center come to nearly the same temperature long before the greatest amount of contraction occurs, and at no time is the outside crust rigid enough to much resist contraction of the interior. Axles as they come from the forge are irregular. Different axles and different parts of the same axle are forged at different temperatures, and must of necessity be so, where the form is the principal point of attention of the forger. The parts forged below temperature W have a fine structure, while the parts forged at a higher temperature have a coarser structure with bright cleavage surfaces; this bad structure being worse as the heat is greater. I do not wish to be understood as thinking that a fine structure attained by forging at a temperature below W is desirable, for it is somewhat to be avoided as producing uncertain results, especially in hammered work. On the contrary, it is a dangerous plan to forge *below* temperature W, and if forged *at* temperature W the bad effect of slow cooling to V is met. Therefore, I cannot see how an axle can be forged so as to leave it in the best structural condition. If every part could be forged at exactly temperature W, however, the best results would be obtained. The subsequent treatment described corrects bad structure due to forging, the result being a uniform product, for the men become skilled in judging the correct temperatures, and their only object of work is the production of the best structural condition. This process was put in operation on December 15, 1886. Very soon after this a series of tensile tests were made, the report of tests being signed January 12, 1887. It is supposed the axles from which the tests were taken were treated within two weeks after the process was put in operation. Six axles were tested, two test pieces being cut from each one. Tests marked "O" were cut as near the outside as possible; tests marked "S" were taken so that the axis of the test piece fell midway between the outside and center; all tests were cut from the wheel seats, which were $5\frac{1}{4}''$ diameter; the test piece being $8''$ long between fillets and $\frac{5}{8}''$ diameter. The following is the result:

Mark.	Elastic Limit.	Ultimate Strength.	Elongation in 8 inches.	Reduction of Area.
O...........	42,710	85,180	20.0	38.5
S...........	42,370	83,120	3.7	16.5
O...........	44,990	92,320	16.9	33.2
S...........	46,940	95,600	16.4	28.7
O...........	41,080	88,670	18.3	37.3
S...........	43,680	89,640	14.5	23.7
O...........	43,030	86,600	20.0	42.0
S...........	44,000	87,860	14.0	27.0
O...........	40,100	86,510	16.5	38.5
S...	39,120	80,900	18.0	35.5
O...........	40,260	88,740	16.0	28.1
S...........	40,750	88,680	13.7	21.7

These results are given to show generally the high elastic limit of steel subjected to this process, without loss of elongation. It must be remembered that the tests were made when the process was first started, and before the men had acquired the skill which they now possess; and that they have acquired more skill and do better and more uniform work is proven by the better results attained by the drop test. It is to be regretted that we have not made recently a regular series of tensile tests like the above; the single tests which we have made from time to time showing better than the above, but are not quite comparable, being made on P. R. R. Standard Test Piece.

Particular attention should be called to tests recently made from an axle; the axle was cut in two, one-half of it alone being toughened. Tensile test pieces were cut from each half, 4 inches between fillets, .505″ diameter. The following is the result of tests:

	Elastic Limit.	Ultimate Strength.	Elongation in 4 inches.	Reduction of Area.
Untoughened	30,000	71,520	24.5	51.5
Toughened ..	44,000	72,020	24.07	57.2

As these tests were part of a fatigue experiment, transverse tests were also made with proportionate results. The tests are remarkable as showing the same properties throughout, except elastic limit; the toughened piece being 46.6 per cent higher than the

other in this respect. As great as this percentage is, it is not enough to measure the superiority of the toughened piece of steel. The untoughened piece was coarse crystalline, with bright cleavage surfaces, and had that structure which breaks very easily on sudden shock, while the toughened piece would not break under a blow of any character until it had been distorted enough to account for breakage from the data of the tensile test. The above statement is one hard to substantiate by actual experiment on any apparatus now used for testing, but I am convinced, from what I have observed, that it is true. Some evidence corroborative of this will follow. While the result of these tests are fresh in your minds, I wish to point out the manifest absurdity of estimating the qualities of axle steel from the ultimate strength and elongation.

These properties seem to be in a great measure independent of the structural arrangement. Axle steel being a ductile material, some considerable elongation will always occur, and the effect of this seems to be to elongate the crystals and increase their cohesion, so that after the elastic limit is passed, the further properties seem to be the same. It is an erroneous supposition that a quick, sharp blow develops crystals, based on the observation that a piece of iron will sometimes show a crystalline structure when broken by a quick blow, and be fibrous under a different breaking blow. The crystals or cleavage planes exist prior to the blow; a quick blow will break them apart, but the slow blow will not. This is proven by etching. A steel axle may break from a blow received in throwing it off a car, and yet test pieces cut from the broken ends give an ultimate strength and elongation above that required by our great railroads. Is it a wonder then that such steel is thought to be a treacherous material? As the comparative elastic limit is the best indication of a good structural arrangement, and is the property on which all load calculations should be based, more satisfactory results can be obtained by incorporating the elastic limit in the specifications. A suggested specification for axles, based on the tensile test, is as follows, viz.: Cut the tests as P. R. R. does, ⅝ inches diameter, 2″ between fillets midway from outside to center of axle, half in journal and half in dust guard bearing. The elastic limit should be high enough to insure a good structure, but not out of the easy reach of the careful manufacturer: say 35,000 lbs. Do not specify the ultimate, but put the elongation at fifteen per cent. These would be lower limits, but one of these properties should be higher than this, as 35,000 lbs. elastic limit and fifteen per cent. elongation in a

two-inch specimen might be obtained in a quite inferior quality of steel. To insure the general quality of the steel being good, require that the elastic limit in pounds multiplied by the elongation in per cent. should exceed 700,000. The result of the test would then have to be above one of the following sets:

ELASTIC LIMIT.	PER. CENT OF ELONGATION.
35,000	20.00
36,000	19.45
37,000	18.92
38,000	18.43
39,000	17.95
40,000	17.50
41,000	17.08
42,000	16.67
43,000	16.28
44,000	15.91
45,000	15.56
46,000	15.22

The manufacturer to furnish 101 axles for every 100 ordered, one axle being a test axle. If the first test fail, another test to be cut from the same axle or from another at the option of the buyer; if the second test fail, the 100 axles to be rejected. If tests are cut from two axles, only 99 to be paid for in case of acceptance.

A substitute test is suggested, for it would be a hardship to the manufacturer to work under such high specifications where a single test might fail from no fault of quality of material: for be it remembered, a shoulder in the forging falls midway in the test.

In proposing the above test, it is not recommended as superior to the drop test, for a good drop test is the best measure of the quality of an axle. The weight and fall of the drop can be arranged so as to bring out all the weak points of an axle. The P. R. R. requirements on steel axles, $4\frac{3}{8}''$ diameter at center, are five blows at twenty-five feet of a 1,640 pound weight, striking midway between supports three feet apart, axle to be turned over after each blow.

When the toughening process was adopted at the Cambria works, a series of continued tests were commenced which consisted

in striking the test axle forty-five additional blows, making a total of fifty blows, cooling the axle cold with water between each six blows, with the following results: Only three axles in all have failed under the inspection test, all of these being toughened during the first month of working the process and before the men were skilled in it. The percentage of axles which have broken under fifty blows, including the three which broke on inspection, is less than the percentage of rejection on the inspection before adopting the process; and this is continually becoming less, as of the last thirty-two axles tested none have broken, all having had fifty blows, one immediately before this breaking on the fortieth blow. I have the record of only one axle carried beyond the fifty blows to breakage; this broke on the one hundred and twenty-first blow.

DISCUSSION.

Mr. Wm. O. Webber.—The results obtained by the methods described by Mr. Coffin are wonderful, and this treatment, if it can be successfully and uniformly carried out, as applied to steel forgings, castings, etc., opens a new era in the application of steel to the mechanic arts.

During my railroad experience I have found an increasing aversion to the use of steel by the mechanical department, for axles, crank pins, and main and parallel rods on locomotives, the general complaint being that this metal was unreliable; and from the numerous breakages, a great many of our larger roads have entirely discarded the use of steel for these purposes mentioned above.

If, as I understand Mr. Coffin, this method reduces the initial strains in the forging to a minimum, makes the whole of a uniform texture, and at the same time increases the elastic limit without materially altering the ultimate strength and ductility of the metal, they have certainly discovered a very valuable process, and one which the railroad world has long been waiting for.

Here in New England I find the prejudice against steel unusually strong—in fact, it is entirely condemned by all the roads except one, whose superintendent of motive power has displayed unusual ability in designing forms of axles and crank pins to reduce the chances of failure to a minimum. If he is present at the meeting I should like to call upon Mr. J. B. Henney, Superintendent Motive Power N. Y. & N. E. R. R., for a statement of his experience with steel

axles and crank pins, which I know would be of great interest to the Society, and bearing directly on the subject under discussion.

Mr. J. B. Henney.—I have been using steel for axles of all kinds, also crank pins, piston rods, main and side rods, and boilers since 1873, with good results. I have had but two steel pins break in that time. This was through no fault of the material. The first one broke in Wisconsin when the thermometer was 48° below zero and three feet of snow on the ground. The tires on this engine were badly track-worn, and the engineer allowed the engine to slip badly, and broke one pin. [Engine was 17″ × 24″ American type.] The other was broken on the road I am now with, on an engine with cylinders 22″ × 22″ Consolidation type. The tires on this engine were badly track-worn, and the pin had been reduced in diameter to 5″ by wear, which I consider too small for an engine of these dimensions. This fact, and the bad condition of tires, I consider the cause of this pin breaking. My practice now in turning crank pins is to turn them of the same diameter through their entire length (as shown in Fig. 95). By this I get a longer

MAIN CRANK PIN (STEEL)

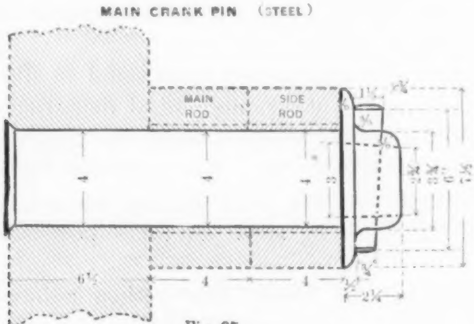


Fig. 95

bearing with no longer pin, and avoid all corners for cracks to start from. I have an engine with cylinders 16″ × 24″ running with a set of this style of pins, which has made up to date 21,200 miles, and her rods have not been taken off since she left the shops, and there is no pound in them to-day. She is running trains which make ten stops in eight miles, and hauling eight and nine coaches. The brasses are made of copper and tin, eight to one, and one-half a pound of phosphorus is added to one hundred pounds of metal.

On the rear drivers, where this kind of pin is used, I allow the crank pin hub to project out beyond the wheel hub the width of

the back end of main rod; this leaves the pin projecting beyond the fit, just the width of side-rod. The wheel-fit journal and eccentric seats on driving axles I have turned of same diameter. The fit on piston-rods for the head I turn straight, and force the head on at eight tons to each inch in diameter, using no key or nut, and no collar. I know that a steel journal can be run with a poorer lubricant, and run cool when an iron one would run hot. This I consider due principally to the difference in the density of the metals. For driving-axle journal bearings I use 88.25 lbs. copper, 7.50 lbs. of tin, 3.75 lbs. antimony, and one-half pound phosphorus to each one hundred pounds of metal, and put no babbitt in them. This makes a very close metal and wears well. All the engines on this line, with the exception of a few small ones, have steel axles, crank pins, piston-rods, and side-rods, and a number have steel main rods, and we have yet to have the first one break. We have a coach running on steel axles that has run to date 127,245 miles, and has never had a hot journal. Should we adopt steel for passenger cars, I should turn the journal, dust-guard and wheel seat of same diameter, using stop blocks at each end of the axle.

Mr. Wm. Hewitt.—It goes without saying that this is an age of steel; and apparently we are only beginning to learn the character and capabilities of this remarkable material. The sphere of its application is continually widening, and it seems only a question of time when it will entirely supplant wrought iron, and after witnessing the fine malleable steel castings at the I. P. Morris Company's establishment yesterday, which I believe were made at Chester, perhaps I should add cast-iron too. We frequently hear the metal condemned, however, on account of its being unreliable and failing to meet certain requirements through some unaccountable and mysterious reason; it is merely a polite way of acknowledging our ignorance of its nature, and these failures no doubt arise from our imperfect knowledge of the metal and the treatment that it requires. I think the Society is to be congratulated, therefore, in obtaining papers of this description, as they give us a better insight into the character of this material, and point out the way to deal with it more intelligently, and thus avoid many of these so-called mysterious failures. I do not know that I can add anything of value on this special subject, but the paper calls to mind some matters of historical interest that appear to be pertinent.

The theory in regard to the structure of the material, which Mr. Coffin describes as first expounded by Brinell, it appears to me does

not differ essentially from that announced by Mr. Chernoff in 1868. In the discussion of a paper on "the injurious effect of a blue heat on steel and iron," by Mr. Strohmeyer, read before the British Institution of Civil Engineers, Mr. W. Anderson describes the theory of Mr. Chernoff thus: "He divided the whole range of temperature from 32 degrees to the melting point of the steel into three zones; steel confined to the temperatures in the lowest zone, the limit of which he called A degrees, could not be hardened, no matter how energetically cooled; steel raised to any temperature in the middle zone, the superior limit of which was at B degrees, did not undergo any molecular change, though some qualities would harden on being suddenly cooled; and, finally, steel heated above the temperature B adopted an amorphous or wax-like structure, and became very plastic, the said plasticity extending up to the melting point. He next drew an analogy between the behavior of a hot concentrated solution of alum and steel in the highest zone. It is well known that a hot solution of alum, if allowed to cool slowly, would solidify in large crystals, but if kept agitated it would solidify into small crystals; if made to cool rapidly, it would also form a finely crystallized solid; and, finally, if made to cool rapidly, and if kept agitated as well, the finest crystals would be formed. And so it was with steel. If heated above the point B and allowed to cool slowly, the metal would become coarsely crystalline and unfit for use; if cooled slowly, but well worked all the time, a fine silky grain would be obtained, and this result would be still more apparent if the cooling took place moderately quickly. If cooled suddenly, as in the tempering of large masses of metal forming guns, a fine and uniform grain would result. The temperatures A and B varied with the chemical quality of the steel. In pure steel, the combination of iron and. carbon only, Mr. Chernoff stated that the temperatures became lower in proportion as the percentage of carbon increased. Sir Frederick Abel, in discussing the same paper, alluded to the different forms which the carbon assumes as hardening and non-hardening under varying temperatures. This paper, with the discussion, has been printed by the Navy Department as Serial No. 21 of the Naval Professional Papers, under the title "The Working of Steel."

Mr. A. L. Holley, in a paper on "Solid Steel Castings," as made of malleable steel at Terrenoire, France, refers to the effect of annealing as increasing the strength and ductility of the material, by virtue of a change in the crystalline state which is characteristic of all ingot metals. He says: "In this state the structure is loose;

each crystal is an independent mass easily detached from its neighbor, and the metal is reduced to its minimum strength and elasticity. In ordinary steel, rolling or hammering will not only close the blow-holes, but will change the crystallization. In the Terrenoire solid steel, annealing at the proper temperature imparts all the qualities due to rolling or hammering ordinary steel. Repeated and careful experiments of the most exhaustive character made at Terrenoire, have established this fact beyond a doubt." He then refers to some experiments of Mr. Chernoff corroborating those at Terrenoire, and draws the obvious conclusion "that it is possible to make a steel in its cast state just as strong as if it had been hammered, provided, however, that the metal is regularly without blow-holes. The temperature at which annealing should take place is a good cherry red; if the temperature is too high, the crystalline structure remains, and with it the lack of strength." Some of these castings contain as low as .18% carbon others used for projectiles contained from .50% to .56% of carbon.

Although Mr. Holley appears to be familiar with the investigations of Mr. Chernoff, it does not seem to have occurred to him that the crystalline structure could be broken up by sudden cooling to a certain lower temperature, and the metal still annealed by holding it at this temperature, and then cooling slowly. That it apparently matters not how you manipulate steel, if you only treat it right in the final operation, is a very important discovery, and a far-reaching one, and opens a subject that is worthy of further investigation.

Prof. J. E. Denton.—Although I agree with the writer of the paper that the showing in it makes it probable that raising the elastic limit of steel does account for the extra toughening, such is not the fact with regard to wrought iron. The records of the tests made by Commander Beardsley of the United States Navy in testing chain cables showed that iron which under the drop test was worthless was often of a higher elastic limit than iron which under the drop test would bend double. I have recently been working upon a case of broken axles. A large number of axles of wrought iron broke under drawing-room car service, and upon taking samples from the axles and testing them, the tensile strength was over 50,000 pounds, the ductility excellent—over twenty per cent.—the metal welded very well, and it bent nearly double cold. Chemical analysis showed a quarter of one per cent. phosphorus and .16 per cent. of silica. These two facts would appar-

ently condemn it in the ordinary opinion. But upon sending for the best axle that I could obtain from railroad people, known to be made in the very best manner, the strength was the same. The elastic limit was three or four thousand pounds higher and the composition just as high in phosphorus and silica, thereby overthrowing any explanation on chemical grounds. There could be no difference found between this excellent iron and this poor sample under any ordinary mechanical tests. But under the drop test a single blow of fifteen feet broke in two the bad axle, while twenty blows from twenty feet, put on as the gentleman described, were required to break the good axle. Ordinary transverse tests also failed to develop any differences of quality in the two irons. The sole difference between the qualities of the irons was brought out by drop test only. There evidently is a large field where impact tests alone can compare certain mechanical qualities of iron and steel that are generally supposed to be measurable by tensile tests.

Mr. E. C. Felton.—I think that the matter which has been presented by Mr. Coffin is exceedingly important, not only to the steel men but to the railroad engineers as well. It is also very interesting as an example of how an abstract theory may be taken into the workshop, applied there, and made of great practical use.

Now there was one point in this paper on which I would like to ask a question. It will be noticed in the two tables given by Mr. Coffin of the results obtained by testing the pieces cut from the axles that in the tests made at the beginning, or when the operation was first adopted, the steel is of an entirely different character from that which is shown to be used in the later tests. That is, when the method of water annealing was first adopted, steel of from 85,000 to 95,000 pounds tensile strength was used, and later on a steel of about 70,000 pounds ultimate strength has been used. Was it found that the steel used in the first place was unfitted for the purpose to which it was put—that is, to be used in axles? Another question I would like to ask is this: Is there any difference in the way in which the two kinds of steel have been annealed? It will be noticed that the higher steel, averaging about 80,000 or 90,000 pounds ultimate strength, shows an elastic limit after annealing of from 42,000 to 46,000 pounds to the square inch; whereas the softer steel in the second table, which has an ultimate strength of 71,000 and 72,000 pounds, when annealed, has an elastic limit of 44,000 pounds. That is, the elastic limit is much higher in the softer steel than in the harder steel after being subjected to the

annealing. I would like to know whether the two kinds of steel have been treated in any markedly different ways?

Mr. T. R. Almond.—I want to relate a little experience of my own in regard to the action of steel after it is hardened. The peculiarity is this: After a piece of steel has been heated to the best possible heat for hardening and immersed in water, or whatever substance is used for hardening it—after it has been brought into that condition, it may, by a peculiar treatment, be brought into a condition which I do not think is very much understood—I call it a pliable condition—corresponding to a temperature of say between 250 and 350 degrees, according to the particular make of steel. I find that hardened steel need not be drawn to a temperature greater than 350 degrees to put it into a condition where it is pliable, and where its shape can be changed. I think this peculiarity is understood by many of our manufacturers of steel tools, because I notice in many instances that they avail themselves of it; but I do not think that they understand exactly what they are handling, or the peculiarities which are distinctly there. One peculiarity is distinctly there, and that is that the steel must not be subjected to a temperature higher than that corresponding to a very light straw color. I find that if I attempt to avail myself of the pliable condition—as I express it—after the steel has been drawn to a temperature of about 400 degrees, it is no longer possible to do so.

Mr. F. W. Taylor.—I can corroborate Mr. Denton's remarks as to the efficiency of a drop test, and the inefficiency of other means of testing axles.

In some experiments which we made in the works of the Midvale Steel Company some years ago, we found that the tensile strength and the percentage of elongation and contraction were by no means an accurate indication of the quality of the axles.

The test which we were required to fill was that demanded by the Pennsylvania Railroad for their passenger car axles, and we found that some bridge steel that we were rolling at the time had the necessary tensile strength and percentage of elongation to fill the specifications, so that we concluded to forge some axles from that steel.

We did so, and after the axles were forged they yielded the tensile strength and stretch required by the Pennsylvania Railroad. But when subjected to the standard drop-test of the Pennsylvania Railroad, the axles forged from bridge steel broke, in each case, from a single blow of the drop, while those forged from the open

hearth steel, which we had formerly used for axles, would stand in some cases two or three times the number of blows from the drop required by the Pennsylvania Railroad.

Apparently no test which could be made with a testing machine at that time would detect this brittleness of the steel. It was a brittleness that showed under impact, and did not show in the testing machine in any way.

Very likely it was the same cause, whatever it may be, that affected the iron of which Mr. Denton speaks.

Mr. J. T. Hawkins.—There seems to be no better established fact in connection with the behavior of steel and iron, than that the elastic limit will be lower, and the percentage of elongation less, as the crystals shown in fractures which are crystalline are larger; that is to say, that generally in fractures showing a crystalline structure, the larger the crystals the lower the elastic limit, and the less the percentage of elongation becomes. There is also probably no better established fact in physics than that in any crystalline formation the crystals are large in proportion as their formation is slowly procured. In these two sets of facts there appears to exist an anomaly worth investigating. The slow formation of crystals in steel or iron would seem to conform to the process of annealing or slow cooling; and generally with steel, if the elastic limit be not higher, certainly the percentage of elongation will be greatest in an annealed or slowly-cooled specimen, while in a bar of iron of crystalline fracture the larger the crystal the lower the elastic limit and the less the percentage of elongation.

I offer these suggestions as possibly leading to some profitable channel for the investigator.

Mr. John Coffin.—In presenting certain experimental results, I will go over the ground as quickly as I can, and in order to economize time, I will first pass around some specimens illustrating the effect of the change of carbon to non-hardening state. They are arranged in pairs, the light-colored one of each pair having been heated to temperature W and quickly cooled, thus fastening its carbon in the hardening state, while the dark one was heated to the same temperature and quickly cooled to temperature V, then slowly cooled; thus passing through the range of crystal-forming temperature rapidly, and afterward cooling slowly enough for its carbon to change to the non-hardening state. We have in this case a perfectly amorphous fracture. You can see no crystal formation in it whatever.

The next specimen I will show you is a hammered bar of axle steel, .38 per cent. carbon, $\frac{1}{2}'' \times 1''$ bent double without a crack. I do not attach any great importance to this specimen, only that such specimens have usually been from rivet and boiler plate steel. The bar was treated exactly as the axles are.

While you are looking at these specimens I will turn to the discussion of the paper. Mr. Hewitt's remarks about Chernoff's investigations are very interesting. Chernoff was the undoubted pioneer in this direction, and should have all due credit. Brinell has merely taken up the subject where Chernoff left it, and has gone into a much more thorough and scientific study of it. I can make my meaning more clear by giving a brief description of what seems to take place in a bar of annealed steel if heated. No great change in its structure occurs until the carbon changes to hardening carbon, at a low orange heat; it then becomes amorphous. If heating is continued crystals begin to form, but if the heating is further continued these crystals disappear again.

The first amorphous state investigated by Brinell is caused by the atomic disturbance incident to the change of carbon; while the second amorphous state, previously investigated by Chernoff, seems to be caused by the action of the law which causes many substances to pass through a plastic amorphous condition before fusion. Chernoff clearly stated that the amorphous or wax-like plastic structure, after being reached by heat, extended quite up to the melting point.

In my paper compare the conclusion quoted from Brinell with the 6th and 7th observations.

I entirely agree with Professor Denton, but would like to correct what might seem to be a difference of opinion. I said that the toughening process very much increased the elastic limit of the material, but not that I thought the toughening was due to this increase. I also said that the increased elastic limit was not enough to account for the increased toughening.

In regard to Mr. Almond's remarks, I have not time to review that branch of the subject, but after the meeting, or at any other time, I would be glad to talk to any member who may be interested in some experiments I have made in that direction.

In reply to Mr. Hawkins, if rather high steel be cooled from a high heat it will behave the same as iron, but if treated by heating to temperature W only, then cooled rapidly to temperature V, and then slowly cooled, it will not have the large crystals spoken of,

because we have broken up the crystals by heating to this temperature and avoided their re-formation by cooling rapidly through the zone of formation.

I will now show you some small specimens. I have not time to give you any theory on them, though I have crudely formed one. I have not used theoretical language in my paper, but merely described what appears to be the facts, and am aware that my observations somewhat conflict with the ordinary theories of crystallization. It occurred to me that if no mistake had been made in regard to the action between crystal faces, and if two pieces of steel could be brought by mechanical fitting into as close relation to each other as the faces of crystals in a piece of steel, and were then treated in a manner which would destroy the crystal faces in a single bar, the result might be the destruction of the faces of the separate pieces of steel in contact, and thereby cause them to unite. In my first experiment I did not much expect success. It was as follows, viz.: Short bars of steel $\frac{3}{8}''$ square were marked on opposite sides and broken. The fractured ends were then fitted together and clamped in this position, and the whole heated in a charcoal fire to temperature W. When cool they were found united. I have here a number of bars so treated, which I will pass around.

A Member.—What grade of steel are they?

Mr. Coffin.—Tool steel.

A Member.—What make of steel?

Mr. Coffin.—Pittsburgh steel. Here are some more specimens I will show you of bars which have been rebroken after being so united. You will notice different amounts of surface in cohesion varying from say $\frac{1}{10}$ to $\frac{1}{4}$ the entire surface of the ends. This seems to be determined by the perfection of the contact. In order to make a better study, I prepared surfaces of small cubes by grinding them together. They were then thoroughly cleaned, clamped together, and heated in a charcoal fire to temperature W.

In these which I will show you, the cohesion was not perfect, but very good; the surfaces having the appearances of unpolished silver. Here is another in which the cohesion was a little better, in one place being perfect, the metal at the edge being torn from the adjacent cube. You will see by turning to the light a beautiful tracery or pattern over the whole surface. Here are some cubes of boiler plate—carbon, .12 per cent.; manganese, .23 per cent. The cohesion was remarkable. By fastening in a strong vise, and using a long wrench I bent the piece without breaking; I then broke it by

means of many heavy hammer blows. The break shows the characteristics of the crystal faces of a piece of soft steel. You will see on each cube small particles of steel which formerly belonged to the other cube.

I have here a specimen of tool steel which is very interesting. It is remarkable as showing the three grades of cohesion, the characteristics of the crystal faces of two types of fracture; the bright face indicating slight cohesion, the dull face indicating better cohesion, and at one place the face is entirely lost by the metal being rent from the adjoining face of the other piece.

I made a number of attempts to unite small pieces in the Bunsen burner which were failures. The cause of failure was found to be due to the force of the oxidizing flame.

These pieces (showing) are the first I succeeded in thus uniting. They were held cornerwise, as shown in Fig. 102. The shape of the outside is carried up into the interior, only a small square spot being united. This experiment gave the clue to the failure, and it was found that the experiment could be performed successfully by wrapping the joint with a piece of platinum foil.

I have a burner arranged here, and can perform the experiment before you.

Fig. 102.

I have here a number of $\frac{1}{4}''$ bars of short length. I will break a few until I get a square break, as I am anxious to have the experiment succeed. I did not break the pieces before coming here, as a clean break is desirable. A little dirt or rust might spoil the experiment. While I am preparing this experiment the discussion might continue.

Mr. T. R. Almond.—I would like to say that in my factory there have been broken perhaps 50,000 pieces of Hobson's tool steel. They have been broken for the purpose of getting them into place, and after being broken the temperature was drawn to a blue. I might mention, in connection with a previous remark that I made, that the temper was drawn under pressure; if the pressure was put on after the temper was drawn, they would not assume a regular shape as was desired. If the pressure was put on before the temper was drawn, they would always come out regular in shape according to the mold in which they were compressed.

In regard to those fractures, I would say that we have every day in the week in my place the same appearances that are shown

in some of the specimens, partially oxidized and unoxidized. I think the success of the gentleman's experiments depends entirely upon the degree to which the surface is not oxidized, or rather upon the absence of all oxidation. The more complete the success he attains the less film of oxide will there be collected on the surface before the experiment commences, and of course the more complete the degree of contact to which the surfaces are brought. The only unexplainable things in the specimens shown are in those which indicate that they have been in absolute molecular contact.

Mr. Coffin.—If any one will read my paper over carefully he will find this action has been fully anticipated. The reason for not treating the subject more theoretically is that the theory is too young yet to go far from home.

Mr. J. T. Hawkins.—Mr. President, I would like to suggest to Mr. Coffin that possibly one thing that would affect the union of these fractures is the lapse of time after they are made; that they unite very much more perfectly if joined immediately than if such union be made after a considerable lapse of time.

The President.—Mr. Coffin just made that remark himself.

Mr. J. T. Hawkins.—I think Mr. Coffin merely referred to the probability of the surfaces becoming soiled if broken, and thereafter handled or conveyed to another place to be joined; and the point I desire to make is that the perfection of union between the fractured surfaces depends, more than any other consideration, upon protecting them from oxidation.

In this connection I call to mind a lecture I heard a good many years ago, by the late Prof. Mapes, very fully illustrating this matter. The general tenor of the lecture was to illustrate or sustain a theory of the Professor to the effect that if any metal could be fractured under conditions which would absolutely exclude oxygen from contact with the surfaces of fracture, they would again unite as perfectly as before fracture, without any greater pressure than was required merely to bring them into contact, and at a temperature not much above that at which the fracture was made; and that the least oxidizable metals would suffer the longest contact with the atmosphere after fracture before acquiring that state at which they would refuse to reunite; the elevation of temperature, everything else equal, of course, conducing to such a reunion.

He instanced the manufacture of lead pipe, in which the metal

in the solid state is made to flow out of an orifice, under hydraulic pressure, over a button or short mandrel supported in the orifice by three or four double-edged knives; the form of the orifice, knives, and mandrel being such that the lead, in passing out, would be split into three or four strips, according to the number of supporting knives, without the separated surfaces ever coming in contact with the atmosphere. The strips, meeting again below the knives, issued from the orifice as perfectly united as before separation. Without the precaution here observed of excluding the air from contact with the cut surfaces, no such reunion would occur; because, even in so short a lapse of time, their oxidation would be sufficient to prevent it.

I myself can give a very familiar instance illustrating this principle. In preparing stereotyped plates for the press, they require to be reduced to a uniform thickness, and this is done by means of a special planing machine, one form of which removes wide, thin shavings from the backs of the plates, much after the manner of a carpenter's plane; in fact, a common carpenter's plane is so used, when the operation is performed by hand. In this operation the shavings removed must be carefully prevented from falling upon the newly planed surfaces, or they will reunite so perfectly as to require as much effort to remove them as at first. If, however, such shavings as have been removed a few minutes be replaced upon the surfaces from which they were taken, there is not the least tendency to reunite. This stereotype metal, being rather slow to oxidize as compared with lead, explains their readiness to adhere with the freshly cut surfaces under a short exposure to the air, where lead would refuse to do so.

The most striking illustration of Prof. Mapes on the occasion referred to was as follows: He prepared two small bars of hardened steel, so small in section at the central point of the bar as to be readily fractured by a slight rap with a small hammer. One of these bars was straight, and the other bent at an obtuse angle, with the small section at the vertex of the angle. He placed the straight bar across the top of a cup filled to very near the top with mercury; and, with a tap of the hammer at the middle point, almost simultaneously with the fracture and almost instantaneously depressed the fractured surfaces into and below the surface of the mercury. The bent bar he placed similarly across the cup, with, however, the vertex of the angle, or the point to be fractured, below the surface of the mercury, and, by suitable means, broke

the bar at the angle, upward, out of the mercury. In this experiment he explained (although I did not witness it), that the fractured surfaces of the straight bar showed no signs of amalgamation with the mercury, while in the bent bar the surfaces were perfectly amalgamated and covered with a coating of mercury. His explanation was, that in the infinitesimal period of time elapsing between the actual fracture of the straight bar and its actual contact with the mercury, the steel being a readily oxidizable metal, the surfaces became sufficiently oxidized to prevent adhesion of, or complete union with the mercury; while with the bent bar, every source of oxygen being absolutely excluded, the two uncontaminated surfaces united perfectly.

From these considerations, if Prof. Mapes's conclusions be sound, it would seem that the cohesion or reunion of steel fractures of surfaces such as produced by Mr. Coffin, can be the more perfectly made the more immediately the fractured surfaces are again brought into contact, especially as two such surfaces of fracture most perfectly fit each other, and thus tend to exclude the air. Of course, the temperature is a factor, as, in the manufacture of lead pipe, the reunion explained in that case will not take place perfectly below a given temperature; and it is not impossible that the character of the flame from the Bunsen burner which he uses to heat his specimens, may have a good deal to do with preserving the surfaces from oxidation, if, indeed, it may not have removed what little may have taken place.

Mr. Coffin.—Mr. Hawkins' discussion has brought before us the question whether the union of the two pieces is due to the change of carbon as I supposed, or is it the result of a weld. Some metals weld cold, lead being one of these. If a bullet be cut in two and the bright surfaces brought together and slightly rubbed, they will unite in small spots. Iron and steel have to be brought to a point near fusion; platinum welds at a somewhat lower heat than iron, but at a red heat it will not weld. If it is argued that the reason for iron welding at a certain heat and at no lower, is that a temperature must be reached at which its scale is fusible so as to flow out and leave perfectly clean metallic surfaces in contact, then the analogy does not extend to the case of platinum, as the surface is as clean at a red heat as it is at its welding temperature. I am inclined to believe that a weld of iron or steel, in the ordinary sense of a weld (a union made by clean surfaces being forced into contact), cannot be made below the ordinary welding temperature.

I cannot, however, give a positive opinion at present, and it may be that Mr. Hawkins is right.

Prof. J. E. Denton.—If I understand clearly what is about to be shown, it is that slight pressure and unusually low heat will unite steel, and, while we are waiting it occurs to me to put upon record an incident which I witnessed regarding the welding of mild steel. I believe it is generally acknowledged that welding mild steel is a difficult matter. A large iron works, welding by machinery, has discovered that the way to weld mild steel on a large scale is to use a carbonizing flame in taking a heat. But upon trying to weld inch bars of mild steel in the same way, I found that the only way to succeed was to use an oxidizing flame, and stick the rods together at a very low red heat with a few very light blows of the hand-hammer, then reheating at full red heat permitted the completion of the weld in a very perfect form.

Mr. E. C. Felton.—I would like to ask Mr. Coffin if he will kindly answer my question.

Mr. Coffin.—I beg your pardon, certainly. The difference is perhaps partly due to a different grade of steel and partly to the treatment.

The first tests were taken during the first two weeks of working the process. Besides, I think they were from freight axles, while the last tests were from passenger axles. The specified requirements are different, some axles being subjected to tensile tests alone, while others are tested under the drop. We are sometimes restricted in making what we think the very best axles. You see if we get them too good, the buyers won't take them. (Laughter.)

To return to our experiment, if the piece if not hot enough, it is as hot as it will get in this flame. I will show you what temperature I have.

Mr. T. R. Almond.—The effect of the first film of oxide on the surface of a piece of metal, in regard to its conductivity—that is as to heat—is something that we should very much better understand than we do, I think. I had it illustrated one time when I was hardening some bright spindles; that is to say, I was heating them in lead and some of them escaped—got out of the jig that I was holding them in and fell into the lead and remained there a few seconds. They were taken out and put on one side to cool so as to give me an opportunity for reheating them. The film of black oxide on the surface must be very small indeed; I don't know what the thickness is; I should like to know; but a very

slight application of fine emery cloth is sufficient to remove it. In reheating the pieces which were blackened in that way with other pieces that were bright, I found that it took three times as long to heat the black ones as it did to heat the bright ones; they were both dipped into the red-hot lead, and there could be no possible difference in the temperature of the medium by which they were surrounded. From that I reasoned, in regard to the hardening of steel, that it is not only very necessary to heat the steel carefully, and all that sort of thing, but it is just as necessary to have the surface of the steel, when it comes out of the medium in which it is heated absolutely uniform in its character. I am myself so particular about it, that if I want to get a good result on anything which I wish to harden, I wash it thoroughly in some liquid, such as gasoline or benzine, before I heat it, and am exceedingly careful not to get any foreign matter on it that can be avoided.

The President.—I would say that this experiment is a success so far that I am unable with my fingers to break the bar in two. It seems to be thoroughly joined. (Applause.) I would say that in breaking this one there is shown a fracture clear across, another shows a fracture in the center, another diagonally—very good breaks.

CCLXXI.

POWER PRESS PROBLEMS.

BY OBERLIN SMITH, BRIDGETON, NEW JERSEY.

(Member of the Society.)

To the maker and user of ordinary machine tools, which are often quite complicated in their construction and action, the machine commercially known as a "power press" may seem too simple to require any literature of its own. If the motions are analyzed which are required in the two general classes—machine tools and presses—it will be found in machines of the *planer* type, for instance, that it is necessary to move either the work or the cutting tool in an accurately straight line to a considerable distance, against a cutting pressure, and in two other straight lines both at right angles to this first line, and one of which has its direction adjustable to make an infinite number of angles with the other. For the necessary feeds and adjustments, these motions must be in both directions, and in the case of the first-mentioned one the backward or return motion must be much faster than the forward motion.

In machines of the *lathe* type, in which may be included screw-machines, milling-machines, boring and drilling-machines, etc., there is frequently a similar set of motions to the last two described for the planer; while for the first-named rectilinear movement is substituted a rotary one, so as to produce cylindrical and conical surfaces instead of planes. In both types of machines many of the motions must be reversible, intermittent, and variable in speed. These requirements, together with all the adjustments necessary, and the great strength and rigidity which ought to be (but is not always) embodied in such tools, make them ponderous as well as complicated, and consequently expensive. This ponderosity or clumsiness for the stationary and slowly-moving parts the writer has elsewhere advocated in writing of the "anvil principle" *versus* the "fiddle principle" when criticising the extreme flimsiness of many of the machine tools in the American market.

In power presses, and indeed in metal working presses of all kinds, such as foot, screw and hand-lever presses, this anvil principle is still more important than in machine tools, not only to control the motions rigidly and accurately, but to absorb the percussive

blows to which such tools are subject. Of course in the case of drop presses this resistance to percussion is still more necessary, but these will not be further discussed here, because they are more akin to steam hammers and other tools of the strictly percussive type, where the blow is given by momentum rather than great pressure, and where the motion is usually an accelerated one. The presses alluded to above as working by foot and hand power will perhaps in some cases fall incidentally within the scope of this paper, but the points which it is especially desirable to bring out will be fully covered by the discussion of power presses proper, the essential principle of which is embodied in a slide-bar made to reciprocate toward and from a stationary bed by a crank or cam upon a main shaft, which is driven (either directly or through a train of gearing) by a belt running upon a fly-wheel, or upon a pulley connected therewith. To simplify the argument, the somewhat complicated double-action presses will be left out of the question. These are used in the "drawing process," and have two or more slide-bars, one within the other, each with a separate set of motions derived from various cranks and cams. It will suffice to discuss a few of the problems which arise in the design, construction and operation of the ordinary type of power press, either geared or non-geared.

In its essential features such a press, as usually built, is really a very simple machine, consisting chiefly, as far as its moving parts are concerned, of a slide-bar connected to a crank on its main shaft by an adjustable pitman—this shaft being detachable from the fly-wheel or gear which runs loose upon it by an automatic "stop-clutch" actuated by a treadle. Such a machine, to a casual mechanical observer who had not studied it as a specialty, would seem to be far easier to design than the machine tools proper, mentioned above, but after taking it up as a specialty his judgment would be reversed, and he would find several knotty problems to keep him awake at night while the builder of lathes or planers was quietly sleeping the sleep of the just. This difference arises partly from the fact that power-press building is both a newer and a smaller industry than the making of machine tools. It has not been so fully experimented with either in point of time or in regard to the number of experiments—in other words, these machines have not arrived at the same stage of development in the evolution of their race as have the older and more numerous tribe of lathes and planers.

A more important difference, however, lies in the fact that machine tools are subjected to very little percussive action, while all the parts of a power press are constantly endeavoring to hammer themselves and each other to pieces. This is due chiefly to the fact that the work in the dies offers a sudden resistance to the moving parts when it is struck; and incidentally, to the sudden stopping and starting of several heavy members of the machine while the wheel which drives them revolves at a constant speed.

As a consequence of these conditions one of the problems arising is, how to fasten the parts together so that nothing will jar loose or come apart while in action. This difficulty can usually be overcome by the use of lock-nuts upon all screws, and by driving fits where screws cannot be used. Such fits, however, are objectionable where facility of taking things apart is properly provided for, and none of the presses in the market are yet ideally perfect in this respect.

Another problem which has not been solved in a wholly satisfactory manner is to make an adjustment of the slide-bar, which is long enough in range, and yet which combines simplicity and cheapness of construction with such strength and security of fastening as to withstand the heavy blows and pressures to which it is subjected. Among the most simple devices for this purpose is perhaps some form of eccentric, either at the lower or upper end of the pitman, but the chief objection to these is their short range of adjustment—since it is difficult to clamp them firmly enough in place so that they will not revolve if their eccentricity is very great. Various kinds of flat wedges have also been used, but are open to the same objection. In some kinds of presses a screw-thread upon the pitman itself, with lock-nuts or other clamping devices, has proved successful, as has also a special screw between the slide-bar and the bearing by which the pitman is attached thereto. The latter, however, lacks simplicity, and all these screw devices, though giving almost any desired range, take too much room for the compactness of design which is usual in punching presses for heavy metal. A very good long-range adjustment can be gotten by making the bed of the press to set up and down; but this again increases cost, especially if made as strong as the rest of the press frame.

A third and more difficult problem is to make an automatic stop-clutch which, by a slight motion of a treadle or hand-lever, will instantly throw the main shaft into gear with the driving wheel

which is revolving upon it, and which will at the same time lock the shaft and wheel together securely against rotary stresses in either direction. By "instantly" is meant within a small part of the wheel's revolution, say from one-third to one-sixth, but it is better if the actual starting is not instantaneous, that time may be given for the inertia of the shaft and attached parts to be overcome, so that they may start gradually. This has been attempted by various forms of friction-clutches, which, if successful in all respects, would not only tend to lengthen the life of the press by avoiding the sudden hammer-blow incident to the ordinary clutch, but would, by the quietness of their action, minister soothing balm to the nerves of the operator and all other persons unfortunate enough to be working in shop or counting-room in the near vicinity. My own experience has, however, led me to believe that this friction-clutch business is a very difficult one to deal with, as the amount of power to be conveyed to the shaft at the time of doing its hardest work is much greater than in the case of ordinary friction-pulleys and clutches for shafting. A device can undoubtedly be made strong enough to do the work properly if the friction is applied out near the rim of the wheel, where it will act with considerable leverage, but the difficulty then is that the parts of the mechanism which are attached to the shaft are so bulky that they give too much momentum for the sudden stopping of the crank at the top of its stroke when the clutch is thrown out. The experimenter with such a clutch, where he may have to stop the crank at every revolution not more than five or ten degrees from a fixed point, perhaps as often as a hundred thousand times a day, with the enormous locking pressure necessary, will find that he has in hand a very different matter from that of stopping and starting a line of shafting, where the time of several revolutions may be used for full engagement or disengagement. In my own practice, after experimenting with friction, and with various forms of springs, to give an elastic blow, I have fallen back for the present on the old principle of a positively locking clutch as the best practicable thing, in spite of the noise and jar which it creates. Such automatic friction-clutches as I have observed in commercial use seem to be adapted for light work only, and do not run as quietly as they should. It is earnestly to be hoped that time will solve this problem in favor of a noiseless clutch of some kind.

When power presses were first invented their use was such a great improvement upon previous methods that it was considered

good enough to wait for the wheel to come round to a certain single point of locking with the shaft, and then to have no provision against "back-lash." In these days, however, competition in getting out presswork rapidly has made it necessary that the shaft shall start as soon as possible, and it is considered desirable to have from three to six interlocking points upon the wheel, according to the speed. It has also become necessary to avoid back-lash, on account of the frequency with which "spring-drawing dies," so called, are used. In these there are very powerful springs which have to be compressed by the downward action of the slide-bar, and which, in reacting, tend to push it upward during its up stroke faster than its normal rate of speed. In such cases, if the wheel and shaft are not rigidly locked against relative motion, the shaft gets ahead of the wheel, so to speak, for a time; and as soon as this action ceases there is an unpleasant blow from the wheel catching up again. As before intimated, this rigid interlocking is difficult at the high rate of speed at which the clutch is usually thrown into gear, and the tendency of the shaft to run away from the wheel on the up stroke has to be stopped by a friction brake. This, however, if set tight enough to work properly, wastes a great deal of power, unless it is made automatic, so as to act only at the particular time desired. This again makes additional expense, and detracts from the simplicity so desirable in machines of this kind.

A fourth problem is so to throw the clutch out of gear automatically as to make the shaft stop exactly in the angular position desired. This would be easy enough were the speed, lubrication and tightness of adjustment of the wearing parts uniform, as well as the weight of dies and other attachments on the slide-bar; but, under existing conditions, there is very great variation in the position of stopping, principally caused by a variation of speed of the driving power. This difficulty has been partially overcome by a friction-brake upon the shaft, but, as before said, it frequently wastes a good deal of power, by the holding down of the treadle, without being stopped at each up stroke, especially where the press is running continuously.

In the practical working of these presses many other minor problems arise, due to the accelerating or retarding action of the various attachments which are frequently put on presses for feeding and gauging the work, etc. In general, it may be said that all press-makers have found it somewhat difficult to contrive a clutch

and brake arrangement which is simple, durable and cheap, but which can be put upon all presses of a given size and sent out to take its chances among the multifarious conditions which the user (generally *not* a machinist, as is the user of a machine-tool) may impose upon it. These may consist of speeds too fast or too slow; wearing adjustments set too tight or too loose; slide-bar normal or heavily loaded; shaft normal or loaded with various cams, gears, etc., for automatic "attachments;" ordinary dies or dies fitted with strong reactionary springs; dies for thick punching which do their work during a considerable portion of the stroke, or embossing dies which meet heavy resistance only at the end of the stroke, and perhaps cause an upward reaction by the resilience of the press frame itself—which in open-front presses is often very powerful, owing to the elasticity of the metal. Furthermore, if, for the sake of uniformity in manufacturing, the standard clutches, etc., are also used in various geared and non-geared drawing and other double-action presses, still other conditions arise—and the molecules of the press-maker's brain-fibre must swing in orbits still different to meet and cope with them successfully.

There has been no attempt made in this paper to analyze thoroughly the mechanism of the power press, nor to point out full solutions of the difficulties presented. The foregoing has simply given a few hints regarding these difficulties, with the hope of calling the attention of the mechanical world more particularly to the interesting machine under treatment. Neither has the writer attempted to mention various improvements which in his own practice he has found it necessary to contrive for the proper working of these machines. There has been within the last ten years a marked improvement in almost all the presses on sale in the market, but there is yet an extensive field open for their further development before they become as perfectly adapted to their functions and environment as is the ordinary engine lathe.

DISCUSSION.

Mr. John J. Grant.—I would like to ask Mr. Smith what the trouble is with the elastic stop-motion which he formerly put on his presses. We have a very large press in the American Sewing Machine Company's Works, and it has been working about a year. It is working absolutely perfectly. I do not see anything

the matter with the elastic motion which he says he has discarded and has fallen back on the old positive stop.

Mr. Wilfred Lewis.—I cannot say that I have had much experience with machinery of this class; but I have been interested in the difficulties presented, and think I may confidently assert that the third problem has been satisfactorily solved. I refer to a patent, No. 352,623, granted to me about a year ago and controlled by William Sellers & Co. We have used these clutches on our machine tools for feed motions very successfully, and they have been applied recently to heavier work. We have an application of the clutch on a car wheel boring mill in which the table, weighing something like a ton, is started and stopped at the rate of twenty revolutions a minute. This table is not driven directly by the clutch but by a shaft gearing into the table and running at 120 revolutions a minute. There is no shock in starting or in stopping. The clutch is absolutely positive, although friction is used, and there is no possibility of slip beyond a certain amount used in starting. It is as positive as a solid clutch and adaptable to various requirements. It can be used either as a positive clutch without slip, or it can be used for the transmission of a definite and limited amount of driving power where slip is desirable to overcome inertia.

Mr. Thos. S. Crane.—One of the requirements in constructing a large press is very frequently to balance the moving parts. In the Marchand double-action presses, largely used in this country for drawing deep tinware, this was done by a lever and heavy counter-balance weight; but it has since been successfully done, by putting a small steam cylinder over the press and connecting its piston with the moving weight. One would suppose that the steam would have to be introduced into the cylinder by a valve, and cut off at each stroke, in the same manner as in a steam engine; but that is not the case at all. The steam is simply introduced under the piston from the boiler, the steam in the boiler being kept at a tolerably uniform pressure for providing other power; and the parts being proportioned to balance the weight at that pressure. I have seen such cylinders from six to fourteen inches in diameter connected with large presses and balancing the weight so that the power required to operate the machine, was merely that due to the work performed; the great mass of the pressing tools moving almost in equilibrium.

Mr. John Coffin.—The first form used by us in our electric

straightening machine was one of those so-called positive friction clutches, which are frictional only in name. The friction being greater on some member than on another makes a practically positive clutch of it. Such is the clutch used by William Sellers, and it is a very good clutch for the purpose. The clutch in use was a roller clutch, and it was very much like the ratchet on the old Willcox & Gibbs sewing machine. It consisted of rollers in place of balls. The clutch now proposed, which we are putting on, is an ordinary crab clutch engaged by means of a steam cylinder. It is necessary to have the first engaging force required very small, as the clutch is actuated by an electro magnet, and the plan is now to move a balance valve of a small steam cylinder and engage the clutch by that means.

Mr. Wilfred Lewis.—I would like to say that the clutch used on the boring mill is not a positive clutch at the start. There is a certain amount of slip, which enables the inertia to be overcome gradually, and after this amount is reached the clutch becomes positive.

Mr. Oberlin Smith.—I am very glad to hear in such a way from Mr. Grant. If he cannot see the faults which I can see in my own presses, so much the better. Probably I am more critical than a good many other folks; at any rate I hope so.

The springs in fly wheels work excellently sometimes—generally in fact; and I did not say that I had wholly abandoned them. I said that I had fallen back, for the present, on a simpler, but improved, clutch mechanism, in some new designs that I am getting out; and the reason is purely a commercial one—our customers demand presses for so little money that it is almost impossible to put the refinements into them which we would like to. They do not generally go into nice machine shops like Mr. Grant's where they are very well taken care of. I find that a lot of costly refinements put onto a press, are apt to give trouble after a time, because they are not properly taken care of by the class of cheap workmen operating them. But the spring in the fly wheel has, under certain conditions, an inherent fault. When the wheel commences to move the shaft gradually, by the spring yielding, the shaft gets under way and goes on at the speed of the wheel until the punch strikes. Then the shaft stops until the spring is compressed again and the wheel catches up with it on the down stroke. This is apt to make a little noise or jar. Then where there is resilience caused by "spring-drawing-dies" or

with a press frame open in front, and hence not very rigid, the bar starts ahead of the shaft on the up-stroke, because whatever springs have been compressed react and give an additional "boost" to the thing, so to speak, and throw forward the shaft with more or less force. If the speed happens to be high, the brake has to be adjusted so tight that there is a good deal of lost power. It is easy to control a press under almost any conditions, if you put enough brake upon it; but, as before stated, the chief objection to the various contrivances which require a heavy brake, is this matter of losing power. It is especially noticeable when running a press nearly continuously, for instance when feeding a long bar of iron through to punch a row of holes in it. The brake (if a plain, ordinary one) is in action all the time, and must be tight enough to stop the clutch properly when you get to the end of your bar, and yet it is needed only at the last instant. I have known a moderate-size press to have four or five horse power entirely thrown away by having the brake set tight enough to control it. I think if Mr. Lewis had tackled regular power presses that go among all sorts of Irishmen (or, say, Philadelphians) to run, he would find, perhaps, that his clutch needed modification, to make it meet all the varying conditions of speed, and inertia, and gummy oil, and reacting dies, and the going in and out of gear a hundred thousand times a day or more. I hope that Mr. Lewis has got ahead of all of us on his automatic stop clutch, but think it needs testing on actual presses of a great many kinds before it can be called a press clutch.

Mr. Crane speaks of balancing presses. That is all right for large drawing-presses or in any heavy slow-running presses, but in these little quick-running presses having to go 150 times a minute, you must not put much on the shafts to balance them—steam cylinders, or anything of that kind. You must have something that is very simple and very quick acting.

Mr. Coffin speaks of the roller friction clutch. I do not call anything a positive action unless it puts the wheel and shaft in absolutely the same relation every time, so if the thing is thrown out under the same conditions it will always stop at exactly the same angle. Anything that thrusts a wedge between is not quite so positive as that. Of course an ordinary friction clutch is not positive at all. I think and hope that the time will come when some form of friction clutch will be adopted, but it hasn't yet got

here. The most successful one I ever contrived had a V groove in the interior of the rim of the fly-wheel, and by having this groove with its sides at a small angle I did not have to push the shoe out with very much force to get my friction. It locked itself tighter by an eccentric the harder the pull upon it, and was unlocked by the action of the trip connected with the treadle. It worked beautifully in the shop for several weeks, but there were practical difficulties with it when it got out among the tinmen, and they had sore trouble. The consequence was we took it off and put on an ordinary clutch. I can't help thinking it was all the tinmen's fault—but I've not yet built any more.

Mr. Grant.—The press of which I spoke has run ten hours a day for a year—hard work, too.

Mr. Stiles.—The time Mr. Grant has used his press is so short, compared with the time a well-made press will remain in good working order, judging by those which have been in constant use for the last twenty or twenty-five years, it seems quite too soon to attribute to the elastic clutch all Mr. Grant claims for it.

Mr. Oberlin Smith.—A theoretically perfect connection between a press fly-wheel and shaft would be a clutch device which was elastic when the engagement was made, but which would lose its elasticity during the down stroke before the dies commenced their work. By that time the engaging device should be automatically locked so that there could be no "back-lash" when the stresses were reversed in direction. There should, however, be an easy way of arranging a useful back-lash, through an angle of several degrees, when it was desired to back out a "stalled" pair of dies by jerking the fly-wheel by hand, and utilizing its momentum as in a hammer; and this is an important practical point, often lost sight of. It must be remembered that this angular space, or looseness, is (in all ordinary clutches) necessary, also, to give *time* enough, at fast speeds, for the engaging device to get home—that is, fully into gear. And, as before said, it is this very space that wants abolishing during a certain part of the stroke, providing the press is running in a *forward* direction, and by the *belt*, rather than by hand. Furthermore, all this mechanism, with its temporary elasticity, its take-up and let-out devices, etc., should be engageable with the fly-wheel at any point—or at any rate at several points—during its revolution.

For some years past I have pondered upon various air-spring devices to get a temporary elasticity, but have thought of nothing

cheap enough to meet commercial conditions. Mr. Munger, one of the Standard Oil Co.'s superintendents, has recently carried out this idea successfully (from a mechanical point of view) by mounting upon one of my fly-wheels a piston and cylinder, lying tangentially between an inwardly projecting arm from the wheel rim and an outwardly reaching arm from the disk engaging the clutch—all being after the manner of a pneumatic door-spring, or the ordinary dash-pot. I fear, however, that the average power-press buyer would not be willing to pay for all this—and mightn't keep it in good order when he got it. In the case just mentioned (Mr. Munger's) such a device was unusually necessary, because the shaft driven by the clutch was geared to other fast-running shafts which were heavily loaded, and the inertia was consequently great.

CCLXXII.

AN INVESTIGATION AS TO HOW TO TEST THE STRENGTH OF CEMENTS.

BY JEROME SONDERICKER, BOSTON, MASS.

INTRODUCTION.

The following paper gives an account of an investigation made in the laboratory of Applied Mechanics of the Massachusetts Institute of Technology by Mr. Jerome Sondericker, instructor in the department, in order to determine what are the proper methods to be pursued in testing the strength of cements. While the investigation is not yet complete, it is believed that a perusal of the tables of results will show that by means of the apparatus thus far devised, a large part of the irregularity common in such tests and usually attributed to lack of homogeneity in the cement itself, has been eliminated, and, hence, that a much larger portion of such irregularities than has generally been imagined must have been due to imperfections in the methods of testing cement that are in common use. It is presented in order to bring the matter before the Society, and to elicit discussion.

GAETANO LANZA,
(Member of the Society.)

Within the past year there have been designed in the Laboratory of Applied Mechanics of the Massachusetts Institute of Technology apparatus for testing the compressive and tensile strength of cement which are believed to possess some valuable features. A description of the apparatus and some of the results obtained from them will be presented in this paper.

COMPRESSION APPARATUS.

The compression apparatus was designed for testing not only cement, but also stone, iron, and other materials of construction.

It was made to be used with an Olsen 50,000 lb. testing machine. A side view of it is shown in Fig. 44. The two pieces of steel, *A* and *B*, are fitted to the slot in the middle of the movable cross-head of the machine and are bolted together by two ½ inch bolts. These pieces receive the upper compression plate, *D*, which is secured to them by means of the center bolt, one inch in diameter. This plate is five inches in diameter. It is provided with a spherical bearing of two and one-half inches radius, the center of the spherical surface being at the center of the lower surface of the plate. The washer, *C*, is also provided with a spherical bearing surface concentric with the other, in order that it may have a firm seat in any position of the plate. The lower compression plate is a cylinder one and one-half inches thick, and five inches in diameter. It is doweled to the platform of the machine, its center being exactly under the center of the upper plate. Concentric circles are described on its upper surface to admit of centering the specimen to be tested. Both plates are made of steel, their working surfaces being hardened and ground plane.

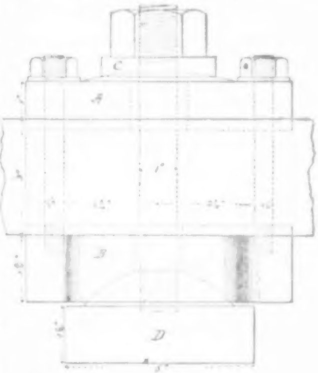


Fig. 44.

Two requisites of apparatus for testing the compressive strength of specimens provided with plane bearing surfaces are as follows:

1st. When adjusted in position, the plates shall bear uniformly over the whole surface of the specimen.

2d. During the progress of the test the moving plate shall remain parallel to its first position.

These conditions were designed to be satisfied, as nearly as possible, in the apparatus just described by the following means:

1st. By providing the spherical bearing so as to make the upper plate adjustable.

2d. By making the radius of this bearing sufficiently large, so that the friction due to the pressure exerted, aided, if necessary, by tightening the nut of the center bolt, would be sufficient to prevent slipping from taking place, due to any eccentric pressure; such as would occur, for example, in the testing of a long iron specimen as a result of its side deflection when near the point of rupture.

In order to secure a uniform bearing over the surface of the

specimen, the plate is suspended loosely in its socket; then, the specimen having been placed in position on the lower plate, the upper plate is brought down upon it slowly, at the same time being adjusted by hand approximately parallel to the surface of the specimen. As soon as the plate comes in contact with the specimen, it tips about its center until it comes to an even bearing.

In order to determine whether the friction of this spherical bearing would be sufficient to prevent slipping in ordinary testing operations, in case the pressure should become eccentric, a wrought iron specimen five inches long and one inch in diameter was tested. At intervals during the progress of the test, the distances between four points, 90° apart, near the outer edges of the plates, were calipered with a micrometer caliper reading by graduation to thousandths of an inch and by estimation to ten thousandths. By this means no slipping could be detected, even after the pressure had become eccentric to the amount of one-eighth of an inch, due to the side deflection of the specimen. This would correspond to a frictional resistance in the bearing of about .04 of the normal pressure. A second experiment was made to determine the amount of eccentricity which would just produce slipping in the joint when not oiled. A cylinder of iron, one inch in diameter, was submitted to pressure on its curved surface, and the eccentricity necessary to produce slipping under various loads was noted. The test was performed as follows: The plate was clamped in position sufficently firmly to prevent slipping under the eccentric load, by tightening the nut on the center bolt. The specimen was then placed eccentrically in the machine and subjected to various pressures up to 10,000 lbs. After applying the pressure the nut was loosened, thus allowing the friction of the ball joint to act alone in resisting the moment of the eccentric load. It was found by this means that, for all the loads tried, the plate began to slip when the eccentricity amounted to three-eighths of an inch. This would correspond to a coefficient of friction in the joint of about .12. No use has been made thus far of the center bolt except for suspending the plate, and for this purpose, of course it might have been made much smaller together with the plate *A*. It is desirable, however, to have some means for fixing the plates parallel to each other, and also for providing for extremely eccentric pressures. The center bolt answers these purposes. It will be noticed that as the center of the spherical joint is in the center of the lower face of the plate, this point remains stationary in all positions of the plate, so that the

pressure upon it is central however it may be inclined; also, the minimum amount of sliding between the plate and specimen occurs as the plate is coming to a bearing.

In testing a cube of cement, two opposite surfaces are rubbed down with water on a piece of slate in order to remove any small surface irregularities. The load is at first applied rapidly up to a point well within the breaking load, and then slowly and at a uniform rate of speed till fracture takes place. In order to ascertain the degree of uniformity of results to be obtained in the use of this apparatus, a number of tests have been made of two-inch cubes of both Portland and Rosendale cement, with and without sand, and from one week to three months old. Four cubes were made at one mixing and all tested at the same time. Table I. gives the results of these tests, including the mean breaking load for each set of four specimens, the maximum variation of any one test from the mean, and the extreme variation occurring in each set. It will be noticed that the variations are quite small, the average maximum variation from the mean being 1.9% and the average extreme variation 3%. No extraordinary precautions were employed in making these tests; they are believed to represent what may be expected in the ordinary use of the apparatus.

TENSION APPARATUS.

It is a well-known fact that the means commonly employed in determining the tensile strength of cement give results varying within wide limits, variations of 20% and even 30% in the strength of specimens of neat Portland cement from the same mixing being quite common, and this in the hands of careful experimenters. An examination of the report of Maclay's experiments, published in the Transactions of the American Society of Civil Engineers for 1877, shows extreme variations in the sets of five specimens of neat Portland cement, having a sectional area of $2\frac{1}{4}$ square inches, as high as 35%; differences of 20% and more occurring frequently. These wide variations are assignable, partly to the impossibility of mixing the mortar and filling the molds so as to obtain briquettes of exactly the same strength, and partly to inexact methods of testing.

Experiments have been made in the laboratory with the view of reducing that portion of these variations resulting from the methods of testing employed. For this purpose clips have been de-

signed which are intended to eliminate eccentric pulls in applying the load.

Before giving a description of these clips, a few explanations will be made. The form of briquette in use in the laboratory is that shown in Fig. 45. The outline of the neck and bearing surfaces consists of circular arcs of one inch radius, terminated by straight lines tangent to them and having a slope of 1 to 2 with the axis of the briquette. The sectional area at the neck is one square inch. The clips first in use had flat bearing surfaces having the same slope as the sides of the briquette. They came in contact with nearly the whole of the flat portion of the sides of the specimen, leaving a free space of about one inch between their points. Upon trial it was found that quite a large percentage of fractures occurred at the edge of the clips. The following experiment was then made: Four narrow, thin strips of steel were inserted between the clips and briquette, so that the briquette bore upon these strips instead of upon the surface of the clips. It was found that as the strips were moved farther away from the neck of the specimen, the number of fractures occurring outside of the smallest section diminished and at the same time the breaking strength increased, until when the strips were placed two inches apart nearly all the fractures occurred at the smallest section. Two inches was therefore selected for the distance between the bearing points of the clips for this form of briquette. The clips shown in Fig. 45 were then designed.

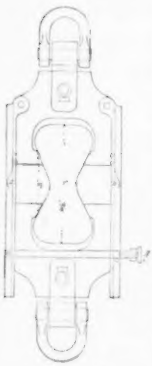


FIG. 45.

Two guides, grooved to a depth of $\frac{1}{8}$ inch are hinged to the upper clip. The lower clip is secured in the guides by means of the clamp screw A; the springs B, attached to the upper clip, release the lower clip from the guides when they are unclamped. A central pull is secured by means of two pairs of knife-edges at right angles to each other, the outer pair being inserted in the yokes which bear on the inner ones. The yokes are prevented from moving sideways by means of projecting points bearing against the sides of the clips. The bearings of the yokes on the inner pair of knife-edges are notched as shown in the figure; thus the yokes occupy a definite position. The outer knife-edges are adjusted so that their plane passes exactly through the axis of the clips, as are also the inner ones; thus the line of pull, which is determined by the intersection of the planes of the two pairs of

knife-edges, is made to pass through the axis of the clips when these are clamped. The bearing surfaces of the clips upon the briquettes are rounded, forming blunt points.

These rounded bearing surfaces must be made accurately and used intelligently in order to secure reliable results. The points must be exactly central, and the bearing surfaces of the briquette square with its face; even then, the adjustment of the specimen in the clips by hand, in the ordinary manner, can only secure an approximately central pull, varying with the degree of care exercised by the operator and the conditions of the bearing surfaces of the briquette. The object designed to be attained by the clips just described is to secure a definitely central pull, at the same time making the task of adjusting the specimen in the clips as simple as possible. The operation of testing is as follows:

In order to prevent the lower clip from dropping when the specimen breaks, a loosely fitting brass band is slipped over the briquette before it is placed in the clips. The lower clip is raised sufficiently to allow the specimen to be placed in position, and clamped firmly enough to support its own weight. The specimen is inserted and the lower clip drawn down till the bearing surfaces of the briquette are just in contact with it. The briquette is then adjusted so that its faces are flush with the faces of the clips. A load usually about one-half the breaking load is rapidly applied. This forces the blunt points of the clips into the specimen, thus obtaining a firm bearing before the guides are removed. The guides are unclamped, the springs freeing them from contact with the lower clip. The stress is then applied slowly and at a uniform rate till fracture takes place. Before taking out the fractured specimen, the lower clip is clamped in position ready for another test. The only hand adjustment is in placing the specimen flush with the faces of the clips. The only possible eccentricity will be caused by the imbedded points not acting at the center of the specimen; if the points are themselves central and the sides of the briquette square, this will not occur. If, however, it does, it can be detected by the clips springing to one side when the guides are unclamped. The grooves in the guides of the experimental apparatus are unnecessarily deep. With shallower grooves, admitting of more rapid clamping and unclamping of the guides, it is believed that the operation of testing may be performed fully as rapidly as by the usual method. The adjustment of the specimen in position is so simple a matter that the clips may be used by any careful workman with a certainty of

securing reliable results. The knife-edge bearings answer the same purpose that is claimed for conical points, and are less liable to be dulled by use with heavy loads.

In order to determine the degree of uniformity in the results obtained by the use of this apparatus, 117 specimens have been tested. They include both Portland and Rosendale cement, with and without sand, and from 7 to 28 days old. The briquettes were made in sets of four, four molds being filled from one mixing. Three sets of specimens were made at the same time from three different mixings, using the same ingredients in all and attempting to obtain the same quality of mortar. Table II. gives the results of these tests, including the mean strength of each set of briquettes, the maximum variation from this mean in pounds and per cents, and the extreme variation occurring in each set in pounds and per cents. The three sets of specimens made the same day with the same ingredients are grouped together and marked *a*, *b*, and *c*. Four of the specimens tested are excluded from the table; these are reported in the column of remarks, with the reasons for their exclusion. It will be seen from this table that the maximum variation from the mean is included between 0.6% and 16.3%, with an average of 5.7%. The extreme variation in strength in each set is from 1.2% to 25.6% with an average of 9.7%. The cement was in each case unsifted, and the sand (ordinary coarse sand) sifted but not washed. The mixture was in each case of about the consistency of ordinary mortar, 25% of water being used with the neat Portland and 37½% with the neat Rosendale cement. It is believed that these results represent what may be expected in ordinary testing operations by the use of the clips described.

EFFECT OF AN ECCENTRIC LOAD.

In order to determine the effect of a slightly eccentric pull, the experiments recorded in Table III. were made. Briquettes of the same mixing are grouped together. In the sixth test, the briquette showed a large cavity near one edge of the fractured section. This, doubtless, decreased its strength considerably. The effect of such a cavity cannot be estimated wholly by the area it occupies, as it also causes a non-uniform stress at the section. Briquette No. 8 was first subjected to a central stress of 640 lbs. without fracture. It was then replaced in the clips $\frac{1}{16}$ inch out of center, and broke at 528 lbs. as recorded in the table. Omitting the second group, this

experiment shows a decrease in strength due to a pull $\frac{1}{16}$ inch out of the axis of the briquette of 12% for the first group, 17% for the third and fourth, and 10% and 9% for the fifth and sixth groups respectively. The ordinary formula for eccentric loads would give a decrease of strength of 27%. A greater number of tests would be necessary to determine the exact percentage of decrease of strength due to a slight eccentricity, but these serve to show the importance of guarding against eccentric stresses in cement testing, however slight they may be.

The machine used in making all the tensile tests was a single lever suspended in a wooden frame, the loads applied being indicated by a spring balance. This machine was tested at the conclusion of these experiments and found to be correct.

The form of briquette used in making these tests is somewhat more slender at the neck and considerably longer than that recommended by the Committee on Cement Testing of the American Society of Civil Engineers. No comparative tests have been made to determine the relative value of the two forms. There are two qualities, developed by testing, which a cement briquette should possess. 1. The fracture should take place at the smallest section. 2. The breaking strength should be the maximum obtainable, the mortar being of ordinary stiffness. The latter requisite can only be determined definitely by comparative tests of different forms of briquette. In the tests just described, out of the 117 specimens tested, 88 specimens, or 75%, broke at or within $\frac{1}{8}$ inch of the smallest section; 20 specimens, or 17%, at a distance of about $\frac{1}{4}$ inch; 5 specimens, or $4\frac{1}{2}$%, at about $\frac{3}{8}$ inch; 4 specimens, or $3\frac{1}{2}$%, at about $\frac{1}{2}$ inch distance from this section. The areas of the cross sections of the briquettes at these points are, at $\frac{1}{8}$ inch, 1.015 sq. inches; at $\frac{1}{4}$ inch, 1.06 sq. inches; at $\frac{3}{8}$ inch, 1.15 sq. inches, and at $\frac{1}{2}$ inch, 1.26 sq. inches. None of the specimens broke within $\frac{1}{2}$ inch of the bearing points of the clips. Further investigation will be made in this direction.

TABLE I. COMPRESSION TESTS OF 2-INCH CUBES.

Cement.	Age.	Breaking load on 4 sq. in. Mean of 4 specimens. Lbs.	Max. var't'n from Mean.		Extreme variation.		Remarks.
			Lbs.	%	Lbs.	%	
Portland, neat.	95 d.	22,540	440	2.0	742	3.3	
" "	88 d.	18,981	181	1.0	270	1.4	
" "	77 d.	15,982	82	0.5	145	0.9	
" "	9 d. 6 h.	9,534	69	0.7	122	1.3	
" "	9 d. 1 h.	9,473	97	1.0	179	1.9	
" "	28 d.	21,614	101	0.5	180	0.9	
" "	14 d. 5 h.	17,867	242	1.3	375	2.1	
" "	13 d. 20 h.	16,469	269	1.6	450	2.7	
" "	7 d. 1 h.	10,061	164	1.6	225	2.2	
" 3 pts. sand.	14 d.	2,269	54	2.4	92	4.0	
Rosendale, neat.	76 d.	9,226	159	1.7	255	2.8	
" "	24 d.	4,210	310	7.4	470	11.1	
" 1 pt. sand.	14 d.	1,429	37	2.6	70	5.0	
		Mean		1.9		3.0	

TABLE II. TENSION TESTS. (*Briquettes* 1 *sq. inch section.*)

Cement.	Age.	Breaking Load. Mean of 4 specs. Lbs.	Max. variation from Mean.		Extreme variation.		Remarks.
			lbs.	%	lbs.	%	
Portland, neat.	28 d.	(a) 616	28	4.5	52	8.4	
" "	28 d.	(b) 651	15	2.3	24	3.7	Mean of 3 specs. 4th broke in large cavity at 492 lbs.
" "	28 d.	(c) 620	26	4.2	50	8.0	Mean of 3 specs. 4th broke in large cavity at 558 lbs.
" "	14 d.	(a) 490	34	7.0	60	12.2	
" "	14 d.	(b) 523	55	10.5	82	15.7	
" "	14 d.	(c) 521	19	3.6	36	6.9	
" "	13 d. 19 h.	(a) 573	27	4.7	54	9.4	
" "	13 d. 19 h.	(b) 561	35	6.2	58	10.3	Mean of 3 specs. 4th placed eccentrically in clips and broke at 482 lbs.
" "	13 d. 19 h.	(c) 550	26	4.7	46	8.4	
" "	7 d.	(a) 322	8	2.5	13	4.0	
" "	7 d.	(b) 324	3	0.9	4	1.2	Mean of 3 specs. 4th subjected to eccentric load and broke at 228 lbs.
" "	7 d.	(c) 312	2	0.6	4	1.3	
" 3 pts. sand.	14 d.	(a) 89	7	7.9	12	13.5	Mean of 3 specs. 4th broke in taking out of mold.
" "	14 d.	(b) 90	7	7.8	12	13.3	" " "
" "	14 d.	(c) 86	14	16.3	22	25.6	
Rosendale, neat.	24 d.	(a) 176	8	4.5	14	8.0	
" "	24 d.	(b) 170	10	5.9	20	11.8	
" "	24 d.	(c) 176	5	2.8	8	4.5	
" "	14 d. 4 h.	(a) 94	8	8.5	12	12.7	
" "	14 d. 4 h.	(b) 97	2	2.0	3	3.1	
" "	14 d. 4 h.	(c) 88	8	9.1	12	13.6	
" "	13 d. 19 h.	(a) 76	3	4.0	5	6.6	Mean of 3 specs. 4th broken accidentally.
" "	13 d. 19 h.	(b) 84	5	6.0	9	10.7	
" "	13 d. 19 h.	(c) 88	6	6.8	10	11.4	
" "	9 d.	(a) 82	2	2.5	4	5.0	
" "	9 d.	(b) 67	3	4.5	6	9.0	
" "	9 d.	(c) 67	5	7.5	10	15.0	
" 1 pt. sand.	14 d.	(a) 60	4	6.7	7	11.7	
" "	14 d.	(b) 59	4	6.7	7	11.7	
" "	14 d.	(c) 57	5	8.8	8	14.0	
		Mean		5.7		9.7	

TABLE III. EFFECT OF AN ECCENTRIC LOAD.

Cement.	No of Test.	Age.	Breaking Load.	Remarks.
Portland, neat.	1	107 d.	508	Stress central.
	2	107 d.	516	Stress central.
	3	107 d.	444	Stress $\frac{1}{16}$ inch eccentric.
	4	107 d.	450	Stress $\frac{1}{16}$ inch eccentric.
Portland, neat.	5	23 d.	596	Stress central.
	6	23 d.	372*	Stress $\frac{1}{16}$ inch eccentric.
Portland, neat.	7	23 d.	632	Stress central.
	8	23 d.	528†	Stress $\frac{1}{16}$ inch eccentric.
Rosendale, neat.	9	24 d.	130	Stress central.
	10	24 d.	108	Stress $\frac{1}{16}$ inch eccentric.
Portland, neat. Means of 6 specimens.	11	12 d.	423	Stress central.
	12	12 d.	379	Stress $\frac{1}{16}$ inch eccentric.
Rosendale, neat. Means of 6 specimens.	13	11 d.	86	Stress central.
	14	11 d.	78	Stress $\frac{1}{16}$ inch eccentric.

DISCUSSION.

Prof. Jas. E. Denton.—Will you say how much the eccentricity is for the present arrangement?

Prof. Lanza.—With usual cement testing machines in the market, where the clips are loose, the eccentricity will vary with the skill and care of the experimenter, and hence will not be the same in any two trials. How great will be its maximum value with any one special machine it is impossible to predict. To show the effects of eccentricity, Mr. Sondericker took our clips and made some tests where he first put the briquettes in centrally, and then put them in with an eccentricity of a sixteenth of an inch; the loss in strength was very considerable, and the results are given in the paper.

Mr. H. de B. Parsons.—I should like to make a few remarks on this paper, as it is important, since the branches of civil engineering and mechanical engineering are approaching in one or two points, and especially in the problems in which cement is the chief factor. In regard to the clip proposed by Prof. Lanza and Mr. Sondericker, those side bars or side clips, in order to keep the specimen straight, I think will be a serious objection in use in a laboratory where a great many tests have to be made for work in which large quantities of cement are used; and the time required to keep those side bars cleaned must be considerable.

* Broke in a large cavity near edge of fractured section.

† Subjected to a central stress of 640 lbs. This was then removed and the eccentric stress applied.

The great objection to the method proposed by the paper is stated on page 177—"A load, usually about one-half the breaking load, is rapidly applied." Cement is too brittle to have any load applied to it suddenly and then left to stop even for an instant's rest, and then the load applied a second time until the specimen is broken. I have myself broken a very large number of cement specimens, and at one time, through carelessness, I ran the wheel of the machine too far, so that I did not let the weight counterbalance. I stopped turning the wheel for a moment until I ran the weight out, and the moment I turned the wheel again and put on more stress, the specimen broke. Now, as I had a number of other specimens made at the same time, I immediately put some in the machine, and they broke on an average of 100% higher. Undoubtedly the reason of the first briquette's apparent weakness was due to the irregularity of applying the load. I afterwards made some experiments on this point, and found that you could not rely on the strength of your cement, if, at any time during the testing, you stopped the pressure even for an instant. So far as eccentricity goes, I think that the present method of using cone points or knife edges at right angles prevents the specimen from getting very far from the central line; and that the great difference shown in the results of tests is due more to carelessness in mixing than to mechanical difficulties, although the present form of briquette is far from perfect. The mixing of cement with water is very often not done in a uniform manner; and as cement receives its strength from chemical reaction, it is necessary that the briquettes should all be subjected to exactly the same conditions in order to obtain exactly similar results. Also in common practice, the cement briquettes are often placed in zinc troughs. The zinc troughs, with the cement and water, which is apt to be impure, have a certain amount of chemical reaction; exactly how much I am not prepared to say, but I take the precaution of always putting my briquettes on glass. Another cause for difference in results of tests is the practice of disturbing the briquettes before they are fully set; and the cement becomes injured internally. Now, as it takes many months for the chemical reactions finally to complete their work, this disturbance in 15 or 20 minutes is much too soon. I recommend that the cement be left in the molds for at least 24 hours, and covered with a damp cloth—not a wet cloth—so as to prevent the upper surface of the briquette from drying more rapidly than the rest. Now if these precautions are

followed out, the results will be found to agree wonderfully well. I have myself made tests where there were four specimens, and two broke at exactly the same figure; one specimen broke at two pounds less, and the other at eight pounds above, and the average pressure upon each was about 280 pounds. In Prof. Lanza's experiments, the close agreement of his results are to be attributed, in my opinion, rather to the precautions taken for a uniform treatment of his briquettes, than to the mechanical devices described in his paper.

Prof. Denton.—The remarks of the last speaker give a good many instances of how delicate a matter it is accurately to measure the tensile strength of cement. I have had an opportunity to verify that. The whole question of testing cement is a very delicate one. The tensile strength has very little connection with the usefulness of the cement, but it governs whether the cement, after having been made as carefully as possible, shall be accepted or not, and any refinement which can reduce the accidental error so that duplicate tests can more nearly agree is a very valuable one. It can only be arrived at by putting on such adjustments as Prof. Lanza has explained. I think the work is therefore a very valuable contribution.

Prof. Lanza.—In regard to Mr. Parsons' first criticism, there is no difficulty in keeping the side bars or guides clean, for they never get dirty in use, as nothing comes in contact with them to make them dirty.

As to his criticism on our applying one-half the breaking load rapidly, it seems to me that the word rapidly must convey to him a different impression from that intended; it is merely meant that this portion of the load is applied more rapidly than the remainder. Nevertheless Mr. Parsons seems to think that the mere fact of stopping to loosen the guides will vitiate the results. However this may be, it is not necessary that any stoppage should be made in the testing, and hence this is not a criticism of the apparatus used and advocated in the paper. In point of fact, however, a stoppage when the load has not yet passed half the breaking load cannot have (of itself) at all as great an effect in producing lack of uniformity as Mr. Parsons implies; for, in our tests we do not get (although we have made the stoppage) any such large variations.

Mr. Parsons then attributes our uniform results to our care in securing a uniform treatment of the briquettes rather than to the

method of making the tests. We do necessarily take every precaution to secure a uniform treatment, knowing full well that without that we could not secure uniformity of results. But if the apparatus had no influence in producing uniformity of results we should not obtain any greater regularity than others who have taken equal care in the treatment.

As a matter of fact, however, Maclay, who is, I suppose, one of the most careful experimenters with the ordinary machinery, obtained a variation of 20% very frequently, and sometimes as much as 35%, while we do not get such variations.

CCLXXIII.

FRICTION IN TOOTHED GEARING.

BY GAETANO LANZA, BOSTON, MASS.

(Member of the Society.)

BEFORE presenting to the society a paper so full of long equations, a brief statement will be made of the reasons that led to writing it, the assumptions on which the work is based, and the conclusions which seem to be warranted by the results of the computations. At the last annual meeting a paper * on the efficiency of gear teeth was presented, written by Prof. Reuleaux, and certain conclusions were drawn by him, one of which was that the work lost in friction is greater with the involute than with the epicycloidal form of tooth. This conclusion, as well as some of the others which he drew, was challenged by several of the members of the society, who, making their computations by other methods which they claimed were more exact, drew precisely the opposite conclusions from Prof. Reuleaux in the cases referred to, and also in other cases.

Moreover, Prof. Reuleaux's work, and also all the reasoning of his opponents, were based on purely theoretical grounds, and all the solutions presented were approximate. Indeed, all the solutions that the writer has seen in print, including those of Rankine, Herrmann, and Moseley, are only approximations.

The present investigation was undertaken in order to determine, if possible, who, if any, were right in their conclusions.

While the writer is one who believes that this question can only be correctly answered by experiment, and that the experiments to be made will require very delicate measurements, and a more accurate experimental knowledge than is now possessed of the small errors of dynamometers, nevertheless, in the present paper, the reasoning is upon a purely theoretical basis, but no approximations have been made in the deduction of the formulæ for the work lost in friction, and the consequent efficiency of the gears when the friction of the journals in their bearings is not taken into account,

* No. CCXVII. Trans. A. S. M. E., Vol. VIII., p. 45.

this being the only case to be considered here. Hence the correctness of the results depends only upon the correctness of the assumptions made to start with, and these will now be discussed. Although the present paper deals fully as much with the practical cases where more than one pair of teeth are in action at once, as with the case where only one pair is in action at once, nevertheless, the assumptions made in this latter case will be discussed first, as that is the only one that is dealt with by Reuleaux and the other writers referred to above.

The efficiency of any pair of gears will be different for every different distribution of the pressure, and this will, when only one pair of teeth is in action at once, vary with the variation of the work done by the following, and the variation of the energy imparted to the driving gear of the pair. To compute the efficiency of a given pair of gears for every possible distribution of the pressure that can occur in practice would be an impossibility. Hence it would seem to be reasonable to adopt for our assumption in this regard, one which should be fulfilled in a large number of practical cases, and we should then have an exact solution for these cases, whereas, for gears under different conditions the results of our computations can only be considered as approximate.

The assumption made in this paper, whether one pair or more than one pair of teeth are in action at once, is that the moment of the resistance is constant. This is equivalent to assuming that the work done by the following gear is constant. The above, and the ordinarily accepted laws of friction are the only assumptions made in the case when only one pair of teeth is in action at once.

When two pairs of teeth are in action at once, it becomes necessary to make another assumption in addition to that of a constant moment of resistance, as this alone does not determine the way in which the work is divided between the two pairs of teeth in action. Formulæ are worked out in this paper, based upon each of the two following assumptions, viz.:

(*a*) That the work done, and hence the moment of the resistance, is at all times equally divided between the two pairs of teeth in action.

(*b*) That the moment of the resistance is so divided between the two pairs in action as to cause them to wear equally; this being the assumption proposed by Mr. Hugo Bilgram.

When (*b*) is used, a third one has to be made, and that adopted here is that the wear, as estimated in a normal direction, is propor-

tional to the product of the normal pressure and the velocity of sliding.*

By way of a summary we may say that when assumption (*a*) is used the assumptions made are the following:

1. The ordinary friction theories.
2. The constancy of the moment of resistance.
3. The equal division of the moment of resistance between the two pairs of teeth in action.

On the other hand, when (*b*) is used the assumptions made are the following:

1. The ordinary friction theories.
2. The constancy of the moment of resistance.
3. The condition of equal wear.
4. The proportionality of the normal wear to the product of the normal pressure and the velocity of sliding.

Before discussing the results which will be found later in this paper, it will be observed that, after the deduction of formulæ for the efficiency of gears in the case where only one pair of teeth is in action at once, the numerical efficiencies of certain special cases of epicycloidal and involute teeth have been computed, which will be found on pages 195, 196; and it seems to the writer that from a consideration of the formulæ and examples we may fairly conclude:

1. That whether the epicycloidal or the involute form of tooth is the most efficient depends upon the proportions used for each, and that either one may be more efficient than the other according to the way these proportions are chosen.
2. That the efficiency of involute gears is not, as has been claimed by Mr. George B. Grant and others, independent of the obliquity; the reason why they have drawn an erroneous conclusion in this regard being the approximations which they have introduced into their work.
3. The differences between the efficiencies deduced for the four cases computed are so small that it seems to the writer that they could be easily masked by any of the following matters:

(*a*) By any variation in the coefficient of friction.

(*b*) By any inaccuracy in our friction theories.

(*c*) By any irregularity in the distribution of the pressure.

4. Hence follows the conclusion already stated, that the question can only be correctly answered by experiment.

* This the writer supposed to be in accordance with a suggestion made by Mr. Bilgram. See Trans. Am. Soc. Mech. Eng'rs, Vol. VIII., page 76.

CASE WHEN MORE THAN ONE PAIR OF TEETH ARE IN ACTION AT ONCE.

As to a choice between the two assumptions of equal division of work, or of equal wear, we must observe:

1. The formulæ based upon the first are identical with those deduced for the case where only one pair of teeth is in action at once, and they are far simpler than those based upon the last assumption.

2. Although the arguments advanced by Mr. Hugo Bilgram in favor of the equal wear theory possess some plausibility, it is not at all certain that the wear is equal.

3. The assumption that the normal wear is proportional to the product of the normal pressure and the velocity of sliding, which we make use of when we adopt the equal wear theory, is in accord with what may be called the more modern theory of friction, and although this last is more popular than the older theory at present, nevertheless it is known that in certain cases of pivot friction it leads to conclusions which are manifestly absurd.

4. Finally, an inspection of the table on page 204, in which are determined the efficiencies of what the writer believes to be the Brown & Sharpe system of epicycloidal and involute gears, will show such small differences between the two forms, that as it seems to the writer, they might be easily masked by any of the matters mentioned before as liable to mask the differences when only one pair of teeth is in action at once.

5. The smallness of the differences between the results of making the computations by the two different assumptions would warrant our using the first, on account of its greater simplicity, whenever it is desired merely to determine the efficiency of the gears; and, as has been already stated, it does not seem to the writer that we are warranted in making comparisons between different kinds of gearing, on the basis of such small differences as those in this table. The formulæ will now be deduced, and the tables of results given. In the case, however, of the assumption of equal wear, the formulæ will be given, but their deduction will be omitted on account of its length.

In this discussion the friction of the journals on their bearings is left out of consideration, and the weight of the gears is assumed to be without appreciable influence, and the notation used is substantially that of Moseley.

Let the center of the driver be B; and that of the follower, C. Let P_1 be the driving force, its leverage being

$$BD = a_1.$$

Let P_2 be the resistance, its leverage being

$$CE = a_2.$$

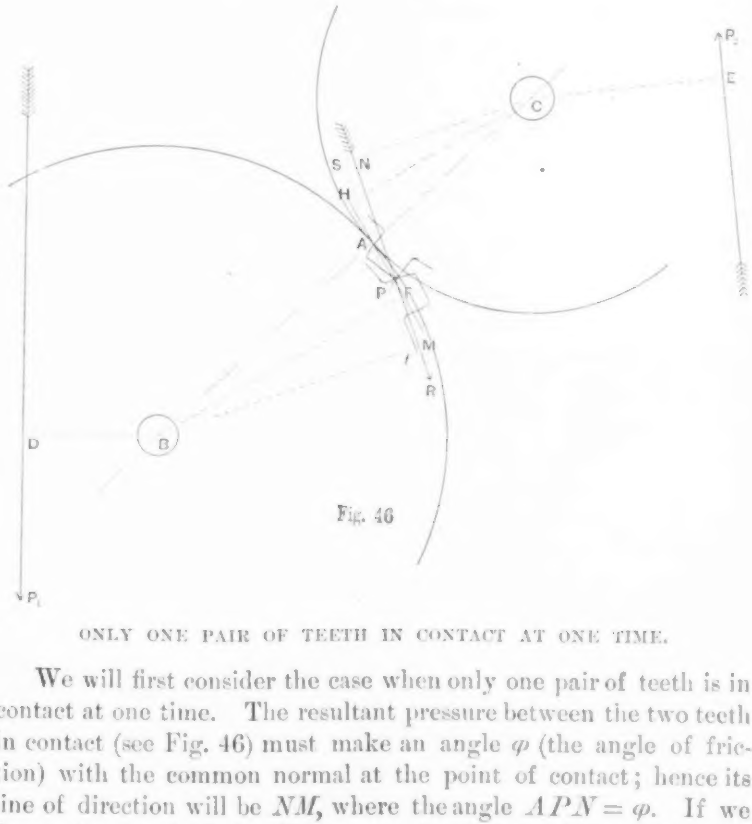


Fig. 46

ONLY ONE PAIR OF TEETH IN CONTACT AT ONE TIME.

We will first consider the case when only one pair of teeth is in contact at one time. The resultant pressure between the two teeth in contact (see Fig. 46) must make an angle φ (the angle of friction) with the common normal at the point of contact; hence its line of direction will be NM, where the angle $APN = \varphi$. If we denote its magnitude by R, we must have, from the conditions of equilibrium, if we let $BM = m_1$ and $CN = m_2$,

$$P_1a_1 = Rm_1, \tag{1}$$

$$P_2a_2 = Rm_2. \tag{2}$$

Hence,

$$\frac{P_1a_1}{P_2a_2} = \frac{m_1}{m_2}. \tag{3}$$

And these equations must always hold, whatever the form of the teeth.

We must next determine the values of m_1, and m_2, observing that the expressions which represent them during the approach will be different from those which represent them during the recess. Their values during the approach may be deduced as follows (see Fig. 46):

Let $BA = r_1$, $CA = r_2$, $AP = \lambda$, this being the length of the line joining the pitch point with the point of contact, and let the angle $PAB = \theta$. Then we have from the figure,

$$m_1 = BM = Bt + tM = r_1 \sin BAt + \lambda \sin PAt;$$

or
$$m_1 = r_1 \sin(\theta - \varphi) + \lambda \sin \varphi. \tag{4}$$

$$m_2 = CN = CS - SN = r_2 \sin SAC - \lambda . \sin APN;$$

or
$$m_2 = r_2 \sin(\theta - \varphi) - \lambda \sin \varphi. \tag{5}$$

On the other hand, we shall have, during the arc of recess (see Fig. 47), the equations

$$m_1 = r_1 \sin(\theta + \varphi) + \lambda \sin \varphi. \tag{6}$$

$$m_2 = r_2 \sin(\theta + \varphi) - \lambda \sin \varphi. \tag{7}$$

By substituting these values of m_1 and m_2 in equations (1), (2), and (3), they will assume the following forms, which hold, whatever be the forms of the teeth.

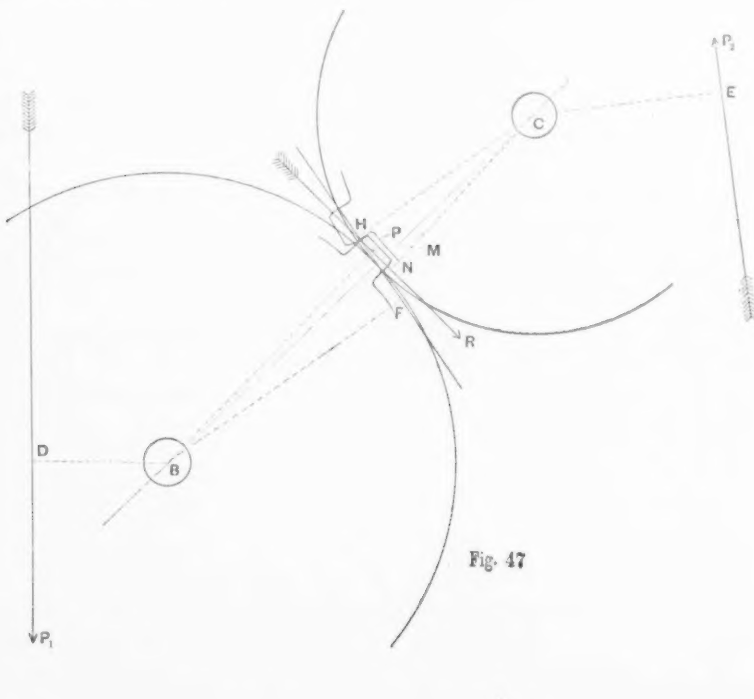


Fig. 47

Approach:

$$P_1a_1 = Rr_1 \sin(\theta - \varphi) + R\lambda \sin \varphi. \tag{8}$$

$$P_2a_2 = Rr_2 \sin(\theta - \varphi) - R\lambda \sin \varphi. \tag{9}$$

$$\therefore \frac{P_1a_1}{P_2a_2} = \frac{r_1}{r_2}\left\{\frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin(\theta - \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin(\theta - \varphi)}}\right\}. \tag{10}$$

Recess:

$$P_1a_1 = Rr_1 \sin(\theta + \varphi) + R\lambda \sin \varphi. \tag{11}$$

$$P_2a_2 = Rr_2 \sin(\theta + \varphi) - R\lambda \sin \varphi. \tag{12}$$

$$\therefore \frac{P_1a_1}{P_2a_2} = \frac{r_1}{r_2}\left\{\frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin(\theta + \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin(\theta + \varphi)}}\right\}. \tag{13}$$

Let now ψ denote the angle turned through by the follower since the time when the point of contact was on the line BC; then will the corresponding angle turned through by the driver be

$$\frac{r_2}{r_1}\psi,$$

and we shall also have

$d\psi$ = elementary angle turned through by the follower,

$\dfrac{r_2}{r_1}d\psi$ = elementary angle turned through by the driver.

Now let

ψ_1 = angle turned through by follower during approach.
ψ_2 = angle turned through by follower during recess.
U_1 = work done by driver during approach.
U_2 = work transmitted by follower during approach.
U_3 = work done by driver during recess.
U_4 = work transmitted by follower during recess.

Then we can readily obtain from (10) and (13) the following equations:

Approach:

$$U_1 = \int_0^{\psi_1} P_1a_1\frac{r_2}{r_1}d\psi = \int_0^{\psi_1} P_2a_2\left\{\frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin(\theta - \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin(\theta - \varphi)}}\right\}d\psi. \tag{14}$$

Recess:

$$U_3 = \int_0^{\psi_2} P_1 a_1 \frac{r_2}{r_1} d\psi = \int_0^{\psi_2} P_2 a_2 \left\{ \frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin (\theta + \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin (\theta + \varphi)}} \right\} d\psi. \quad (15)$$

And these two equations are entirely general; *i. e.*, they always hold, whatever the form of the teeth, and whatever assumption be made in regard to the way in which the pressure varies at different points in the path of contact.

Under the assumption of a constant moment of resistance, they become:

Approach:

$$U_1 = P_2 a_2 \int_0^{\psi_1} \left\{ \frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin (\theta - \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin (\theta - \varphi)}} \right\} d\psi. \quad (16)$$

Recess:

$$U_3 = P_2 a_2 \int^{\psi_2} \left\{ \frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin (\theta + \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin (\theta + \varphi)}} \right\} d\psi. \quad (17)$$

And we then have also

$$U_2 = P_2 a_2 \psi_1,$$
$$U_4 = P_2 a_2 \psi_2,$$

and from these equations we can find the efficiency

$$\frac{U_2 + U_4}{U_1 + U^3}.$$

EPICYCLOIDAL TEETH.

The only cases that will be discussed here are those where the same describing circle is used for the approach and recess.

Let $r =$ radius of the describing circle, and let

$$\frac{r_2}{2r} = e.$$

Then we shall have as shown in the figure, where

$$BA = r_1,\ CA = r_2,$$
$$DA = r,\ AP = \lambda.$$

Arc $AP = r_2\psi$.

$$\therefore \text{ angle } ADP = \frac{r_2}{r}\psi,$$

$$\text{and } \lambda = AP = 2r\sin\left(\frac{ADP}{2}\right)$$
$$= 2r\sin e\psi.$$

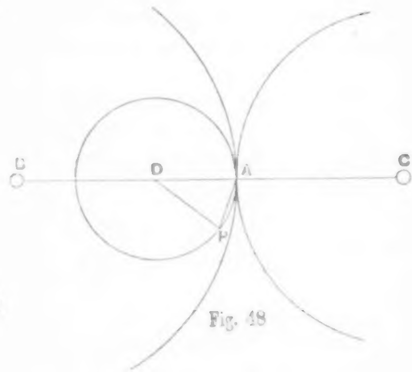


Fig. 48

Also it will be evident from the figure that

$$\theta = DAP = \frac{\pi}{2} - e\psi.$$
$$\theta - \varphi = \frac{\pi}{2} - (e\psi + \varphi).$$

$$\theta + \varphi = \frac{\pi}{2} - (e\psi - \varphi).$$

If we make these substitutions in equations (16) and (17), and make the necessary integrations, we shall obtain

Approach:

$$U_1 = \frac{\frac{P_2a_2}{e}\left\{\left[1+\left(1-\frac{2r}{r_1}\right)\left(1+\frac{2r}{r_2}\right)\tan^2\phi\right]e\psi_1-\left(\frac{2r}{r_1}+\frac{2r}{r_2}\right)\tan\phi\log_e\left(\cos e\psi_1-\left[1+\frac{2r}{r_2}\right]\tan\phi\sin e\psi_1\right)\right\}}{1+\left(1+\frac{2r}{r_2}\right)^2\tan^2\phi}. \quad (18)$$

Recess:

$$U_3 = \frac{\frac{P_2a_2}{e}\left\{\left[1+\left(1+\frac{2r}{r_1}\right)\left(1-\frac{2r}{r_2}\right)\tan^2\phi\right]e\psi_2-\left(\frac{2r}{r_1}+\frac{2r}{r_2}\right)\tan\phi\log_e\left(\cos e\psi_2+\left[1-\frac{2r}{r_2}\right]\tan\phi\sin e\psi_2\right)\right\}}{1+\left(1-\frac{2r}{r_2}\right)^2\tan^2\phi}. \quad (19)$$

Having found the values of U_1 and U_3 as above, we can readily deduce the efficiency whose value is

$$\frac{U_2 + U_4}{U_1 + U_3}.$$

When $r_1 = \infty$, or the driver becomes a rack, we merely substitute its value; certain terms drop out, and there is no indetermination.

When $r_2 = \infty$, or the follower becomes a rack, we must make certain substitutions as follows:

Let $A_1 = r_2\psi_1 =$ arc of approach.
Let $A_2 = r_2\psi_2 =$ arc of recess.
Observe that in this case $a_2 = r_2$.
Then we may write

$$\frac{P_2 a_2}{e} = 2Pr, \qquad e\psi_1 = \frac{A_1}{2r}, \qquad e\psi_2 = \frac{A_2}{2r}, \qquad \frac{r}{r_2} = \frac{2r}{r_2} = 0.$$

If these substitutions be made, we have the formula which applies when the follower is a rack.

Before making any numerical computations under this case, we will first deduce the corresponding formulæ for involutes.

INVOLUTE TEETH.

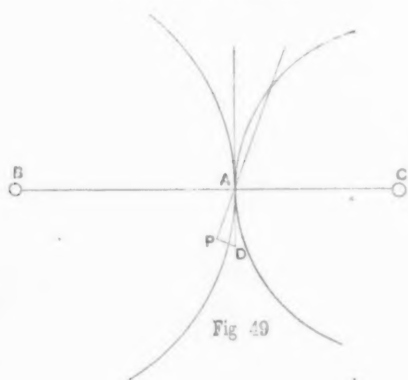


Fig 49

Let angle $PAD = \eta =$ the obliquity. Then if P is the point of contact corresponding to the angle ψ turned through by the follower, we shall have

$$\lambda = AP = r_2\psi \cos \eta,$$

$$\theta = PAB = \frac{\pi}{2} - \eta,$$

$$\theta - \varphi = \frac{\pi}{2} - (\eta + \varphi),$$

$$\theta + \varphi = \frac{\pi}{2} - (\eta - \varphi).$$

Hence from equations (16) and (17) we obtain:

Approach:

$$U_1 = -P_2a_2\left\{\frac{r_2}{r_1}\psi_1 + \left(1+\frac{r_2}{r_1}\right)\frac{\cos(\eta+\varphi)}{\cos\eta\sin\varphi}\log_e\left(1-\frac{\cos\eta\sin\varphi}{\cos(\eta+\varphi)}\psi_1\right)\right\}. \quad (20)$$

Recess:

$$U_2 = -P_2a_2\left\{\frac{r_2}{r_1}\psi_2 + \left(1+\frac{r_2}{r_1}\right)\frac{\cos(\eta-\varphi)}{\cos\eta\sin\varphi}\log_e\left(1-\frac{\cos\eta\sin\varphi}{\cos(\eta-\varphi)}\psi_2\right)\right\}. \quad (21)$$

When $r_1 = \infty$, or the driver becomes a rack, we merely substitute this value, certain terms drop out, and there is no indetermination.

When $r_2 = \infty$, or the follower becomes a rack, equations (20) and (21) would become indeterminate. To remove the indetermination put $a_2 = r_2$, and for ψ write $\frac{A}{r_2}$, where $A = r_2\psi$, and introduce A as the independent variable, and put $r_2 = \infty$ before making the integrations.

We thus obtain

Approach:

$$U_1 = P_2A_1\left\{1 + \frac{A_1}{2r_1}\frac{\cos\eta\sin\varphi}{\cos(\eta+\varphi)}\right\}. \quad (22)$$

Recess:

$$U_2 = P_2A_2\left\{1 + \frac{A_2}{2r_1}\frac{\cos\eta\sin\varphi}{\cos(\eta-\varphi)}\right\}. \quad (23)$$

EXAMPLES.

In the following four examples we will assume a pair of 30-tooth gears, the diameter of whose pitch circles is 15 inches, the addenda being so taken as to give one tooth contact. This will give us in all four cases:

$r_1 = r_2 = 7.''5$; $\psi_1 = \psi_2 = \frac{\pi}{30}$. Assume in all cases $\tan\varphi = 0.1$.

I. Given $r = 1.''875$, the teeth being epicycloidal. Find the efficiency.

Result, efficiency = 0.9895.

II. Given $r = 3.''75$, the teeth being epicycloidal. Find the efficiency.

Result, efficiency = 0.9886.

III. Given $\eta = 14°$, the teeth being involutes. Find the efficiency.

Result, efficiency = 0.9901.

IV. Given $\eta = 18°$, the teeth being involutes. Find the efficiency.

Result, efficiency = 0.9894.

TWO OR MORE PAIRS OF TEETH IN CONTACT.

Let P_1, P_2, a_1, a_2, have the same meanings as before.

Let R denote the resultant pressure between the forward pair of teeth; R_1, that between the second, etc.

Let m_1', m_1'', etc., be the perpendiculars from B on R, R_1, etc., respectively.

Let m_2', m_2'', etc., be the perpendiculars from C on R, R_1, etc., respectively.

Then we must have the equations

$$P_1 a_1 = Rm_1' + R_1 m_1'' + \text{etc.}, \qquad (24)$$
$$P_2 a_2 = Rm_2' + R_1 m_2'' + \text{etc.} \qquad (25)$$

And, moreover, the pressures R, R_1, etc., make angles φ with the normals at the points of contact as shown in the figure. In order to work out the efficiency of a pair of gears where more than one pair of teeth are in contact at the same time, it is necessary to make some assumption in regard to the way in which the work is divided between the different pairs of teeth.

ASSUMPTION OF EQUAL DIVISION OF WORK.

Let

$$\alpha = \text{pitch angle},$$
$$\psi_1 = \text{angle of approach},$$
$$\psi_2 = \text{angle of recess},$$

all measured on the follower; and let $\psi_1 \leqq \alpha$, $\psi \leqq \alpha$, so as to include all cases where two pairs of teeth are in contact simultaneously, either all or a part of the time.

The assumption of equal division of work gives:

$$Rm_2' = R_1 m_2'' = \frac{P_2 a_2}{2}.$$

$$\therefore P_2 a_2 = 2Rm_2' = 2R_1 m_2''.$$

Now, we shall ascertain the work required to be exerted on the driver during the action of one given pair of teeth, by separating the whole period $\psi_1 + \psi_2$ of contact of one tooth on the follower into the several parts, when two pairs are engaged, and when one

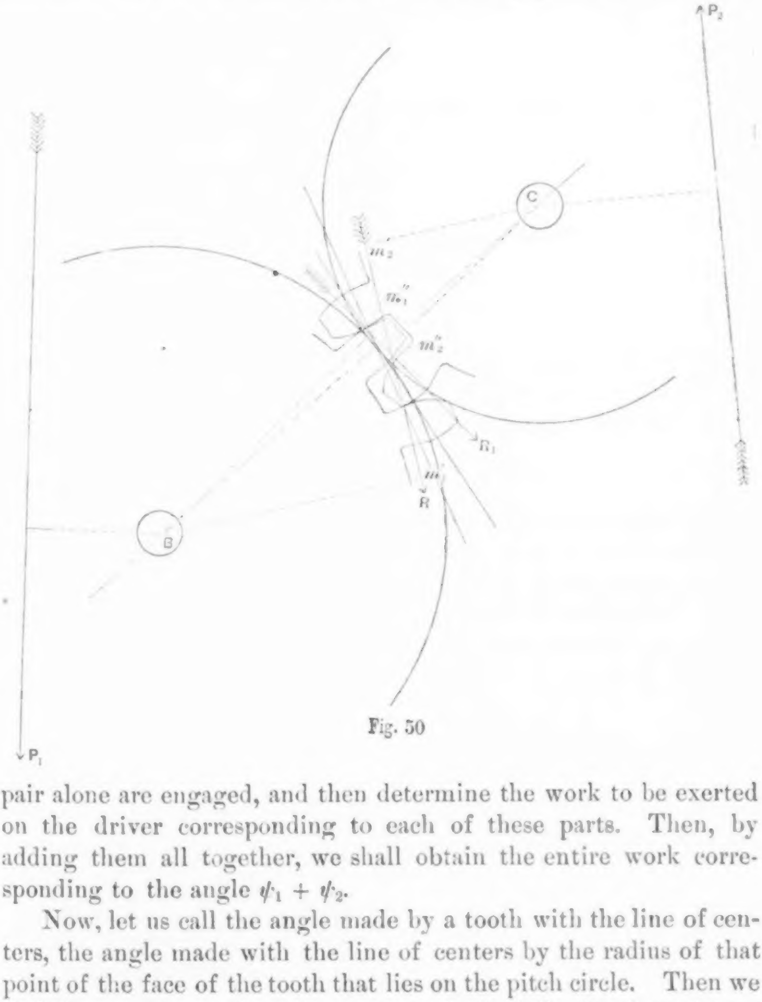


Fig. 50

pair alone are engaged, and then determine the work to be exerted on the driver corresponding to each of these parts. Then, by adding them all together, we shall obtain the entire work corresponding to the angle $\psi_1 + \psi_2$.

Now, let us call the angle made by a tooth with the line of centers, the angle made with the line of centers by the radius of that point of the face of the tooth that lies on the pitch circle. Then we shall have, in the motion of any one tooth, the following four stages; moreover, to fix the ideas, we will assume the path of approach to lie to the left, and that of recess to the right, of the pitch point:

1°. From angle ψ_1 to angle $\alpha - \psi_2$ to the left of the pitch point the tooth is in approach while the tooth ahead of it is in recess.

2°. From $\alpha - \psi_2$ to 0 the tooth is in approach and is acting alone.

3°. From 0 to $\alpha - \psi_1$ to the right of the pitch point the tooth is in recess and is acting alone.

4°. From $\alpha - \psi_1$ to ψ_2 the tooth is in recess with the tooth behind it in approach.

Moreover, we have also:

5°. The corresponding angles for the forward tooth corresponding to 1° are

$$\alpha - \psi_1 \text{ and } \psi_2.$$

6°. The corresponding angles for the rear tooth corresponding to 4° are

$$\psi_1 \text{ and } \alpha - \psi_2.$$

Now the angle from ψ_1 to $\alpha - \psi_2$ of the approach, and also the angle from $\alpha - \psi_1$ to ψ_2 of the recess is passed over by two pairs of teeth successively, each pair having the moment of resistance $\frac{P_2 a_2}{2}$, which is equivalent to having one pair of teeth pass over these angles with the moment $P_2 a_2$. The remainder of the angle $\psi_1 + \psi_2$, *i.e.*, that from $\alpha - \psi_2$ to 0 of the approach and that from 0 to $\alpha - \psi_1$ of the recess by only a single pair of teeth having the moment of resistance $P_2 a_2$. Hence the work done by the driver while the follower turns through the angle $\psi_1 + \psi_2$ is

$$U_1 = P_2 a_2 \int_0^{\psi_2} \left\{ \frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin (\theta - \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin (\theta - \varphi)}} \right\} d\psi$$

$$+ P_2 a_2 \int_0^{\psi_1} \left\{ \frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin (\theta + \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin (\theta + \varphi)}} \right\} d\psi.$$

This is the same equation (in form) that was deduced for only one pair of teeth in action at once, and it is equally true (in form) for any number of pairs in action simultaneously. Hence we may say that the following are the formulæ for the work to be exerted on the driver while the follower traverses the angle $\psi_1 + \psi_2$.

EPICYCLOIDS.

$$U_1 = \frac{P_2 a_2}{e}\left\{\frac{\left[1+\left(1-\frac{2r}{r_1}\right)\left(1+\frac{2r}{r_2}\right)\tan^2\phi\right]e\psi_1-\left(\frac{2r}{r_1}+\frac{2r}{r_2}\right)\tan\phi\log_e\left[\cos e\psi_1-\left(1+\frac{2r}{r_2}\right)\tan\phi\sin e\psi_1\right]}{1+\left(1+\frac{2r}{r_2}\right)^2\tan^2\phi}\right\}$$

$$+\frac{P_2 a_2}{e}\left\{\frac{\left[1+\left(1+\frac{2r}{r_1}\right)\left(1-\frac{2r}{r_2}\right)\tan^2\phi\right]e\psi_2-\left(\frac{2r}{r_1}+\frac{2r}{r_2}\right)\tan\phi\log_e\left[\cos e\psi_2+\left(1-\frac{2r}{r_2}\right)\tan\phi\sin e\psi_2\right]}{1+\left(1-\frac{2r}{r_2}\right)^2\tan^2\phi}\right\} \quad (26)$$

INVOLUTES.

$$U_1 = -P_2 a_2\left\{\frac{r_2}{r_1}\psi_1+\left(1+\frac{r_2}{r_1}\right)\frac{\cos(\eta+\varphi)}{\cos\eta\sin\varphi}\log_e\left(1-\frac{\cos\eta\sin\varphi}{\cos(\eta+\varphi)}\psi_1\right)\right\}$$

$$-P_2 a_2\left\{\frac{r_2}{r_1}\psi_2+\left(1+\frac{r_2}{r_1}\right)\frac{\cos(\eta-\varphi)}{\cos\eta\sin\varphi}\log_e\left(1-\frac{\cos\eta\sin\varphi}{\cos(\eta-\varphi)}\psi_2\right)\right\}, \quad (27)$$

these formulæ being the same as those given when only one pair of teeth is in action at once.

When either the driver or the follower is a rack, the same course is to be pursued as was indicated in the case of one-tooth contact. It is also to be observed that when these formulæ are used for more than one pair of teeth in action at once, they are based on the assumption that the moment of the resistance is at all times divided equally between the separate pairs of teeth.

ASSUMPTION OF EQUAL WEAR.

In this case only the resulting formulæ will be given for epicycloidal and involute gearing, the deduction being omitted.

Let U_1' denote the work done by the driver during that part of the contact of a given pair of teeth while two pairs are in action; and U_1'' that done by the driver while only one pair is in action.

Then we shall have

EPICYCLOIDAL TEETH.

Let

$$a = \frac{2r\tan\varphi\sec e\alpha}{r_1\tan e\alpha - 2r\tan\varphi}, \text{ and } b = \frac{2r\tan\varphi\sec e\alpha}{r_2\tan e\alpha + 2r\tan\varphi},$$

Then will

$$U_1' = 2P_2a_2\frac{r_2}{r_1}(\alpha - \psi_1 - \psi_2) + 2\frac{P_2a_2r_2(a+b)}{er_1a\sqrt{1-b^2}}\left\{\tan^{-1}\left[\left(\frac{1+b}{1-b}\right)^{\frac{1}{2}}\right.\right.$$

$$\left.\left.\tan\left(e\psi_1 - \frac{e\alpha}{2}\right)\right] + \tan^{-1}\left[\left(\frac{1+b}{1-b}\right)^{\frac{1}{2}}\tan\left(e\psi_2 - \frac{e\alpha}{2}\right)\right]\right\}. \quad (28)$$

In order to obtain the whole of the work to be performed upon the driver during the entire action of the tooth selected, we must add to (28) the work corresponding to the two middle periods when there is only one pair of teeth in action. This latter is to be found from the following formula:

$$U_1'' = P_2a_2\int_0^{\alpha-\psi_2}\left\{\frac{1+\dfrac{\lambda\sin\varphi}{r_1\sin(\theta-\varphi)}}{1-\dfrac{\lambda\sin\varphi}{r_2\sin(\theta-\varphi)}}\right\}d\psi$$

$$+ P_2a_2\int_0^{\alpha-\psi_1}\left\{\frac{1+\dfrac{\lambda\sin\varphi}{r_2\sin(\theta+\varphi)}}{1-\dfrac{\lambda\sin\varphi}{r_2\sin(\theta+\varphi)}}\right\}d\psi, \quad (29)$$

by making the proper substitutions. Then we have for the total work to be done upon the driver, while the follower describes the angle $\psi_1 + \psi_2$, the expression

$$U_1' + U_1''.$$

CASE: WHEN A RACK IS THE DRIVER.

When $r_1 = \infty$, (28) would become indeterminate. If, however, we observe that

$$r_1a = \frac{2r\tan\varphi\sec e\alpha}{\tan e\alpha - \dfrac{2r}{r_1}\tan\varphi},$$

and that, when $r_1 = \infty$, this becomes

$$r_1a = \frac{2r\tan\varphi}{\sin e\alpha},$$

and if we substitute this for r_1a in the denominator of (28) all the indetermination disappears.

The mode of removing the indetermination from (29) is the same as was previously explained.

CASE: WHEN A RACK IS THE FOLLOWER.

When $r_2 = \infty$, we need to make the following substitutions in the differential equation from which (28) is derived.

Let $A_1 =$ arc of approach, $A_2 =$ arc of recess, $A =$ arc whose angle measured on the follower is ψ, and let $p =$ the pitch: then

$$\psi = \frac{A}{r_2},\ \psi_1 = \frac{A_1}{r_2},\ \psi_2 = \frac{A_2}{r_2},\ d\psi = \frac{dA}{r_2},$$

$$\alpha = \frac{p}{r_2},\ a_2 = r_2,\ e\psi = \frac{A}{2r},\ e\alpha = \frac{p}{2r}.$$

Then

$$U_1' = 2P_2r\left\{\frac{2r\tan\varphi}{r_1\tan e\alpha} - 1\right\}$$

$$\int_{p-A_2}^{A_1}\left\{1 + a\cos\left(\frac{p}{2r} - \frac{A}{r}\right)\right\} d\left(\frac{p}{2r} - \frac{A}{r}\right).$$

$$\therefore U_1' = 2P_2r\left\{\frac{2r\tan\varphi}{r_1\tan e\alpha} - 1\right\}\left\{\frac{p - A_1 - A_2}{r} +\right.$$

$$\left. a\left[\sin\left(\frac{p}{2r} - \frac{A_1}{r}\right) - \sin\left(\frac{A_2}{r} - \frac{p}{2r}\right)\right]\right\}. \qquad (30)$$

The changes to be made in equation (29) have already been explained.

INVOLUTE TEETH.

Let

$$a = \frac{\cos(\eta - \varphi)}{\cos(\eta + \varphi)}, \qquad b = 2\frac{r_2}{r_1}\frac{\cos\eta\sin\varphi}{\cos(\eta + \varphi)},$$

$$c = \frac{2\cos\eta\sin\varphi}{\cos(\eta + \varphi)}, \qquad \text{and } e = \frac{a - 1 - c\alpha}{2c}.$$

Then

$$U_1' = 2P_2a_2\left(\frac{r_2}{r_1}\right)(\alpha - \psi_1 - \psi_2) + P_2a_2\frac{(a-1)\left(\frac{r_2}{r_1} + 1\right)}{c}$$

$$\log_e\left\{\frac{(\psi_1 + e)^2 + \left(\frac{\alpha}{c} - e^2\right)}{(\alpha - \psi_2 + e)^2 + \left(\frac{\alpha}{c} - e^2\right)}\right\} + 2P_2a_2\frac{\left(\frac{r_2}{r_1} + 1\right)\{\alpha - e(a-1)\}}{c\left(\frac{\alpha}{c} - e^2\right)^{\frac{1}{2}}}$$

$$\left\{\tan^{-1}\left(\frac{\psi_1 + e}{\left(\frac{\alpha}{c} - e^2\right)^{\frac{1}{2}}}\right) - \tan^{-1}\left(\frac{\alpha - \psi_2 + e}{\left(\frac{\alpha}{c} - e^2\right)^{\frac{1}{2}}}\right)\right\}. \qquad (31)$$

The value of U_1'' can be deduced from equation (29) by making the suitable substitutions and integrations, or it can be derived directly from (27) by substituting $\alpha - \psi_2$ for ψ_1, and $\alpha - \psi_1$ for ψ_2.

Then will the total work be to exerted on the driver, while the follower describes the angle $\psi_1 + \psi_2$, be

$$U_1' + U_1''.$$

When a rack drives, $r_1 = \infty$, $\therefore b = 0$, but no indetermination arises.

CASE: WHEN A RACK IS THE FOLLOWER.*

To remove the indetermination, make the following substitutions in the differential equation from which (31) is derived: $a_2 = r_2$, $\psi = \frac{A}{r_2} d\psi = \frac{dA}{r_2}$, $\frac{b}{r_2} = b_1$, $\alpha = \frac{p}{r_2}$. Then, when $r_2 = \infty$, we have

$$U_1' = 2P_2 \int_{p-A_2}^{A_1} \left\{ \frac{\frac{1}{A} + \frac{a}{p-A} + b_1}{\frac{1}{A} + \frac{a}{p-A}} \right\} dA$$

$$= 2P_2 \int_{p-A_2}^{A_1} \left\{ \frac{p + (a-1)A + b_1 A(p-A)}{p + (a-1)A} \right\} dA. \quad (32)$$

Hence, by simple division, we obtain

$$U_1' = 2P_2 \left\{ -\frac{b_1}{a-1} \int_{p-A_2}^{A_1} A\, dA + \left[\frac{b_1 ap}{(a-1)^2} + 1 \right] \right.$$

$$\left. \int_{p-A_2}^{A_1} dA - \frac{b_1 ap^2}{(a-1)^3} \int_{p-A_2}^{A_1} \frac{(a-1)\, dA}{p + (a-1)A} \right\}.$$

Hence, by making the integrations and inserting the limits, we have

$$U_1' = 2P_2 \left\{ \frac{b_1}{a-1} \left[(p - A_2)^2 - A_1^2 \right] + \left[\frac{b_1 ap}{(a-1)^2} + 1 \right] (A_1 + A_2 - p) - \right.$$

$$\left. \frac{b_1 ap^2}{(a-1)^3} \log. \left(\frac{p + (a-1)A_1}{ap - (a-1)A_2} \right) \right\}. \quad (32)$$

The manner of finding U_1'' has already been explained.

EXAMPLES OF INTERCHANGEABLE GEARS.

There will now be given in tabular form the results of using the formulæ that have been deduced to determine the efficiency of an interchangeable set of epicycloidal gears where the addendum is equal to pitch divided by π, the diameter of the describing circle being one half the diameter of the fifteen-tooth pinion; also the efficiency of an interchangeable set of involute gears with the same addendum as above stated, and with an obliquity of 14° 30′, these being usual systems. The first thing to be done before using the formulæ is to determine the angle of approach and that of recess, as measured on the follower, and these can be very easily determined by computation.

In the following tables the values of these angles will be given in every case except when the follower is a rack; for, in that case, we have to use the arcs instead of the angles, or else use the angles as measured on the driver. The efficiencies given in the tables have been determined on the assumption of a coefficient of friction equal to $\frac{1}{10}$, and on each of the two assumptions in regard to the distribution of pressure that have been already referred to.

In the cases marked with an asterisk, $\psi_1 + \psi_2 > 2\alpha$; and hence three pairs of teeth are in contact at once during a part of the travel. When, therefore, we use the second assumption, the formulæ already developed on the assumption of equal wear do not apply, and it would be necessary to develop formulæ for three-tooth contact in order to determine the efficiencies, if these are to be computed by the second assumption.

EFFICIENCY OF A TRAIN OF GEARS.

When we wish to determine the efficiency of a train of gears, it is not correct merely to determine the efficiency of each pair by the preceding formulæ, and to multiply these efficiencies together. An approximation to the truth might be obtained by such a proceeding, but not a correct result; for the efficiency of any pair varies with the mode of variation of the moment of the resistance, and this mode of variation will be different for each successive gear in the train.

Hence we must use the above-described process only while we are dealing with an elementary angle $d\psi$, and make our integration after we have multiplied the elementary efficiencies.

To illustrate this method let us assume that we have a train of

EPICYCLOIDS.

No. of Teeth on Follower.	ψ_1	15 Teeth on the Driver.			100 Teeth on the Driver.			Rack Driving.		
		ψ_2	Efficiency.		ψ_2	Efficiency.		ψ_2	Efficiency.	
			First Assumption.	Second Assumption.		First Assumption.	S cond Assumption.		First Assumption.	Second Assumption.
15	17° 56′ 2.″2	17° 56′ 2.″2	.9685	.9697	20° 43′ 42.″0	.9801	.9828	21° 25′ 0.″2	.9823	.9850
50	6° 2′ 3.″2	5° 22′ 48.″7	.9782	.9796	6° 13′ 6.″6	.9913	.9932	6° 25′ 30.″2	.9944	.9951
100	3° 6′ 33.″3	2° 41′ 24.″3	.9792	.9806	3° 6′ 33.″3	.9932	.9953	3° 12′ 45.″0	.9959	.9975
Rack.			.9825	.9853		.9943	.9975			

INVOLUTES.

No. of Teeth on Follower.	ψ_1	15 Teeth on the Driver.			100 Teeth on the Driver.			Rack Driving.		
		ψ_2	Efficiency.		ψ_2	Efficiency.		ψ_2	Efficiency.	
			First Assumption.	Second Assumption.		First Assumption.	Second Assumption.		First Assumption.	Second Assumption.
15	20° 3′ 2.″3	20° 3′ 2.″3	.9654	.9704	27° 53′ 34.″0	.9754	.9870	31° 30′ 56.″9	.9764	*
50	7° 39′ 44.″9	6° 0′ 54.″7	.9659	.9823	8° 22′ 4.″2	.9895	*	9° 27′ 16.″2	.9924	*
100	4° 11′ 2.″1	3° 0′ 27.″3	.9758	.9861	4° 11′ 2.″1	.9928	*	4° 29′ 38.″6	.9961	*
Rack.			.9767	*		.9961	*			

four gears, of which No. 1 is the driver and No. 4 the follower, and that the arc of action is, in every case, equal to one pitch.

Let us number them successively, beginning with the driver. Let P_1a_1 denote the driving moment exerted by gear No. 3, and P_2a_2, as before, the moment of the resistance of gear No. 4. Let R be the pressure between the teeth of Nos. 3 and 4, R_1 that between Nos. 2 and 3, and R_2 that between Nos. 1 and 2.

Let the radii of the pitch circles of the four gears in their order be r_1, r_2, r_3, and r_4. Let m_1 and m_2 have the same meanings as before with reference to Nos. 3 and 4, m_1', m_2' be the corresponding quantities for Nos. 2 and 3, and m_1'', m_2'' for Nos. 1 and 2. Then we must have the equations

$$\begin{aligned} Rm_2 &= P_2a_2, \\ R_1m_2' &= Rm_1, \\ R_2m_2'' &= R_1m_2', \\ P_1a_1 &= R_2m_1''. \end{aligned}$$

By eliminating the R's we obtain

$$P_1a_1 = \frac{m_1''}{m_2''}\frac{m_1'}{m_2'}\frac{m_1}{m_2}P_2a_2.$$

If, now, $d\psi$ denote the elementary angle turned through by the follower, then will that turned through by the driver be

$$\frac{r_4}{r_1}d\psi.$$

Hence we have $\qquad dU_1 = P_1a_1\frac{r_4}{r_1}d\psi = \frac{r_4}{r_1}\frac{m_1''}{m_2''}\frac{m_1'}{m_2'}\frac{m_1}{m_2}P_2a_2d\psi,$

and hence $\qquad U_1 = \frac{r_4}{r_1}\int\frac{m_1''}{m_2''}\frac{m_1'}{m_2'}\frac{m_1}{m_2}P_2a_2d\psi,$

and by this means we can find the efficiency of the train.

The integration may become very complex, and the quantity to be integrated will be different for every different location of the axles. Thus, when the approach of a pair of teeth of Nos. 3 and 4 is just beginning, the pair in action between Nos. 2 and 3 may be just beginning their approach, just finishing their recess, or anywhere else, according to the relative positions of the axles. Hence it would hardly seem to be worth while to work out any special cases here.

When the arc of contact is greater than one pitch the complexity is increased, and the uncertainty is no less. Hence, when we desire to know the efficiency of a train of gears it would seem to be best to obtain it by multiplying together the efficiencies of the different pairs, but to bear in mind that we thus obtain only an approximate solution, and not to draw any conclusions from the result that cannot fairly be drawn in view of the fact that such lack of exactness exists, and has a tolerably wide range.

DISCUSSION.

Prof. J. Burkitt Webb.—There is no doubt that the difference in friction between involute and cycloidal teeth is small, even insignificant in many cases, and so far I agree with Prof. Lanza and others; the main part of this paper, however, seems to me open to criticism.

Prof. Lanza commences by characterizing the analyses of Moseley, Rankine, Herrmann and Reuleaux as "only approximations," and proposes to make an exact treatment of the subject. To one personally unacquainted with these masters of mechanical science such a reference might give a very unfair idea of the value of their work. The statement, also, that "the case where only one pair of teeth is in action at once" "is the only one that is dealt with by Reuleaux and the other writers referred to," does not agree with the fact that Moseley treats of more than one pair in action.

Examination shows, also, that the approximations complained of are avoided in form only, inasmuch as they scarcely affect the last place of decimals used in this paper, and have no effect upon any of the conclusions drawn. It appears, too, that the attempt to calculate friction on the supposition of equal wear, which is the only part of the mathematical treatment which can claim to be new, is based on a misconception of the circumstances which determine the wear. The mathematical portion of the paper seems, therefore to fail of any useful result.

The analysis in this paper is, up to a certain point, a free quotation from Moseley, the often-important friction of journals and weight of wheels being, however, omitted; at this point a slight change is made in the treatment and claimed to be an improvement, which is doubtful. An examination of the treatment of the subject by this eminent mathematician forty years since will con-

vince any one of its practical and complete nature, and the following comparison therewith of the more complicated forms proposed in this paper fails to show any advantage over Moseley's simpler forms.

If in Moseley's complete value for P_1* we omit weight and journal friction we have

$$P_1 = \frac{a_2 r_1}{a_1 r_2} \left\{ \frac{1 + \dfrac{\lambda \sin \varphi}{r_1 \sin (\theta + \varphi)}}{1 - \dfrac{\lambda \sin \varphi}{r_2 \sin (\theta + \varphi)}} \right\} P_2 ;$$

or

$$P_1 a_1 = P_2 a_2 \frac{r_1 \sin (\theta + \varphi) + \lambda \sin \varphi}{r_2 \sin (\theta + \varphi) - \lambda \sin \varphi},$$

which being substituted in the expression for the differential work

$$dU_1 = P_1 a_1 \frac{r_2}{r_1} d\psi,$$

found in article 219, page 268, that expression becomes, after indicating the integration

$$U_1 = P_2 a_2 \frac{r_2}{r_1} \int_0^{\psi} \frac{r_1 \sin (\theta + \varphi) + \lambda \sin \varphi}{r_2 \sin (\theta + \varphi) - \lambda \sin \varphi} d\psi,$$

an expression essentially identical with formula (250), page 269, the only difference being that Moseley has performed the division of the numerator of the fraction by the denominator so as to get a decreasing series, of which he omits the unimportant terms. Prof. Lanza holds that it is more accurate to integrate the expression as it stands.

To give a clearer idea of the difference the analysis which follows is confined to involute gearing, where

$$\lambda = r_2 \cos \eta\psi ;$$

a similar discussion, with the proper value of λ for epicycloidal gears, would show the superiority of Moseley's treatment in that case also, but will not be given on account of space.

* See page 267, "Engineering and Architecture," by Canon Moseley. John Wiley & Son, N. Y., 1866.

Substituting this value of λ in the last equation, remembering that $\sin(\theta + \varphi) = \cos(\eta - \varphi)$, and putting, for brevity, $\cos(\eta - \varphi) = c$ and $\cos\eta \sin\varphi = b$, we get

$$U_1 = P_2 a_2 \frac{r_2}{r_1} \int_0^{\psi} \frac{r_1 c + r_2 b\psi}{r_2 c - r_2 b\psi} d\psi.$$

Moseley's form, obtained by division, would be (perform the division in this equation, or substitute λ in Equation (250), quoted on previous page):

$$U_1 = P_2 a \int_0^{\psi} \left\{ 1 + \left(1 + \frac{r_2}{r_1}\right)\left(\frac{b}{c}\psi + \text{etc.}\right) \right\} d\psi,$$

where the "etc." stands for $\frac{b^2}{c^2}\psi^2 + \frac{b^3}{c}\psi^3 + \text{etc.}$, the insignificant terms omitted by Mosely.

Neither of these equations presents any difficulty in integration, so that we may either use the first and get the form advocated in the paper under discussion (where, though it corresponds with U_3, we shall retain U_1 and, for convenience, drop the subscript to ψ in the equations which follow), namely,

$$U_1 = -P_2 a_2 \left\{ \frac{r_2}{r_1}\psi + \left(1 + \frac{r_2}{r_1}\right)\frac{c}{b}\log_e\left(1 - \frac{b}{c}\psi\right) \right\};$$

or we may use the second, and obtain

$$U_1 = P_2 a_2 \left\{ \psi + \left(1 + \frac{r_2}{r_1}\right)\left(\frac{b}{c}\frac{\psi^2}{2} + \frac{b^2}{c^2}\frac{\psi^3}{3} + \text{etc.}\right) \right\},$$

where we have re-introduced the largest of the terms omitted by Moseley.

In view, now, of the "etc.," both of these results are equally accurate, neither containing any approximations, and the second form may easily be shown to be identical with the first, thus: add ψ inside the last parenthesis, and subtract $\left(1 + \frac{r_2}{r_1}\right)\psi$ outside of it, and then multiply and divide this parenthesis by $-\frac{b}{c}$, which will reduce the last equation to

$$U_1 = P_2 a_2 \left\{ \psi - \left(1 + \frac{r_2}{r_1}\right)\psi - \left(1 + \frac{r_2}{r_1}\right)\frac{c}{b}\left(-\frac{b}{c}\psi - \frac{b^2}{c^2}\frac{\psi^2}{2} - \frac{b^3}{c^3}\frac{\psi^3}{3} - \text{etc.}\right)\right\},$$

where the last parenthesis is a series for a logarithm, so that, as was to be proved,

$$U_1 = -P_2 a_2 \left\{ \frac{r_2}{r_1}\psi - \left(1 + \frac{r_2}{r_1}\right)\frac{c}{b}\log_e\left(1 - \frac{b}{c}\psi\right)\right\}.$$

Now the work done by the follower during recession, is $P_2 a_2 \psi$; dividing this by Moseley's form for U_1 we get

$$\text{Efficiency} = \frac{1}{1 + \left(1 + \frac{r_2}{r_1}\right)\left(\frac{b}{c}\frac{\psi}{2} + \frac{b^2}{c^2}\frac{\psi^2}{3} + \text{etc.}\right)},$$

which corresponds with the reciprocal of Moseley's modulus, Equation (254), page 271.

Now it will be seen that the improvement claimed upon Moseley's work reduces itself to the retention of the terms judged by him to be unimportant, namely $\frac{b^2}{c^2}\frac{\psi^2}{3}$ and the more insignificant ones which follow it, and we may easily satisfy ourselves as to the correctness of Moseley's judgment by calculating this term for one of the worst cases of friction, say two five-toothed pinions gearing together, with, say, 15 per cent. friction. In this extreme case these neglected terms affect the efficiency by about one-half of 1 per cent. For the worst case calculated in the paper, 15 teeth with 15 teeth, and 10 per cent. friction, these terms would be about fourteen times less, affecting only the fourth place of decimals, and in the most of the cases calculated their effect would be inappreciable. Consequently the exact treatment and avoidance of approximations promised by the author does not exist in the paper, which differs from Moseley's treatment simply in neglecting the decimal figures beyond four places, instead of throwing away the terms of the series corresponding to those figures.

It might be contended, however, that while such terms may be dropped in numerical work, they should be retained when we are discussing exact principles and laws; but here, too, Moseley's form seems best, as it allows the equation to be put in various simple forms answering plainly various questions. Thus we may put the

reciprocal of the efficiency, *i.e.* Moseley's modulus, in the form (omitting the higher terms),

$$\frac{1}{\text{Efficiency}} = 1 + \frac{b}{c}\psi + \frac{2}{3}\frac{b^2}{c^2}\psi^2,$$

which is adapted to equal driver and follower, and shows the law by which the efficiency increases when we increase the number of teeth of such a pair; thus comparing 5 into 5 with 15 into 15 teeth, we see that for the latter case, ψ being one-third its former value, and $\frac{b}{c}$ being about one-tenth for 10 per cent., or one-seventh for 15 per cent. friction, the efficiency would be somewhat increased, by an amount easy to calculate. We may also see the effect of the last term on the whole quantity and make sure that it is neglectable in the case of 15 into 15 and larger gears.

Or, we may put the modulus in a form which will show the effect of enlarging the follower while the driver remains constant, in which case $r_2\psi_2$ will remain constant though ψ_2 diminishes, which will increase the efficiency, thus

$$\frac{1}{\text{Efficiency}} = \left(1 + \frac{b}{2c}\frac{r_2\psi}{r_1}\right) + \left(\frac{b}{2c} + \frac{b^2}{3c^2}\frac{r_2\psi}{r_1}\right)\psi_2 = A + B\psi,$$

if we put A and B for the constants in the parentheses.

Or, we may write

$$\frac{1}{\text{Efficiency}} \doteq \left(1 + \frac{b\psi}{2c} + \frac{b^2\psi^2}{3c^2}\right) + \left(\frac{r_2 b\psi}{2c} + \frac{r_2 b^2\psi^2}{3c^2}\right)\frac{1}{r_1} = A' + \frac{B'}{r_1},$$

which shows the effect of retaining the follower constant and enlarging the driver, in which case r_2 and ψ_2 are both constants, and r_1 increases, thus increasing the efficiency.

In the above special equations we have supposed the arc of recession to remain a fixed proportion of the pitch, and a similar discussion would, of course, apply to the arc of approach. The use of these equations in the present discussion is not only to show the greater simplicity and convenience of Moseley's form, but also to make clear that our 5 into 5 case was the worst case by showing that the enlargement of either or both driver and follower decreases the friction.

On page 195 of the paper a questionable method is adopted of

applying equations (20) and (21) to a rack-follower. The substitution of special cases in a general equation is usually considered to be a valuable check upon the correctness of the same; here, however, it is said that the equations will become indeterminate and that they must be put in another shape, with another independent variable, before integration to get a result for this special case, which by inference throws a doubt upon the general equations as inapplicable to the special case. If, however, the logarithm be replaced by the series this difficulty vanishes, as may be seen by noticing the last equation but one, which takes this form for a rack-follower, q_2 becoming equal to 0,

$$\frac{1}{\text{Efficiency}} = 1 + \frac{b}{2c}\,\frac{r_2 q}{r_1}.$$

It should be remarked, in concluding this part of this discussion, that there is of course no difficulty in retaining as many terms of the series as may be needed for any degree of accuracy desired.

It has already been stated that Moseley has discussed several pairs of teeth acting at once under the first assumption mentioned in this paper, and we will now consider the assumption proposed by Mr. Bilgram, as to the correctness of which there can be no doubt.

It is not enough to assume the wear proportional to the product of the normal pressure into the velocity of sliding, as would be the case for a conical pivot, but the surface over which the wear is distributed must also be taken into account. Now in involute gearing, which will serve to illustrate the error made, the point of contact d (Fig. 90) will travel over the flank of A with a velocity proportional to

$$ad \times \omega_a$$

and over the face of B with a velocity proportional to

$$bd \times \omega_b$$

and the velocity with which face slides on flank will be proportional to the difference

$$bd \times \omega_b - ad \times \omega_a$$

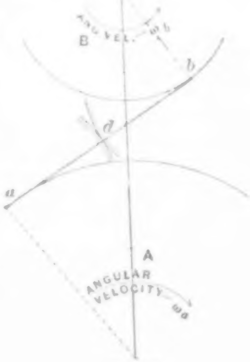


Fig. 90

The depth of wear on flank and face will not only be proportional to this difference and to the normal pressure Q_1 but also inversely proportional to the distance over which the wear is distributed so that the normal wears upon flank and face will be respectively proportional to

$$Q_1 \frac{bd \,.\, \omega_b - ad \,.\, \omega_a}{ad \,.\, \omega_a} \text{ and } Q_1 \frac{bd \,.\, \omega_b - ad \,.\, \omega_a}{bd \,.\, \omega_b},$$

and the total normal wear will be proportional to the sum of these, or to

$$Q_1 \left(\frac{bd \,.\, \omega_b}{ad \,.\, \omega_a} - \frac{ad \,.\, \omega_a}{bd \,.\, \omega_b} \right)$$

A similar expression being found for the normal wear due to a pressure Q_2 between a second pair of teeth, we can equate these values and with the help of an equation placing the sum of the moments of Q_1 and Q_2 about the axis of B equal to $P_2 a_2$ the values of Q_1 and Q_2 may be determined and introduced into the equations for determining the friction.

The analysis, therefore, of the paper being based upon a faulty assumption, must be rejected as incorrect.

It seems to me much more important to discuss carefully this normal wear than to extend the decimal places expressing efficiency, and there is an important phase of the subject touching the connection between this wear and the form of the teeth. An examination of the cycloidal form upon the above principles shows it to be a nearly permanent or persistent form, whereas involute teeth are seen to be liable to rapid change of form from wear. Such shape-changing wear must either destroy the form of an originally correct tooth, and with it the uniformity of the velocity ratio, or it might correct a badly-shaped tooth and improve the velocity ratio, which consideration leads to the proposal of a general problem as follows:

Required to find a form of tooth which will preserve its shape in spite of wear, *i.e.*, to find a *persistent form of tooth*, if such there be, the number of pairs of teeth in action at once being the same for the whole of the arcs of contact.

If there be such a form we should need to know whether it is a stable or unstable form, in which former case the problem could be stated otherwise and more generally, thus:

Required that *normal* (*persistent* or *eventual*) form toward which all teeth will wear and which is the only one affording a velocity ratio invariable with regard not only to the angular positions of the wheels but to the time the wheels have been in use.

It is claimed in the paper that Mr. Grant has drawn erroneous conclusions by reason of approximations and as he and others have taken the normal pressure as constant a reference to this point is desirable.

Moseley, it is true, assumes the moment of the driven wheel constant, but it is evident that there is a choice of three assumpsion with very little difference between them, viz.:

(*a*.) A constant moment on the driver,

(*b*.) A constant moment for the normal pressure,

(*c*.) A constant moment on the follower;

And it is perhaps more a question of convenience than of exactness which we choose, as practical cases agree sometimes with one and sometimes with the other extreme. A difference of efficiency, therefore, depend on which of these assumptions we may make, is unreliable, unless this difference should, in some way, be an important item in a special practical case to be considered.

I have some suggestions to make as to dynamometric work, such as that of the extensive "Sellers" experiments reported by Mr. Lewis.* Professor Lanza also alludes to such work as difficult. The disregard of a fundamental scientific principle must necessarily increase the difficulty of the work and perhaps make the results useless. It is well known that it is fatal to accuracy to determine a small quantity as the difference of two large ones, whose unavoidable errors encroach upon and may entirely bury the quantity sought; this was the case in the experiments referred to, and yet it seems possible to avoid the difficulty.

The following apparatus (Fig. 89) is proposed as a means of measuring with very little error the tooth friction between spur gears of nearly the same size. The principle employed is to keep the teeth in forcible contact by a spring for that purpose alone, while the friction is measured by an arrangement with which the spring has no connection, and which measures nothing but the friction.

In the cut (Fig. 89) E is a tank in which is floated a caisson F, the two constituting a floating dynamometer of simple construction upon the same principle as those described by me in the *Electri-*

* Trans. A. S. M. E., Vol. VII., pp. 273 and 549, Papers 198 and 213.

cal World, September 17 and 24, 1887. Upon the tank are two boxes J and J for the shaft I, and upon the cross pieces FF and FF of the caisson are two upright hangers having journals not only for I but also for another and lighter shaft G. To the driving pulley H fast on I is conveyed power sufficient only for the work lost in friction, and the moment exerted by the belt upon H causes the floating caisson to tip out of the horizontal position in which it naturally floats until by sliding along the poise L it is brought back to the horizontal position. The continued product, then, of this weight in pounds by the distance in feet that it has been moved and by 2π times the number of revolutions per minute gives the horse power absorbed in the friction of the gears. The gears A, B, B, C, C and D are arranged thus: C, C and D are fast upon a light shaft G so as to constitute essentially one gear. The journals of G are made as small as possible, and there should be an adjustment of the boxes allowing for exact regulation of the depth to which the teeth engage with those of A and B, B. The office of G is principally to sustain the weight of B and C, C. Of the gears A and B, B, the latter are fast upon the shaft I, while A is loose between them. MM is a piece also free to revolve about I and having two ends or arms, each of which holds a flat spring N and has a slot with tap bolt P for clamping the piece fast to B. The ends are also cast hollow so that a bar (dotted) O may be inserted and the piece MM forcibly revolved by means of a known weight hung upon the end of the bar, and thus the springs set with a known pressure when the piece MM is to be clamped firmly to B. The other ends of the springs NN enter between the spokes of B and bear against lugs on A, so that the action of the springs is to

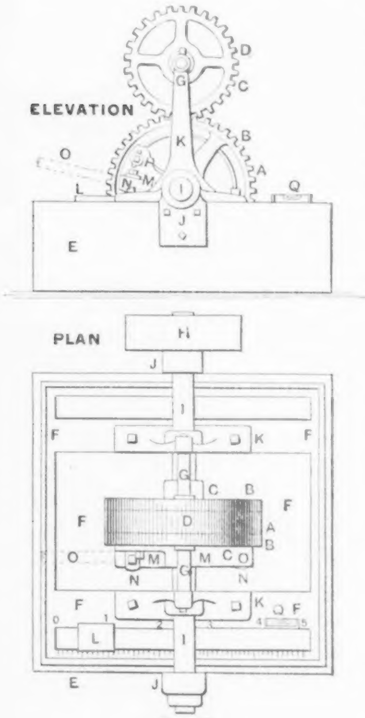


Fig. 89

revolve A forcibly and cause the teeth of the series of wheels to press against each other any desired amount, corresponding therefore with the transmission of a certain number of horse power. In fact this power will in reality be transmitted by the wheels through the closed cycle ABCDA, which transmission will produce the friction to be measured without interfering with the delicate measurement of the same. We shall get also an inconsiderable amount of journal friction from the journals of G and I in the hangers K, K; that between I and K may be made practically zero by floating up the caisson until there is no pressure to cause friction, the weight of I and attached parts being supported entirely by J, J. The friction of I in J, J does not of course come into the problem nor have anything to do with the experiment. The gears should be separated by perhaps an eighth of an inch, and that much play should be allowed the caisson so as to allow a little end play of the gears; stops beneath the shaft I confine the caisson endwise with this much freedom, which is better than having collars on I running against K, K. Q is a level to indicate the horizontal position of the caisson, and the proper sensitiveness of the latter is obtained by the proper proportions of the caisson and by weights of any form (not shown) laid on the bottom of the caisson to bring the centre of gravity of the whole floating mass to the proper point.

Mr. Hugo Bilgram.—Prof. Lanza apparently mistakes my proposition as to the division of force transmitted by two or more teeth, when in simultaneous contact. This appears most clearly from the statement: "Although the arguments advanced by Mr. Hugo Bilgram in favor of the equal wear theory possess some plausibility, it is not at all certain that the wear is equal." When speaking of equal wear I had reference to corresponding contacts only. The wear in any other set of contacts may be a different quantity.

My assertion that when a force is transmitted by two separate contacts, both being exposed to wear, the wear must be equal on both contacts (measured in the line of motion), seems to me so axiomatical that the expression of doubt on this point can only indicate a misapprehension. If the wear should differ, a simultaneous contact could not persist and an inversion of the tendency to wear would soon re-establish double contact. I need hardly add that an equal velocity on both points was premised, as otherwise the statement must be modified to suit the case.

The further assumption that wear is in proportion to pressure,

other things being equal, is not, as would appear from the paper just read, a premise to the equal wear theory, but a means of reaching further conclusions, and I have qualified it as being, presumably, only approximately true.

My position as to the wear of toothed gearing is simply this:

FIRST ASSUMPTION: Considering only one pair of teeth in contact at a time: Let A and B (Fig. 83) be a pair of teeth, $p\,p$ being the path of contact. In following the progress of contact between the points 1 and 2 it is obvious that $a_1\,a_2$ and $b_1\,b_2$ will be the portions of the teeth-curves in action, the projecting circles being concentric with the pitch circles. Denoting the arc $a_1\,a_2$ by α and $b_1\,b_2$ by β, the slip during the considered interval is equal to the difference $\alpha - \beta$ and its effect being distributed over the distances α and β, the tendency to wear will equal $\frac{\alpha-\beta}{\alpha}$ and $\frac{\alpha-\beta}{\beta}$ on the teeth A and B respectively. Applying this deduction to cycloidal and involute teeth, the tendency to wear can be represented by Figs. 84 and 85. In the cycloidal gear the tendency to wear is uniform on each side of the pitch line, while on involute gears this tendency is zero at the pitch point and increases with the distance from this point.

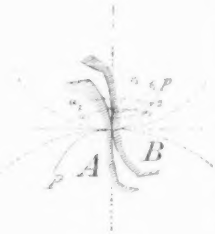


FIG. 83.


FIG. 84. FIG. 85. FIG. 86.

In the second assumption, two pairs of teeth are considered to be in constant contact, so that one contact will end and another begin when one of the contacts is at the pitch point.

The tendency to wear in cycloidal gears, being uniform along each branch of the path of contact, the force will tend to be distributed at a fixed ratio between the approaching and receding contact, and the wear as well as the loss from friction will be reduced to one-half of what it was under the first assumption. In involute gears, however, the pressure will be distributed unequally, bearing harder on the contact nearer the pitch points. The wear at the pitch point being zero, that at the points of simultaneous contact cannot exceed it, hence the wear as well as the loss from friction will be as represented in the diagram, Fig. 86, showing it to be practically one third of what it would be were only one tooth in contact at a time.

When a third assumption is made (*i.e.*, a single contact during one portion, and a double contact during another portion of the action) the two preceding assumptions can be combined to meet the case.

The deductions of the formulæ for the "assumption of equal wear" being omitted in the paper of Prof. Lanza, it is not possible to point out the divergence from my "equal wear theory" which appears to exist, judging from the tabulated results; since my theory would point to an equal division of the force in cycloidal gears, at least where both gears are of equal diameter, the results of Prof. Lanza's first and second assumptions should be identical in both cases.

Mr. Wilfred Lewis.—The paper by Prof. Lanza goes into the question of "Friction in Toothed Gearing" very minutely, and the results obtained upon the two assumptions of equally divided pressure and equally divided wear, are especially interesting and instructive. With cycloidal teeth, the conditions of equally divided pressure and equally divided wear are frequently much the same, and but little if any improvement in efficiency could be anticipated from the establishment of the persistency of wear suggested by Mr. Bilgram. In fact, I am surprised rather at the magnitude of some of the differences shown in the table than at the generally close agreement of the results from the two assumptions.

With involute teeth the case is different, and a great improvement in efficiency would naturally be expected from a glance at Mr. Bilgram's diagram.

This is brought out analytically by Prof. Lanza, but the amount of labor involved detracts somewhat from a feeling of perfect confidence in the work, and there are, indeed, so many chances for error in the use of such ponderous formulæ, that instead of accepting the results obtained as a standard for testing other methods, it would seem quite as proper and desirable to use some other method as a check upon the accuracy of the work performed in these lengthy solutions.

Great care has been taken to avoid approximations, and mathematical exactness has been the chief end in view—to determine, if possible, the correctness of conclusions based upon previous solutions which were only approximate. The assumptions made at the outset are not claimed, however, to be unalterable or inflexible, and it is even suggested that they may be modified in the light of

future experiments, but the fundamental analysis for a single pair of teeth in action is certainly based upon very reasonable ground. At the same time it should be observed that the probability of the truth of the assumptions made is no greater than that of some others which might have been made with at least equal propriety.

Prof. Lanza has adopted as the basis of his work the assumption that the moment of resistance is constant, but I see no more reason why this should be an accepted condition than that the driving moment should be considered as constant. If, for example, we have a train of gears, *A*, *B*, *C*, in which *A* drives *B*, and *B* drives *C*, it is evident that the moment of resistance for *A* must be the driving moment for *C*, and that one assumption necessitates the other. There is really no choice whatever between them, and while it must be admitted that there will be a slight theoretical difference in the results obtained from one or the other as a basis, it must nevertheless be granted that the results are in both cases equally correct, and I am clearly of the opinion that whatever difference can be made to appear will be altogether too microscopic to be seriously entertained as of any practical significance. In fact, the results obtained upon the basis of one plausible assumption, as, for example, that of Reuleaux, cannot differ sensibly from those obtained upon another, whether it be a constant driving moment or a constant moment of resistance; and I fail to see the virtue of adhering strictly to any of these assumptions as dwelt upon in the case of a train of gears. Here the assumption of a constant driving moment is manifestly inadmissible throughout the train, and an already complicated problem is still further embarrassed by a difficulty which adds nothing, when surmounted, to the value of the results. We are told that the efficiency of a train of gearing might be approximately determined by multiplying together the efficiencies of each pair of wheels, but that such a result would not be correct, and a plan is suggested whereby the truth may be known; but the difficulties which it presents bring about a return to the original method of approximating, care being taken to remember that a want of exactness, having a tolerably wide range, exists.

Now, in view of the fact that no assumption can be claimed as absolutely correct, and that it makes almost no difference in the result which assumption is taken, I cannot admit that the want of exactness referred to has even a tolerably narrow range, or that the plan suggested would, if carried out, lead to results that could

be regarded as any nearer the truth. Further than this, I believe that another assumption, equally correct in theory, might have been made which would have reduced the problem to much simpler elements, and have done away with a great deal of laborious analysis.

It is well known that an interchangeable system of gearing is determined by the shape of its rack, and that, of course, the rack will gear correctly with every wheel in the set, and inasmuch as it is quite a matter of indifference whether the driving moment or the moment of resistance be considered as constant, I propose to approach, if possible, still nearer to the truth, by assuming both to vary so as to split the difference and make the force transmitted from one wheel to the other, through the medium of an imaginary rack, a constant.

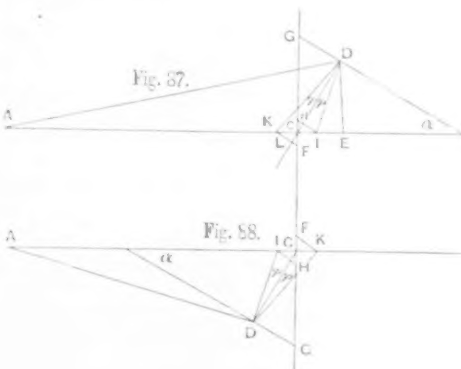


Fig. 87.

Fig. 88.

By this means the problem is resolved into its elementary components, and all that we have to consider is the friction of a wheel driving a rack against uniform resistance and that of a rack driving a wheel by a uniform pressure.

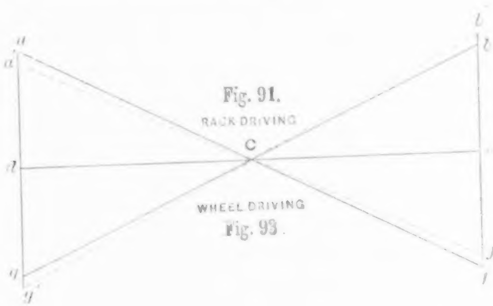
Referring to Fig. 87, A is the centre of a wheel gearing with the rack GF, C is the pitch point, and D a point in the arc of action.

Drawing DK and DI at the angle of friction φ to DC, and KF, IH, and GD perpendicular to DC, we have, when the rack is driving, GF equals the product of sliding and pressure at the point D in terms of AC, the rack travel under a unit of pressure in the direction of motion ; or, when the wheel is driving, GH becomes equal to the product of sliding and pressure in terms of AC.

To prove this we have CD for the sliding at the point D in terms of the rack travel AC as demonstrated in my discussion of

Prof. Reuleaux's paper.* The pressure between the teeth in the direction of KD must be $\frac{KD}{DE}$, the normal component of which is $\frac{DL}{DE}$. Therefore the product of sliding and pressure is $\frac{DL}{DE} \times CD = DL \sec \alpha = GF$. Similarly, when the driving pressure acts in the direction DI, GH may be shown to be the product of sliding and pressure, and the construction is demonstrated.

Fig. 88 is a construction for the loss in friction on the other side of the line of centres. Plotting the results upon a base line dCe representing rack travel, C being the pitch point, we obtain Figs. 91 and 93, the area of which is proportional to the loss in friction.



It is evident that for involute teeth, to which I will confine myself for simplicity, these figures will be bounded by straight lines, and we are enabled by their use to make a direct and obvious comparison between the exact method here given and the approximate method previously considered. In that case the angle of friction was neglected, and gC was shown to be the product of sliding and pressure. The loss in friction by the approximate method is therefore bounded by the dotted lines $a'Cb'$ and $g'Cf'$ from which it appears that, during approach the exact method gives a loss slightly greater, and during recess one slightly less than the approximate. When the approach and recess are equal, the increment is slightly greater than the decrement, the relation being as CF to CH, and in general we may say that the error of the approximate method is represented by $\frac{CF - CH}{CG}$. The greater the obliquity, and the greater the angle of friction, the greater will this difference appear; but taking an obliquity of 20°, which is larger

* Trans. A. S. M. E., Vol. VIII., p. 69, Figs. 68 and 69.

than usual, and a coefficient of .1 for friction, I find by a careful calculation that the loss in friction by the exact method would be only 1.003 times the loss as estimated by the approximate method. The greatest amount of loss given by Prof. Lanza does not exceed .04, and consequently the error for an extreme case would be about $.003 \times .04 = .00012$. How such a quantity as this could affect conclusions hitherto drawn it is difficult to imagine, but it is interesting to observe the close agreement in the results of the two methods, and the consequent futility of drawing such tight lines in practice. Of course, when the approach and recess are unequal, the error may be greater or less, but its practical significance is always very small.

The loss in friction for a wheel driving a rack, or a rack driving a wheel, can be determined graphically as shown, or analytically by means of formulæ derived directly from the graphical solution. When the approach and recess are equal, this loss is the same whether the wheel drives the rack or the rack drives the wheel, but the efficiencies in the two cases are different. Letting L = loss in friction, $\frac{1}{1 + L}$ = efficiency of wheel driving rack, and $1 - L$ = efficiency of rack driving wheel.

The efficiency of the two wheels A driving B, is the efficiency of A driving a rack times the efficiency of a rack driving B, the approach and recess for the rack and wheels being determined by that of the two wheels gearing together. Whether an exact or an approximate solution be desired, I should, in all cases, prefer the graphical method for simplicity and directness of application, and it should also be observed that the same method can be used with facility as shown in my previous discussion, where two teeth are in action at once.

In this way the efficiency of any combination of gears can be determined by a comparatively simple analysis. There are no moments whatever to be considered, no indeterminations or the appearance of such, and the efficiency of a train of gearing can be found with theoretical exactness by multiplying together the efficiencies of the several pairs of wheels.

The criticisms which I have made are not intended to reflect upon the accuracy of Prof. Lanza's analysis, although I suspect some numerical errors, but merely to suggest that equally correct results might be obtained from another point of view at a considerably less expenditure of mathematics and nervous energy.

Prof. Jas. E. Denton.—As there are now ample means available for computing what *ought to be* the friction of any given pair of spur wheels, I would suggest that a pertinent addition to the theory presented would be a description of some cases of gearing which in practical use have *shown* exceptional friction or wear.

Since the date of the meeting, my attention has been called to the following case of excessive wear (Fig. 78) by a member of the

FIG. 78.

Society. A cast-iron spur pinion, Fig. 78, 4 inches pitch diameter, 2 inches face, having 13 teeth, is driven by a cast-iron wheel of 59 teeth making 36 revolutions per minute as an average. The two wheels are the primary pieces of an agricultural machine operated by two horses in the open field. The mean effort or force transmitted at the pitch circles is about 200 pounds, or 100 pounds per inch of width. The 4-inch wheel wears to the amount shown in the accompanying cut in about a year of service.

Attempts have been made to secure less rapid wear by changing the form of tooth from epicycloidal to involute, and by changing from cast teeth to those cut with cutters made by standard establishments. No sensible difference in rate of wear resulted from these changes.

The pinion shown in the cut represents cast teeth. Bevel gears, both involute and epicycloidal, either hand or machine made, show a similar rapid rate of wear at other parts of the mechanism of the machine.

The driving gear of 59 teeth wears as rapidly as the pinion, proportionally to the less amount of rubbing upon each tooth due to the greater number of teeth.

Copies of the wheels run at the factory for long intervals do not show excessively rapid wear. Also less rapid wear occurs in machines at work in some parts of the country than in others.

It is consistent with all the circumstances to conclude that the wear is caused mainly by the accumulation of grit, the latter varying with the soil worked over by the machine.

Mr. Jno. T. Hawkins.—Referring to Prof. Denton's remarks, will say that I have recently encountered a case of abnormal wear in a pinion-gear, which, up to this time, has not been satisfactorily accounted for. It is the more singular, for the reason that we have a large number of machines in operation containing duplicates of the gear shown and operating under precisely similar conditions; and, while we have observed perhaps a little more wear than appeared necessarily to follow the conditions of working in one or two other cases, the remainder have, so far as we know, shown no more wear than should be looked for, under the conditions; that is to say, they are in such condition as, after running a year, might reasonably promise that they would run ten more; and nothing to approach the one in question appears in the worst other case.

The conditions, as given on the sketch, Fig. 77, are that the 130-toothed wheel is a crank gear, the crank pin being located as shown by the circle near the periphery, and at a radius of $18\frac{11}{64}''$. The shaft carrying the 13-toothed pinion is the driving shaft. The connecting rod from the crank pin is made to reciprocate a body, running on rollers, weighing about 2,400 pounds; and, by means of a rolling pinion and racks, this body makes a stroke equal to four times the radius of the crank, while the body carrying the rolling pinion, and weighing 810 pounds, moves through only twice the radius of the crank. The 2,400-pound body has a rack gearing with a wheel upon a cylinder, the latter rotating in fixed bearings, said cylinder weighing 868 pounds. The work of performing these operations at from 1,500 to 1,700 revolutions of the crank per minute is what is required of the pinion. The pinion is of bronze, of the composition, copper 64, tin 8, lead 1, for purposes of strength. The large wheel is of cast iron; and both wheel and pinion are accurately cut with Brown & Sharpe standard epicycloidal cutters. It will be observed that the pinion

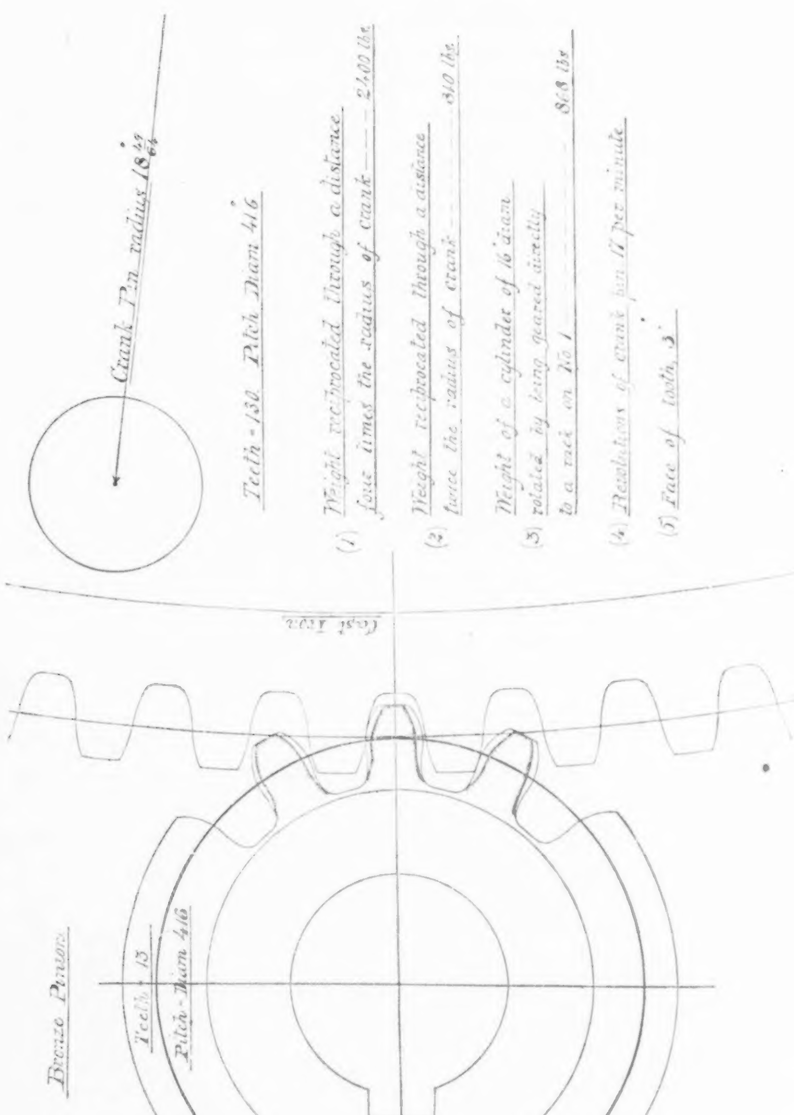

Crank Pin radius 18 49/64"
Teeth - 130 Pitch Diam 416"
(1) Weight reciprocated through a distance four times the radius of crank ---- 2400 lbs.
(2) Weight reciprocated through a distance twice the radius of crank ---- 840 lbs.
(3) Weight of a cylinder of 16" diam rotated by being geared directly to a rack on No 1 ---- 868 lbs
(4) Revolutions of crank pin 17 per minute
(5) Face of tooth 3"
Cast Iron.
Bronze Pinion.
Teeth - 15
Pitch - Diam 416

FIG. 77.

has worn about equally on both sides of the teeth, the inner shaded line showing its worn condition. This is probably accounted for, however, by the nature of the movements, the pinion acting as a driver from one dead centre until the reciprocating bodies arrive at maximum velocity, or when crank is at the "live centre;" and thereafter until the dead centre is reached the reciprocating parts become drivers and bring the pressure on the opposite sides of the teeth, the pinion shaft being provided with a heavy fly wheel. From the proportions, it is clear that there are always two full teeth of the pinion in gear; and, while it is doubtless true that the pressure exerted at times on these teeth approaches the greatest which they should be called upon to sustain, it does not, so far, appear clear that it should not be competent to perform the work actually brought upon it—particularly in view of the considerable number of exactly similar ones running under identical conditions without such excessive wear as is shown. I have every reason to believe that not only have these gears received good attention and been well lubricated, but that, if anything, owing to the observed wear while in progress, they have had extra good care in this particular. It is also not found probable or possible that at any time any gritty matter has been allowed to get upon them.

Prof. Lanza.—The greater part of Prof. Webb's remarks have nothing whatever to do with the real points in question. His chief criticism seems to be, that Moseley did something which, if not quite the same thing in Prof. Webb's estimation, is just as good as what I have done, and that the use of the exact formulæ makes so little difference in the resulting efficiency as not to be worth the extra labor. It would appear, therefore, that he must think that the object of the paper is to furnish working formulæ for a more accurate determination of the efficiency than can be obtained by means of the approximate ones of Moseley, Reuleaux, Rankine, and others.

The fact is, however, that, in my opinion, any one of these approximate solutions will give the efficiency with a far greater mathematical exactness than the degree of accuracy with which we know the experimental constants that enter into the formulæ; and that really correct results are only to be obtained by experiment. My objects in writing the paper are stated at the bottom of page 187 and on page 188.

In order to establish some of these conclusions, notably 1 and 2,

of page 187, it was necessary to use exact formulæ, and, as I know of none such on record, I had to work them out myself. I followed Moseley through his fundamental equations, and, when he dropped into approximations, I abandoned him. It also seemed desirable to use his notation. The special approximation referred to in conclusion 2 is neglecting the moment of the friction, and this has been neglected in every treatment of the subject that I have seen in print except in those of Moseley and Kennedy.

Prof. Webb asserts that Moseley treats of more than one pair of teeth in action at once; I can, however, find no reference to the matter in his book on *Engineering and Architecture*, edition of 1875, which is the one that I have.

As to the assumption of equal wear I will speak in my reply to Mr. Bilgram.

Another reason for presenting the paper was that it seemed desirable that the exact formulæ should be placed on record.

As to Prof. Webb's dynamometer I have no opinion to express, as I have had enough to do with testing dynamometers to know that the only way to determine whether it is better or worse than others now in the market is to submit it to test.

In reply to Mr. Lewis's remarks about the danger of numerical errors arising in the use of such long formulæ, all that can be said is that such precautions have been taken as were deemed necessary to avoid them, and, therefore, the numerical results are believed to be free from them, but of this I cannot, of course, be absolutely certain, as all of us are liable to errors of that kind.

As to the different assumptions that might be made in regard to the distribution of the pressure, it seems hardly necessary for me to say any more than has been already said in the paper.

Mr. Lewis's graphical solution by the use of an imaginary rack is certainly a neat one. It would be interesting to see some tables of results worked out by its means, and also to see the method extended to the cases of our ordinary systems of interchangeable gears where more than one pair of teeth are in action at once. Until this is done, no very decided opinion can be expressed as to its real value as a working method. It assumes, of course, a distribution of the pressure that rarely, if ever, occurs in practice, but it may, after all, represent such a fair average of all these conditions as to be a good working method.

In regard to the relative merits of a graphical and of an analytical solution in general, this is a matter largely of taste, and of a

person's natural bent, some persons naturally inclining to graphical, and others to analytical methods. Which is the best in any special case must depend upon circumstances, as, in some cases the one and in other cases the other gives the neater solution.

As to Mr. Bilgram's remarks I will say that I have carried out the calculations on the assumption that the wear is proportional to the product of the normal pressure by the velocity of sliding, and that I supposed that in so doing I was following Mr. Bilgram's suggestion as I understood it from the statement in Vol. VIII. of the Proceedings, page 76, lines 9 and 10, and moreover this seemed to be a suggestion in general accord with the modern theory of pivot and axle friction, which finds a good deal of favor at the present time. It appears, from his remarks in this discussion, however, that he only intended it as a provisional condition, and not the one that he thinks ought to be used in the equal-wear theory.

It would be interesting to see the fundamental condition as he would express it, put into analytical form. The work of deducing the formulæ for the efficiency would become doubtless more complicated than the work that I have done, and how the results would compare cannot be predicted absolutely, but it would seem probable that the differences between the results and those of my first assumption would be rather smaller than those between the results of my two assumptions.

Finally, it may be said that the main conclusions of the paper are not, it seems to me, in any way affected by anything that has been said in the discussion.

CCLXXIV.

CENTRIFUGAL PUMPS AND THEIR EFFICIENCIES.

BY WM. O. WEBBER, BOSTON, MASS.

(Member of the Society.)

LOOKING back at first to the records and work accomplished by some of the earlier pumps of this type, we find that perhaps the first centrifugal pump, in a primitive state, was brought out by the mathematician Euler (an account of which was published in the proceedings of the Academy of Berlin for 1754), and which was probably a failure in a practical sense.

The next pump to attract our attention, and one which is credited more nearly to have approached the efficiencies of the present day, was one erected by a Mr. McCarty in the New York Navy Yard in 1830.

Then followed, in 1851, at the great London Exhibition, the celebrated Appold pump, which is really the first pump of which an authentic set of experiments are available. Mr. Appold determined that the efficiency mainly depended upon the form of blades in the fan and the shape of the volute or enveloping case, and that the best form for the blades was a curve pointing in opposite direction to that in which the fan revolved, and for the case that of a spiral tapering pipe or volute.

To quote somewhat at length from a paper* entitled "The History and Theoretical Laws of Centrifugal Pumps, as Supported by Experiment and their Application to their Design," by the Hon. R. Clere Parsons, B.A., B.C.E., Dublin, Stud. Inst., C.E., in January, 1875, Mr. Anderson, M. Inst. C. E., of the firm of Messrs. Eastons & Anderson, deputed the author, in company with Mr. Hesketh, Stud. Inst. C. E., to make some experiments upon centrifugal pumps, both with a view to determine the laws which regulate their discharge, and at the same time, if possible, to improve their efficiency.

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* Excerpt minutes of proceedings of the Institution of Civil Engineers. Vol 47, Session 1875-76, Part 1.

The first experiments were made upon a pump whose suction and discharge pipes were 10 inches in diameter; the fan was 14 inches in diameter, and the casing surrounding the fan was circular, as is shown in the dotted lines of Fig. 51.

The pump was driven by a single acting engine working directly onto the shaft of the pump. The water was raised by the pump from a large cast-iron tank and discharged back into it through a measuring tank. In the bottom of the measuring tank

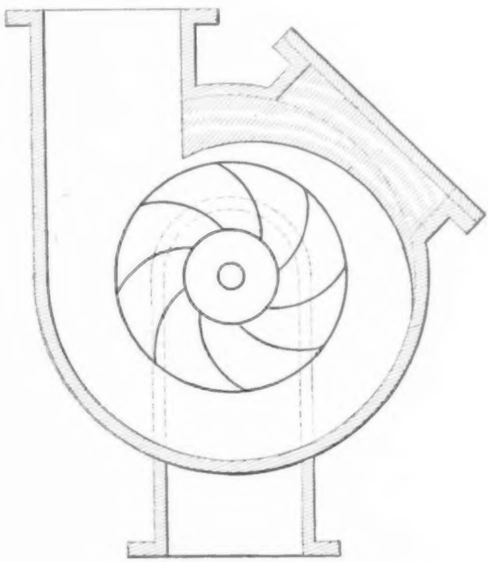

FIG. 51.

was a hole in a thin sheet-iron plate, and by the head of water maintained over this hole the discharge was calculated by the formula,

$$Q = 0.62 \times A \times \sqrt{2\,gh} \text{ cubic feet per second.}$$

Where A = area of hole in square feet,
h = head of water in feet over hole.

The revolutions per minute of the fan were measured by a counter attached to the shaft, and the lift was determined by means of a staff fastened to a float in the lower tank. The staff

was graduated in feet and decimals of a foot, and was applied to a water-gauge glass connected with the discharge pipe.

In order to take account of the residual velocity of the water in the discharge pipe in estimating the lift, the tube with which the water-gauge was connected had a bend whose extremity met the water flowing in the discharge pipe, consequently the water in the gauge glass stood at the same height as the water issuing from the discharge pipe.

The engine was supplied by steam from a portable boiler, into which the feed water was injected by a hand pump.

During an experiment the speed of the engine was regulated by an observer so as to maintain a constant head of water in the measuring tank, and consequently insure a uniform discharge from the pump.

An experiment was continued until 30 gallons of water were evaporated in the boiler, and the time required to accomplish this was accurately noted.

Thus the number of pounds of water raised per minute divided by the number of pounds of water evaporated in the boiler per minute is proportional to the efficiency of the pump.

These experiments were repeated for different discharges, and tabulated in the following form:

TABLE I.

Gallons Discharged per Minute.	Lift in Feet.	Ft. Lbs. Raised per Minute.	Ft. Lbs. Raised per Lb. of Water Evaporated.	Revolutions per Minute.	REMARKS.
	5.667			305	
745	6.000	44,886	7,779	363	
879	6.250	54,937	8,224	380	Appold's fan in circular case.
989	6.666	65,936	11,883	393	
,153	7.000	80,710	11,385	416	

The casing around the fan was next altered from the circular to the spiral form, as is indicated by the dotted lines in Fig. 51, by fitting wooden blocks inside; and a fresh series of experiments was now made under circumstances resembling the former set as much as possible.

The following are the results tabulated in a similar form:

TABLE II.

Gallons discharged per Minute.	Lift in Feet.	Ft. Lbs. Raised per Minute.	Ft. Lbs. Raised per Lb. of Water Evaporated.	Revolutions per Minute.	REMARKS.
	5.750			320	Appold's fan in *spiral* casing.
577	6.500	37,505	8,930	346	
746	6.925	51,600	10,809	363	
878	6.750	59,265	11.264	368	
999	7.085	70,029	12,088	387	
1,150	7.750	89,125	13,248	403	
1,288	8.333	107,329	15,996	423	

Thus, by comparing the first and second tables, it will be noticed how greatly the spiral casing has improved the efficiency, and also increased the discharge, the boiler pressure remaining the same.

With the same casing, but another fan, designed on the principles laid down by Rankine in his "Applied Mechanics," and also advocated by Glynn in his treatise on "Water Power," some experiments were made similar to the preceding ones.

The following tables show the results of these experiments:

TABLE III.

Gallons Disc arged per Minute.	Lift in Feet.	Ft. Lbs. Raised per Minute.	Ft. Lbs. Raised per Lb. of Water Evaporated.	Revolutions per Minute.	REMARKS.
	5.500			390	Rankine's fan ir circular case.
578	5.925	34,200	7,203	316	
741	6.167	45,695	8,556	335	
880	6.333	55,733	8,377	348	
993	6.583	65,372	10,748	355	

TABLE IV.

Gallons Discharged per Minute.	Lift in Feet.	Ft. Lbs. Raised per Minute.	Ft. Lbs. Raised per Lb. of Water Evaporated.	Revolutions per Minute.	REMARKS.
	5.416			300	
580	6.333	36,731	9,675	324	Rankine's fan in *spiral* case.
743	6.667	49,528	10,857	334	
879	7.000	61,530	11,692	343	
996	7.333	73,036	12,954	353	

These results prove that Rankine's fan is far inferior to that of Appold. The blades of this fan were for half their length, from the center outward, similar to those of Appold; but for the remaining half of their length, they curved forward in the direction in which the fan revolved, ending in radial tips (Fig. 52).

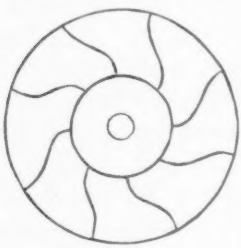

FIG. 52.

This method of estimating the efficiency of a pump is by no means an accurate one; but in altering a pump, it is a convenient way of determining whether the alteration has proved a success or the reverse.

A new casing of a spiral form was now designed, and a fresh series of experiments undertaken. These experiments were of a much more elaborate nature than those just described; and as it may make the deductions from them more easily understood, their general arrangement will be described.

In order, as far as possible, to insure a constant lift, the pump was supported on the side of a large "barge," and the engine for the purpose of driving it was placed in the bottom. The power transmitted to the pump was estimated by the dynamometer used by Messrs. Eastons and Anderson at the Royal Agricultural Shows.

The method of measuring the amount of water raised by the pump described in the former experiments was repeated in this instance; the water being raised from the Thames, in which the barge was floating, and discharged back into it through the measuring tank. The lift was measured by taking the difference of the levels of the water in the river and that in the discharge pipe, as shown by two gauge glasses.

The revolutions of the pump were indicated by a counter attached to the pump shaft. A number of experiments were made upon this pump, and the results tabulated as follows:

TABLE V.

No. of Experiments.	Gallons of Water Discharged per Minute.	Lift in Feet.	Foot Lbs. Raised per Minute.	Foot Lbs. Indicated per Minute.	Revolutions per Minute.	Efficiency, per Cent.
1	1,012	14.67	148,461	298,438	392	49.74
2	1,108	14.70	162,875	317,158	394	51.35
3	1,107	14.65	175,364	332,136	395	52.80
4	1,280	14.70	188,160	343,754	398	54.74
5	1,350	14.75	199,128	357,194	399	55.75
6	1,431	14.75	211,073	374,954	400	56.20
7	1,501	14.70	220,650	388,897	402	56.69
8	1,568	14.75	231,280	404,737	403	57.01
9	1,630	14.80	241,240	409,612	404	58.90
10	1,695	14.75	251,987	419,790	405	60.17
11	1,753	14.80	259,450	435,630	406	59.42
12	1,012	17.40	176,088	370,548	424	47.53
13	1,108	17.20	190,576	388,316	425	48.97
14	1,197	17.20	205,884	404,156	427	51.09
15	1,280	17.30	221,440	417,214	428	53.08
16	1,350	17.30	233,550	433,054	429	53.93
17	1,431	17.40	248,994	447,552	431	53.63
18	1,501	17.40	261,174	460,512	432	56.71
19	1,568	17.40	272,832	471,552	433	57.86
20	1,630	17.60	286,880	479,810	434	59.79
21	1,695	17.60	298,310	486,050	435	61.37
22	1,753	17.60	308,528	494,210	436	62.42
23	1,012	11.81	119,517	238,603		50.09
24	1,197	11.80	141,246	268,970		52.51
25	1,350	11.83	159,681	297,829		53.61
26	1,501	11.83	177,568	321,918		55.16
27	1,630	11.92	194,196	341,127		56.93
28	1,753	12.00	210,360	357,007		58.92
29	2,029	12.33	250,175	416,954		60.00
30	2,301	12.12	278,881	463,268		60.13
31	2,544	12.17	309,604	503,759		61.46
32	2,765	12.17	336,500	553,346		60.80
33	2,933	12.75	373,867	558,870		64.04

Mr. Parsons deduces some very valuable facts in relation to centrifugal pumps, which may be briefly stated as follows:

First—There are two totally different conditions in which a centrifugal pump may be situated while it is rotating. One, in which it is revolving just fast enough to raise the water up to the discharge pipe, and no farther; and another in which it is revolving slightly faster, and discharging water out of this pipe. In the first case there is only centrifugal force, which is produced by the water in the fan rotating, that maintains the column of water in the discharge pipe.

In the second case this force is still produced, but in addition to it another, which may be called the force of impact, or, in other words, the force with which the blades of the fan impinge against the water discharged by the pump.

Second—That a fan when rotating will support a column of water the velocity due to whose height is equal to the tangential velocity of the circumference of the fan.

Third—That the internal angles of the blades vary with both the lift and discharge with which the pump is intended to work.

Fourth—That the pump should be so proportioned in its passages as to have a gradually increasing velocity in the water until it arrives at the circumference of the disk or fan, and then to have a gradually decreasing velocity until it issues from the discharge pipe.

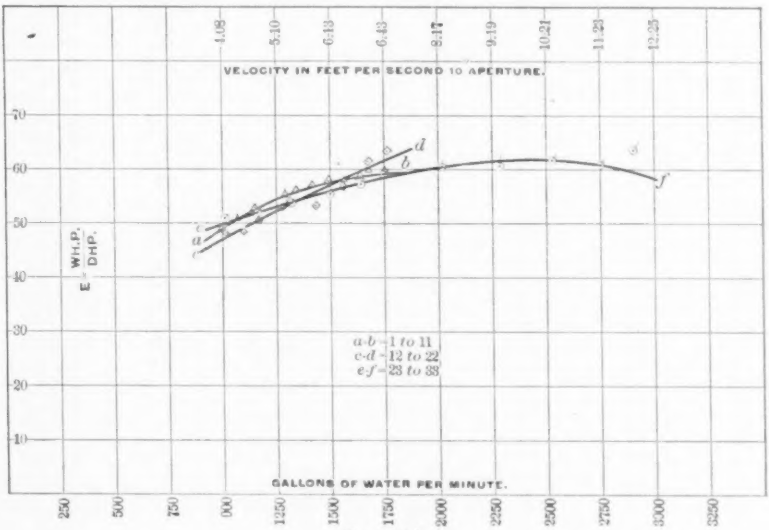

FIG. 164.

This condition is obtained by having a conical or tapering suction pipe, a spiral casing surrounding the fan, a proper amount of "whirlpool" space in which to eliminate the "eddies" produced by the ends of the blades of fans, and a tapering or conical discharge pipe. And lastly, that the tangential velocity of the fluid being pumped, on leaving the fan or disk, should be not more than from 24 to 30 feet per second.

These facts would seem thoroughly to dispose of the fallacy in

making a centrifugal pump of one diameter of disk to give the best results and efficiencies for all conditions of lift and elevation of fluids being pumped.

The writer would here present for comparison two diagrams, one plotted from the results just given in a tabulated form of the experiments made by Messrs. Parsons & Hesketh, and the other

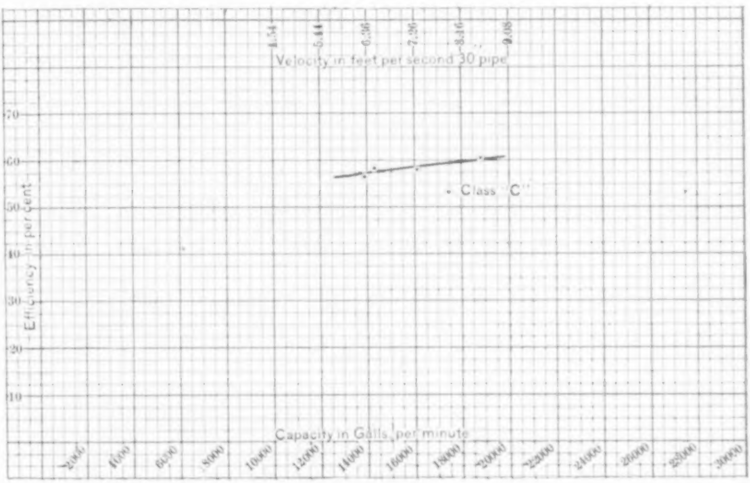

FIG. 53.

from his own experiments made with a smaller pump, and published before in a paper, A. S. M. E., Vol. VII., page 608 (Fig. 164).

Also, another diagram plotted from some of the best efficiencies of large pumping plants with which the writer is familiar, and a table giving some data regarding these plants that may be of value as collected in this form, although some of the data have been published before:

TABLE VI.

	Location and Maker.								
	Bultewyker Polder Amsterdam. Gwynne.	Gwynne.	Fas Bouches du Rhone. Gwynne.	Fas Bouches du Rhone. Gwynne.	Bijlmermeer, Amsterdam. Gwynne.	Mystic Pond. Hibbard.	Woburn. Hibbard.	Portland, Me. Hibbard.	Erie Basin. Andrews.
Size and class	30″ B	30″ C.	30″ B	30″ B	30″ B	18″ 3-2	18″ B	24″ 3	30″ B
Diam. cylinder	20″	8.5″-16″	....	....	....	....	....	....	....
Stroke "	18″	16″	....	....	....	....	....	....	....
Rev. per minute	135	....	114.2	114.1	....	....	....	180	....
Diameter suction and discharge	30″	30″	30″	30″	....	....	....	24″	30″
" of disk	67″	....	....	....	....	....	....	60″	....
Lift in feet	14.3 to 15.3	4.52	5.48	5.27	14.7	8	13.1	9.1	9.21
Average discharge in G. P. M.	16156.8	17405.4	14252.4	13993.2	18816	7639.3	6944	14763.27	16634.77
" " " cu. feet P. M.	2160.	2327.	1905.4	1870.7	2515.5	1021.3	928.3	1973.7	2223.9
Water lift in H. P.	62.9	19.64	24.03	22.76	70 +	15.42	21.78	34.11	23.33
Indicated H. P. Engine	107.52	36.569	41.322	40.207	114.19	....	....	....	....
Efficiency W. H. P. ÷ I. H. P.	58.5 %	53.75 %	57.9 %	56.7	61.3	....	....	....	....
Total coal consumption	1938 lbs.	270.05	810.5	794.128	....	2000 lbs.	1950 lbs.	5000	2031
" " " per hour	325 lbs.	90.01	101.313	99.266	326.9	83.33	812.4	1250	1015.5
" " " I. H. P. " "	3.02	2.425	2.44	2.48	2.86	....	....	....	....
" " " W. H. P. " "	5.22	4.459	4.21	4.37	4.67	5.4	37.3	36.6	43.5
" fuel water consumption	....	2156.16	6749.816	....	....	....	....	....	....
" " " " per hour	....	718.722	843.727	827.358	....	....	....	....	....
" " " " I. H. P.	....	19.67	20.32	20.67	....	....	....	....	....
Lbs. coal per lb. water	....	8.11	8.367	8.367	....	....	....	....	....
Duty	36,886,153	48,846,294	38,648,515	37,243,070	42,418,774	36,698,001	5,560,341	5,456,862	7,744,917
Duration of test	6 hours	3 hours.	8 hours.	8 hours.	....	....	....	4 hours.	2 hours.

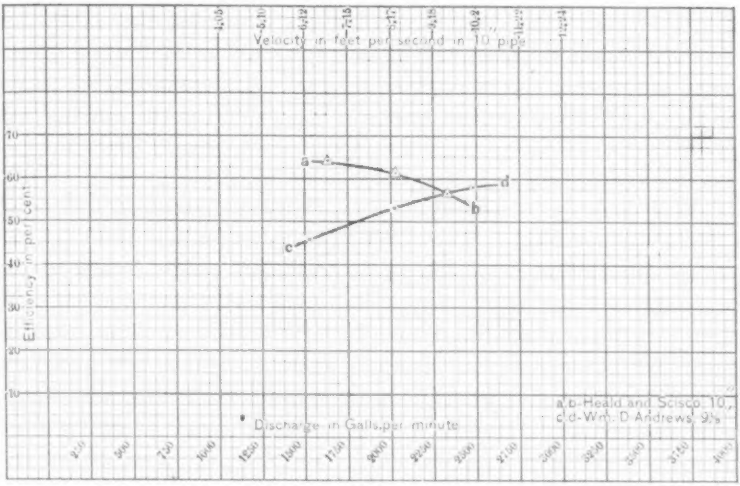

FIG. 54.

TABLE VII.

	MAKER.						
	Andrews.	Andrews.	Andrews.	Heald & Sisco.	Heald & Sisco.	Heald & Sisco	Berlin Schwartzkopff.
Size	No. 9	No. 9	No. 9	No. 10	No. 10	No. 10	No. 9
Diam. discharge.	9½"	9½"	9½"	10"	10"	10"	9¼
" suction	9¾"	9¾"	9¾"	12"	12"	12"	10.3
" disk	26"	26"	26"	30 5"	30 5"	30 5"	20.5
Rev. per minute.	191.9	195.5	200.5	188.3	202.7	213.7	500
Gals. per minute	1513.12	2023.82	2499.33	1673.37	2044.9	2371.67	1944.8
Height in feet	12.25	12.62	13.08	12.33	125.8	13 0	16.46
Water H. P.	4.69	6.47	8 28	5.22	6.51	7.81	....
Dynam'eter H. P.	10.09	12.2	14.38	8.11	10.74	14.02	11
Efficiency $\frac{W.H.P.}{D.H.P.}$	46.52	53.0	57.57	64.5	60.74	55.72	73.1

DISCUSSION.

Professor R. H. Thurston.—The case is practically the same as that of fan blowers, and the remarks which might be made in regard to this case are substantially the same, I presume, as would

apply to that. In connection with the paper on fans the remark was made that Professor Rankine's form of fan admits a form of vane which shall be curved during the earlier portion of its line and should become radial at the end, and that is the correct form for that type of pump. In this paper it is stated that that form proved to be entirely inadequate to its work. I never have been able to ascertain exactly the proportions of the pump as treated in the case referred to here. The paper, of which this seems to be an abstract, was published in full; but many details of very great importance were omitted, and although I endeavored at the time to get more details, I never could ascertain what were the principal dimensions. I presume, however, the discrepancies between the claimed high efficiency of the Rankine form of centrifugal pump and that shown by experiment are very likely due to the same form of misapprehension as existed in regard to the form of the fan blower; that is to say that while the form prescribed by Rankine is absolutely correct for the conditions under which Rankine uses it, if that same form be used under other conditions, whether they be differences of general form and proportion or in method of construction of casing, they will probably prove very inefficient. I have a faint recollection that at the time of the publication of these results, or soon after, a debate arose between D. K. Clark on one side and engineers holding a contrary opinion on the other, in which the two sides showed, each to its own satisfaction, that the results of those tests proved that Rankine was wrong—and also, on the contrary, that Rankine was right; and I imagine that the statement given here can hardly be taken as a proof that the Rankine form of pump is not a correct one.

On the other hand, it is not to my mind clear that the Rankine form is the best for all cases. In one case, at least, in which I endeavored to design a pump on the Rankine principle, I found that the endeavor to get that form of curvature of blade led me to make a very awkward form of pump and such form of channels as proved unsatisfactory; and I gave up that form of blade entirely and adopted the Appold form substantially, and so obtained a very satisfactory design. I presume the fact is that any one of a thousand different forms of pump-vane can be adopted for certain thousand sets of conditions. I presume the general case is simply this: the method of operation of every pump of high efficiency is one in which the water is taken in at the axial line, is diverted more or less in a radial direction, is given an accelerated

motion, and the acceleration of rotation is continued with radial motion until, at a certain point at a fixed radial distance from the center—which distance is determined by the size and proportion of the pumps—that rotation is so great that the centrifugal action will balance the head against which the pump works; which head includes not simply the static head but also that due to friction and to the acceleration itself. Then that zone of water revolving at this maximum velocity, being passed, the problem of the engineer is next to reduce that velocity, to take out kinetic energy until the velocity of the water is brought down to a limit which is as low as outside conditions will permit; that is to say, down to the speed of delivery of the water. In order to effect that result, we may have any one of a number of different forms of blade. Rankine would make the zone at which equilibrium occurs half way between the axial line of the pump and the outer surface of the casing. He would take the first portion of this path to give the water its maximum velocity; then would use the other portion, which would be exterior to the pump vane entirely, as what he calls a "vortex chamber," in which the water, in its whirl, should gradually reduce its velocity until it should come down to that of delivery. In that way the primary and essential condition of getting centrifugal action enough to overcome the resisting head is attained; and then again the excess of energy in the water at that point over that required for its delivery, finally, is restored and economized, and maximum efficiency of pump is attained. But that same action may be obtained by entirely different forms of vane from those given by Rankine. In the case which I have in mind I was not able to get a satisfactory design using the Rankine vane; but I was able to get it by the involute form of vane, and could have obtained it by a great number of other forms of vane in which the water would be taken on without shock and delivered at the demanded velocity, the real condition of efficiency.

I presume, without having enough experience to be dogmatical about it, that it is possible, within limits, to design a pump to do a given amount of work with a given speed of rotation. If I wanted to drive a pump at a very high speed of rotation I should adopt the Appold curve, carrying it back so as to produce a sharp curvature. If I wanted to run my pump at moderate speed I should adopt more nearly Rankine's form of curve. If I wanted to drive at a low speed of rotation I should adopt a form which would be like that which I showed this morning. In making such

designs, or rather in designing pumps or blowers for general work, I think it will be found advisable to modify the form of vane to suit the velocity desired to be given it.

I would suggest, further, that there are other modes than that adopted by Prof. Rankine, and before him by Prof. Thompson, of utilizing energy which has been thus stored in the whirl. One is to make the pump-vane of such form that it shall bring the speed of rotation up to the desired point; then carrying the blade beyond the point at which Rankin would terminate his, throwing it sharply backward in such a way that the issuing jets shall be thrown backward, and surplus energy taken up so as to act effectively in balancing the action of the pump itself, and in securing maximum efficiency.

Another way of converting kinetic energy is that of receiving water issuing at maximum speed—as in a pump in which the vanes are radial at their extremities—in a cone-shaped pipe in which its velocity may be gradually reduced by the expansion of the pipe and by increasing the pressure head within the cone, thus producing exactly the same result as the other devices. So it would seem, I should say, very probable that a great variety of types of centrifugal pumps and blowers may be made, each of which shall be founded on a specific method of first producing and then of economizing, the energy of rotation.

Prof. De Volson Wood.—I may be wrong about this particular form of the Rankine pump; but this is my impression in regard to it. Rankine, I know, in some of his analyses, takes the case in which the discharge from the vane of the pump is radial, and in which the direction of the first element is such that when it is running with a proper velocity there shall be no shock. That was mentioned this morning. Now Rankine assumes that the proportions are properly made, and then analyzes the problem. He assumes that the best efficiency will be produced when the direction of the discharge is radial. This is very nearly true. It is not theoretically exact, but it is very nearly exact. Now if that is the case, it may be certain that if a wheel be run with a different velocity, then you get a different efficiency, so that if a Rankine centrifugal wheel were properly proportioned to accomplish this end, and somebody experimented upon it, running it with a velocity half what it ought to have or twice what it ought to have, it will not give the best efficiency. Now in regard to the form of the vane, I understood the last speaker to say that there was a defi-

nite form between the ends of the vane. That principle I do not understand. Rankine, in his analyses—and I only refer to Rankine because he has been referred to here as having given the standard analysis—other analysts do the same—they give us the direction of the vane at the start, the initial element; they give us the direction of the vane at the final element. Between those you may have, as the last speaker suggested, an infinite number of curves. It shall be a smooth curve; but I have never seen anything which determines its law. It is arbitrary with you; it is arbitrary with me; it is just as arbitrary as the path of a particle from a point to a horizontal plane, the velocity will be the same whether it goes down a straight line or a curved line, friction neglected. So with this there may be an infinite variety of forms between the ends of the vanes.

One word in regard to an experiment which I once made with two pumps to test their efficiencies. I had the pumps taken apart, and one of them showed a smooth passage-way in the bucket or vane, while the other had an angle in it, and I said to myself the former pump will beat of course. It is constructed on the right principle. We ran the two pumps slow at first: the former did better than the latter. We ran them at a higher speed: the former still did better than the latter, but not so much better. We ran them at a still higher speed, and the former was only a little more efficient than the latter. Experiments were stopped at that time. I did not make the computation at the time, and consequently did not know the facts which I have just stated. But afterward in making the reductions it appeared quite plain that if we had run both at much lower speed, requiring them to do greater work, the pump having the angle in its bucket would have given a better efficiency than the other, so that I was convinced that a simple inspection of the pump was not sufficient to determine whether, at all speeds, one would be better than the other.

Mr. Hugo Bilgram.—It appears to me that the difference between the blades with radial ends and those of an involute form may be due to the section of the pump. When following the path which the water assumes, after entering the central orifice, we find it takes the form of a spiral as shown in sketch. The angular velocity of the mass of water in rotation is the same at all points, but the linear velocity increases with the distance from the center. If the section is such that the width decreases as the dis-

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tance from the center increases, namely, if the outline follows the form of a hyperbola, the spiral which the water describes will be strictly an arithmetical spiral, which will be steeper when a greater quantity of water is passing, owing to less pressure, and of less rise when a high head retards the flow of water. It is well known that the force which the blades must transmit to the water will be transmitted with the greatest efficiency if the blades are at right angles to the direction of the impelling force. Therefore all elements of the curve of the blades should be at right angles with the path of the water. It should, therefore, be an arithmetical spiral in the opposite direction, as indicated in the sketch Fig. 55, and its form will be steeper, *i.e.*, more nearly radial, as the pressure to be overcome is greater.

When the section of the fan is square instead of hyperboloidal, the spiral, instead of being of the arithmetical type, will con-

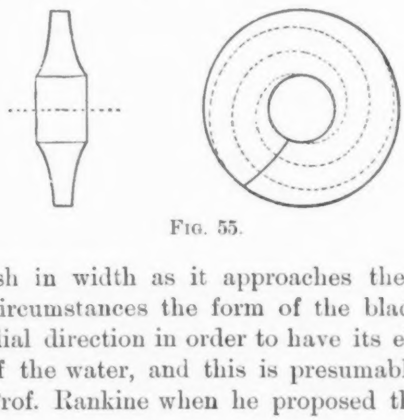


FIG. 55.

stantly diminish in width as it approaches the circumference. Under these circumstances the form of the blades will tend to approach a radial direction in order to have its elements normal to the path of the water, and this is presumably the condition assumed by Prof. Rankine when he proposed this form of the blades.

Prof. Thurston.—He precisely inverts that. He proposed that form of blade for your first case for the hyperboloidal section.

Mr. Bilgram.—Then I fail to see why the form at the tips should approach a radial direction, for the transmission of force is certainly not as advantageous as it would be were the blades at right angles to the motion of the water, *i.e.*, to the direction in which the accelerating force is to be transmitted.

After* a more mature consideration of the problem I found that I erred in stating the curve which runs normal to an arithmetical spiral to be also an arithmetical spiral, while in reality it

* Added after adjournment.

becomes more nearly radial as it leaves the center; hence the curve given by Prof. Rankine is really complying with the conditions which formed the basis of my remarks.

Prof. J. E. Denton.—I fail to be convinced by the contents of the paper or by the remarks of Prof. Thurston that there can be any such inferiority in the Rankine form of blade as the paper claims.

Of course, theoretical speculation or deduction must yield, when it is shown to be opposed to the results of actual measurement, but such theory as is involved in the principles of Rankine's centrifugal pump requires the most positive experimental evidence to weaken it. I think that any practical engineer will admit, whatever his just skepticism regarding the ability of mathematicians to derive results which are capable of practical application, that the history of the development of turbine water-wheels testifies to the fact that all improvements in such wheels represent inventions to accomplish three things, viz.:

(1) To enable as much as possible of the whirling motion of the water to be taken from it by the blades of the wheel.

(2) To enable (1) to be accomplished with the least possible size of wheel per horse power.

(3) To enable (1) and (2) to be accomplished consistently with the smoothest and most direct passage of the water into and through the wheel.

These are principles, the realization of any or all of which in the proportions of a turbine wheel are indisputably connected with the improvement of that wheel, by the testimony of all intelligent hydraulic engineers. Consequently, we have a right to expect that the reverse proposition will be equally true, viz.: Any design which realizes these three principles completely, must be superior to any that realizes them in a less degree, and it is under this proposition that I would have Rankine's statements judged regarding the radial tipped blade in centrifugal pumps. The three desirable elements in the pump are similar to the turbine—

1. That the water shall enter smoothly; which the inclination of the inner end of the blade accomplishes.

2. That, as much as possible of the rotary motion of the blade must be imparted to the water, which can be best accomplished by pressing upon the water in a direction normal to the rotary path of the blades. This requires that the blade be radial, and hence it is made to become so as quickly as possible by connect-

ing the inclined inner edge by the shortest possible smooth curve, leading to a radial outer extremity or tip.

3. That the given centrifugal effect shall be obtained with the least possible size or weight of wheel. This also calls for a radial blade, for a curved blade would accelerate the water in a direction inclined to the radius, and the rotary component (which is alone available for raising water through centrifugal force) of such acceleration would be equally well obtained by a radial blade of less extent or weight. The advantages regarding friction cannot be favorable to the curved blade, which affords an unnecessarily indirect path to the free vortex beyond the pump blades.

These simple fundamental principles should merit the same confidence as those accepted for turbines, and before I shall be willing to sacrifice any of my own confidence in them to opposing experimental deductions, I must see a better means of measuring power than the one set forth in this paper, viz., the measurement of the quantity of water evaporated from a boiler in a given time, the possible error of which we all know to be such as to stamp the method and its results as positively crude, and throw a doubt over all the data presented.

No experiments are more difficult to perform accurately than the measurement of the action of fluids. Turbine tests have now received such elaborate attention that the value of slight variations of form in a wheel can be reliably determined, but centrifugal pumps and fans, I believe, have yet to receive the benefit of such measurements, and I, therefore, do not believe that any experiments have ever reliably disproved such sound theory as Rankine has left us, and I do not find myself able to suppress the thought that until Prof. Thurston is ready to supply us with the results of experiment thoroughly and satisfactorily disestablishing Rankine's views, he should avoid any attempt to destroy the force of an authority whose genius in mechanical analysis was of the nature of inspiration.

Prof. Thurston.—I merely desire, since I have been referred to, to say that I have made no experiments, and know no facts, that look in the direction of overturning Professor Rankine's theory. I very cordially indorse all that has been said in regard to Professor Rankine and his work. What I meant to say, and what I *did* say, was that the apparent discrepancies between what has been shown in this paper, as telling against Rankine's work, and the results that were predicted by Rankine, or were antici-

pated from the use of Rankine's method, probably are due to the very sort of misconception that the gentleman who has just spoken has indicated.

Prof. Wood.—I wish to make one favorable remark on this paper. I thought I had it before me, but as I have not, I will refer to it in a general way. The paper speaks of bringing the water to the pump so that it shall have its greatest velocity at the pump and gradually lose its velocity from the pump to the end of the discharge pipe. It seems to me to be a beautiful recognition of the principle of energy where the shock of sudden change of velocity is avoided, and that the principle, theoretically, was a good one.

*Mr. Wm. O. Webber.**—I have read with a great deal of interest the discussion of my paper by Profs. Thurston and Wood, and Mr. Bilgram, and while they may be right in sustaining Prof. Rankine and his formulæ, for the exact speed and other conditions for which he calculated them, still I am convinced, from the very numerous experiments which I have made with over twenty different forms of disks, and with the apparatus described in my first paper on Centrifugal Pumps, which I know to be accurate, and which Mr. Jas. B. Francis has seen and approved of, that Rankine's formulæ will not and do not give the best results when applied to the ordinary commercial use of such pumps. In the first place, in a commercial sense, it would not be possible to design a pump for every different condition of lift and speed. So for this reason I have designed three classes or sizes of pumps, called respectively A, B, and C; the A, being for higher lifts and low speeds; the B, for moderate lifts and medium speeds, and the C, for low lifts and faster speeds.

I have found that a curve representing very nearly the curve of rolling friction, *i.e.*, the epicycloidal curve, drawn tangent to the circle of the hub of disk, with a diameter of rolling circle equal to one-half the radius of disk and prolonged at the same degree of increasing curvature to the periphery of disk, has given the best results, not only by experiments in the testing flume but in actual practice in every-day work. I propose, as soon as my duties will allow me, to furnish another paper on this subject of forms of disks to the society for their consideration and further discussion.

There is a pump now built in this country using the Rankine fan with the curvature changed to radial on the outer ends of

* In closing discussion under the rules.

blades, and no tests of this pump in actual service have approached the efficiencies obtained by Gwynne's pumps in England or my pump in this country. It may be, of course, that the Rankine pumps never happened to be in the exact condition for which they were best adapted, but the fact remains the same as I have stated.

I have fully appreciated the value of the vortex chamber in my pumps, and in my later designs have increased its amount nearly 50 per cent., but I do not think, and have experiments to back my opinion, that the diameter of the disk should be in any case less than equal to one-half the diameter of vortex space, and think further that the proportion should be about three-quarters of disk to one-quarter of vortex chamber at the greater, and down to seven-eighths and one-eighth at least.

CCLXXV.

THE USE OF KEROSENE OIL IN STEAM BOILERS.

BY LEWIS F. LYNE, JERSEY CITY, N. J.
(Member of the Society.)

THE action of kerosene oil for the prevention of scale in steam boilers is a subject upon which the books appear to be silent. At least the writer has been unable to find any practical data that could be made use of in determining its value as a scale destroyer. It has often been recommended, but its application for that purpose, so far, has been quite limited. Many theories in regard to its action inside steam boilers have been indulged by engineers, but no systematic course of treatment has yet been brought to my notice. Therefore a portion of practical experience in that line is presented herewith, with the hope that others may be induced to compare notes and all parties concerned receive substantial benefit. In consultation with an engineer for whom I entertain a very high regard, he suggested that I try crude petroleum, as it was better than kerosene, but he gave no plausible reason why, nor advice as to the quantity, or method of introducing it. There was but one course left for me to pursue, which was to try some experiments.

At the Jersey City Electric Light Company's station we have two sectional boilers of the new Root type, 100 horse power each, and one of 155 horse power. We use the Passaic water, which makes a great deal of scale, and in the steam space I have noticed a very marked corrosive action, more especially upon the cast-iron flanges of the safety valves. Within the shell boilers which we formerly used hard scale was formed to an alarming extent, and we could not get a scraper between the tubes, to remove all of it. In the dry season salt water found its way into the reservoir, and I now have large lumps of saline matter which were removed from our boilers.

In the boilers which we are now using hard scale would form so as to more than half fill some of the four-inch tubes of which our boilers are principally composed. Something had to be done. I had tried several compounds, with more or less degrees of success, but still the interior of our boilers could not be kept clean. We

tried blowing off, used scrapers and other devices, but without obtaining permanent relief. The deposit was mostly in the lower row of tubes and within four feet of the back ends near the mud drum. The loss of heat due to this incrustation was great.

Nystrom gives the following formula for calculating this loss: $H=\frac{t^2}{32\times t^2}$, in which t equals the thickness of scale in sixteenths of an inch, and H the per cent. From this it would seem that a scale of one-sixteenth would cause a loss of about fifteen per cent.; three-sixteenths, twenty-three per cent., and so on. Some have claimed the conductivity of scale to be at least thirty per cent. less than iron, hence we recognize the necessity of keeping boilers free from scale and sediment.

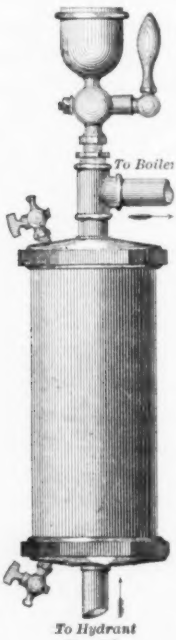


Fig. 56

As a preliminary experiment with kerosene oil, I took a test tube of one inch diameter, and in it placed a lump of scale taken from our boilers. A table-spoonful of water was then added, with a film of kerosene upon its surface. Heat was then applied from a Bunsen burner. When ebullition began, the kerosene separated into globules and followed the sides of the tube to the bottom; thence they arose through the center to the surface. This action continued as long as heat was applied, and proved conclusively, to my mind, that the kerosene would not remain upon the surface of water in a boiler, as has been argued by some engineers. I have since proved this correct beyond a doubt, by drawing water impregnated with kerosene from the bottom of the boiler. From the time the water in the test tube began to simmer, the scale commenced to disintegrate, and continued until nothing was left but the hardest substances.

I concluded from this experiment that kerosene oil was just the thing to use in our boilers, so the apparatus shown in the accompanying engraving, Fig. 56, was made, and attached to the feed pipe. It is an inexpensive affair, made of a piece of five-inch iron pipe, twelve inches long, with a cap screwed upon each end as shown. A pipe at the bottom connected with the hydrant, while the pipe at the top conveyed the kerosene into the feed pipe when the water was turned on. A tallow cock was screwed into the top for filling

while the air and water were drawn off at the stop cocks shown for that purpose. When the feed water is taken from a tank, or well, where there is no pressure to force it into the boiler, a globe valve must be placed in the feed pipe, so as to compel the water to flow temporarily through the reservoir, in the direction shown by the arrows, carrying the oil along with it. Our feed pipes used to clog up with hard lumps of scale and rust from the water, but since we began using kerosene these pipes are all clean and they do not rust. It herefore recommend that the reservoir be placed so that the kerosene may pass through all the water pipes, if possible. When our apparatus was ready for use the water was blown off from a 100 horse-power boiler, the blow cock closed, and two quarts of kerosene oil introduced. The injector was then started (by steam from one of the other boilers), and as the water rose, the kerosene reached every part of the interior surface. Before filling this boiler an examination was made, which showed scale in the tubes of three-sixteenths to one-quarter inch in thickness, while in the headers it was half an inch thick in places. We never put cold water in our boilers, either to wash or fill them, unless they are cold, for I have known cracks to originate in steel boilers by so doing. We put in two quarts of kerosene every other day for one month, when this boiler was blown off at ten pounds pressure. It was then opened and examined, when we found that the scale was partly dissolved and loosened so that a scraper removed most of it from the inside of the tubes. The scale in the headers remained quite hard, although the surface was softened by the action of the kerosene. The blow-off cock was then closed, and two quarts of kerosene put in, after which the boiler was filled with water, as described. The object of putting in the kerosene first is to have it penetrate the scale and loosen it, as the water rises; and we know that it does this. During this second month, we used the same quantity of kerosene, and in the same way as the first month. At the expiration of that time the water was blown off, and an examination revealed the tubes perfectly clean; there was, however, some scale left in the headers, but it was so soft that it could be easily removed with the finger-nails. We used no scraper this time, but just closed the boiler, put in two quarts of kerosene, and filled it as at first. During the third month we blew down two gauges of water every week, and used the same quantity of kerosene as before. At the expiration of the third

month we blew off the water and opened the boiler, when, to our satisfaction, we found it *clean*—a condition that never before existed since we started the boilers. The dirt had all settled in the mud-drum and when the blow-off cock was opened, it passed into the sewer. Not a teaspoonful of sediment was found inside this boiler. I closed this boiler and ran it three consecutive months without opening the blow-off cock or changing the water during that time, using the same quantity of oil as before. At the end of the three months the water was blown off, and no dirt was found in the tubes, and very little soft mud in the mud drum. This I thought was a very conclusive experiment. We then adopted a rule of one quart of kerosene oil per day for each of the 100 horse-power boilers, and three pints for the 155 horse-power boiler. The water is blown down two gauges every week, and the entire contents every month. Water is never used to wash them out, nor is a scraper necessary; for the mud all goes out with the water. An examination is made of the interior, and we put them to work again. This is a wonderful relief to us, for the reason that no scale forms in any of our boilers, and the corrosive action mentioned as having existed at first has entirely ceased.

For the information and convenience of our members who are chemists, I append an analysis of the Passaic water during the year 1882, from which they may readily arrive at a fair average of the practical conditions under which our boilers are operated.

Another thing worthy of special notice is, that it was impossible to keep a glass water tube in use more than three months at a time, and oftentimes they would break within two months. Before using kerosene these tubes would become badly grooved and eaten away at the upper ends so that they would break. Our engineer came very near losing his eyesight through the breaking of one of these glasses, and his face was badly disfigured by being cut with the broken glass. Now these tubes do not show any such action, and they have been in use more than a year.

I admit that rubber packing and kerosene oil do not agree; so to guard against any trouble from that source, I had new nuts one and one-half inches deep placed at the ends of the glass tubes, and used asbestos wicking, dipped in boiled oil and then squeezed dry, for packing. They do not leak, and these joints are permanent.

I never use rubber packing in flange joints about a boiler, or in any place where it comes in contact with steam. For faced joints, I insert asbestos in sheets one-sixteenth inch in thickness; and for

TABLE SHOWING A MONTHLY ANALYSIS OF THE PASSAIC RIVER FOR 1882.

BY PROF. R. A. LEEDS, PH.D., STEVENS INSTITUTE, HOBOKEN, N. J.

	January.		February.		March.		April.		May.		June.	
	Parts in 100,000.	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.
Free ammonia	No Analysis taken.	No Analysis taken.	0.001	0.00058	0.0045	0.0026	0.0035	0.00204	0.004	0.0023	0.001	0.00058
Albuminoid ammonia			0.016	0.0093	0.018	0.0105	0.016	0.0093	0.018	0.0105	0.026	0.015
Oxygen required to oxidize organic matter			0.81	0.47	1.21	0.70	0.82	0.478	0.82	0.44	1.315	0.797
Nitrites			None.	None.	None.	None.	None.	None.	None.	None.	None.	None.
Nitrates			1.12	0.64	0.60	0.35	0.58	0.335	0.58	0.338	0.306	0.175
Chlorine			0.53	0.309	0.35	0.204	0.375	0.218	0.4	0.23	0.35	0.204
Total hardness			4.05	2.33	4.05	2.36	4.90	2.33	4.0	2.33	3.00	1.75
Total solids			....	....	8.50	4.95	6.0	5.25	9.0	5.24	9.00	5.24
Mineral matter			....	....	6.50	3.79	6.0	3.50	4.5	2.62	5.00	2.91
Organic and volatile matter			....	....	2.00	1.16	3.0	1.75	4.5	2.62	4.0	2.33

	July.		August.		September.		October.		November.		December.	
	Parts in 100,000.	Grains per Gallon.	Parts in 100,000	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.	Parts in 100,000.	Grains per Gallon.
Free ammonia	0.0025	0.00145	0.002	0.0012	0.0045	0.00262	0.0125	0.0072	0.0015	0.00087	0.0055	0.0032
Albuminoid ammonia	0.029	0.0169	0.028	0.016	0.0285	0.0166	0.0435	0.0253	0.0235	0.0137	0.0155	0.009
Oxygen required to oxidize organic matter	0.92	0.54	0.94	0.548	1.20	0.70	1.07	0.62	0.48	0.28	0.55	0.32
Nitrites	None.	None.	None.	None.	None.	None.	None.	None.	Trace.	Trace.	0.00005	0.000029
Nitrates	0.37	0.215	0.074	0.431	0.66	0.38	0.34	0.198	0.58	0.338	0.486	0.283
Chlorine	0.44	0.256	4.9	2.86	14.00	8.16	0.525	0.306	0.55	0.32	0.525	0.306
Total hardness	3.30	1.92	3.40	1.98	4.00	2.33	3.77	2.19	4.1	2.39	3.1	1.81
Total solids	10.00	5.82	11.00	6.42	25.00	14.57	8.5	4.95	9.5	5.54	12.00	7.0
Mineral matter	5.50	3.20	6.00	3.30	18.00	10.49	5.0	2.90	6.0	3.50	9.00	5.25
Organic and volatile matter	4.50	2.62	5.00	2.92	7.00	4.08	3.5	2.04	3.5	2.04	3.00	1.75

rough joints, the same material one-eighth inch thick. After purchasing the asbestos, I spread upon both sides, with a brush, all the boiled oil that it will readily absorb, and then hang up the sheets until required for use. When a gasket is needed, we cut it to the

required size, then rub both sides well with pure graphite, and screw it up hot, after which no more attention is required. We have joints made in this way which have been in use more than three years, and as yet have showed no signs of leaking. I took some of them apart recently, and the surfaces separated very nicely, and there was no evidence of rusting or corrosive action upon them. A short time since, one of our neighbors had great difficulty in making a joint upon a badly rusted mud drum of a shell boiler, and it could not be kept tight for any considerable length of time. He made a gasket of asbestos prepared as described, and has not touched it since. In taking apart our flanged joints, we broke no bolts nor split any nut, as is usual with such joints made in the ordinary way; for, when those joints were made, we smeared the threads and nuts with graphite mixed with boiled oil. This mixture occasionally applied to the stems of safety valves will prevent them from corroding and sticking fast. A small monkey wrench easily removed the nuts, and none of them were rusted. This is the way we treat all of the nuts and bolts upon our boilers, and the same mixture is used in putting up steam pipe. The result is that we break no fittings nor do we split any pipe in taking them down, after years of service. I am aware of instances where graphite used upon iron surfaces has shown, when taken apart, a sort of hard scale, having the appearance of some sort of cement. This is liable to occur when the graphite contains foreign substances, such as silica and sulphur. The common, cheaper forms of graphite will not give satisfaction for these purposes; it must be pure. The kind that I now use, and have used for the past fifteen years, is made by the Dixon Crucible Co., who have a special process, by which they remove all earthy and mineral substances, leaving the flakes of graphite pure. I have never had any unpleasant experiences with this grade of graphite, and have used it extensively, not only as described above, but also in the cylinders of steam engines. In conclusion, I desire to say that crude petroleum has, to my certain knowledge, been used in steam boilers during the past eleven years and upward, where, with judicious application, it has been successful in removing and preventing scale. While this is admitted, I must also acknowledge that great damage to boilers has resulted by not observing the necessary precautions in the quantity put into the boiler each time. I will mention but one instance, which is that of a tugboat now running in New York harbor. The boiler was badly scaled, and some one advised the engineer to use crude

petroleum; so he "*gave the boiler a good dose*," as he said. In a few days the tubes began to leak, and the crown-sheet bagged down. The boat was then laid up, when it was found that the heavy oil had mixed with the mud and had formed a paste on the crown sheet. This paste kept the water from reaching the plates, hence the result stated above. This paste was so dense that water from a hose would not dislodge it; and I do not hesitate to say that, had kerosene oil been used in this instance instead of crude petroleum, the boiler would not have been injured. The reason is that there is not sufficient body in kerosene oil to form a paste. The chief objection to crude petroleum is that it is too heavy, while in kerosene oil there is no substance which will stick fast to the interior of a boiler.

In kerosene oil we find all that is necessary to accomplish the desired object without the objectionable features just mentioned. I appreciate the fact that there may be instances where steam is blown directly into fibrous materials in the course of their manufacture, and also in the preparation of articles of food, where the odor or possibly the taste of kerosene oil might be disagreeable; but such instances are few, when compared with the many, where it might be successfully employed for the prevention of scale in steam boilers. Some engineers advance very queer theories against the use of kerosene; but I must assert that, for the most part, they are only imaginary. We have as yet found no objections in our experience. Our boilers do not lift their water, they are free from scale, and our fuel bill is thereby greatly reduced.

DISCUSSION.

Mr. J. T. Ridgway.—There are many engineers who never have used mineral oil or its products in steam boilers as a scale remover, and Mr. Lyne's paper will be of great benefit to those who are troubled with scale in their steam boilers. Whether the formula quoted by Mr. Lyne from Nystrom, to calculate the loss in evaporation due to the presence of scale, is correct or not, we are all agreed that heavy scale in steam boilers is not only a great loss in the evaporation of water, but even positively dangerous from the liability of burning the plates, which thus are deprived (whether iron or steel) of the necessary strength to withstand the steam pressure; frequently disaster results to the whole structure.

I have been using crude petroleum for the past twelve years, and,

by experiment, have found it more efficient than kerosene, but have been very careful to use crude oil 44° to 50° gravity, and not an oil that has been reduced and put on the market as a cheap lubricating oil. Different types of boilers require different treatment. While Mr. Lyne could charge the Root boiler heavily and frequently with impunity, as the detached scale would soon find its way to the mud drum, and be remote from the hottest place in the boiler, in a horizontal tubular boiler such would not be the case. The scale, being loosened from the flues and shell, would collect in one mass on the bottom sheet in position to do much harm (when there is heavy firing) if not soon taken out. My practice has been (and with good results): Three days previous to washing out in a 54″ × 15′ boiler, to put in about a quart of crude petroleum (not lubricating oil); the day before washing out repeat the dose, and on the day that the fires are to be hauled put in about two pounds of sal soda. Then wash out from the top first, with strong water pressure. On removing manhole or handhole plates below flues, on the lower sheets there will be a fine collection of loose scale, which can be easily and quickly taken out.

The apparatus which I use for introducing the oil and soda into the boiler is not as elaborate as the one used by Mr. Lyne. In the water supply pipe near the injector, I put in a quarter-inch pet cock, on the end of which is attached a piece of rubber tubing, say 12 inches long. To operate this, have the desired quantity of oil or soda water in an open vessel. Start the injector, giving it a limited supply of water, drop the free end of the rubber tubing into the liquid, and open the pet cock. The contents of the vessel will very soon be drawn through the tubing by suction, and must necessarily pass into the boiler.

The whole expense of the rig is about one dollar.

Mr. J. H. Cooper.—I have had no experience with kerosene as a boiler purger, but found, during many years of application to boilers in Philadelphia, that a petroleum compound known as Allen's Anti-Lamina, worked well to prevent and remove scale. Latterly I have known and tried the Eucalyptus extract for cleaning boilers and keeping them so. I met some wonderful statements about it while sojourning on the Pacific coast, and sought to prove them.

It was then premature to say that this new boiler purger had become a universal cure for all the infirmities to which boilers are liable the world over, from the impurities of feed water; but the fact can now be stated certainly, that, so far as tested, in the East as

well as the West, it has performed all that is claimed for it, removing and preventing scale every time and from every water upon which it has been tried.

To engineers the following statement will prove interesting, and the facts of it have been fully realized by myself:

The Eucalyptus scale preventer is not only harmless upon the boiler plates, but it absolutely prevents the rusting of them, and in the case of steamboat boilers it neutralizes galvanic action and preserves them against pitting. It has been used, also, with marked success for removing grease from condensers.

Mr. Louis G. Engel.—I am induced to offer the following, because caustic soda and other materials which may possibly increase the gravity of the water and cause foaming, have been suggested as substitutes for kerosene, and by the clause near the end of Mr. Lyne's paper, "Our boilers do not lift their water," as tending to that condition.

In a laboratory experiment of about twelve hours' duration in two similar glass beakers with similar Bunsen burners under them, there was a marked difference observed in the manner of boiling between the beaker containing distilled water and a quantity of scale, and the beaker containing distilled water, a small proportion of kerosene, and a similar quantity of scale. In the beaker with the kerosene the boiling was much more rapid, either due to a loss in gravity of the total liquid, or to the lubrication of the particles of water by the oil, as the other conditions were apparently the same. And further, when the oil had been boiled out of the water by long evaporation, till the water was practically odorless, the two beakers boiled in a similar way, but a difference was immediately observed on adding oil to one beaker. The experiment was not strictly quantitative, and I would not like to state that more water can be evaporated by adding oil to it and boiling than by boiling alone; but it seemed so in this imperfect experiment.

I am inclined to favor the lubrication theory, as the gravity was probably not sensibly affected by the mechanical mixture of oil and water; and as it is certainly possible to diminish the friction between the wind and the surface of the water by a film of oil, it may be possible in a similar way to diminish the friction between the particles of water themselves.

I trust that some one may have tried the same experiment and arrived at the same conclusion.

Prof. J. E. Denton.—Mr. Lyne's paper bears on a very impor-

tant subject. The manner in which he has handled it shows his skill in taking care of his establishment, and the knowledge of that fact and others makes me think that kerosene would not always do as well elsewhere. I have known kerosene to be injected into boilers as the paper describes and give no satisfaction, first, by not removing the scale and secondly, by neutralizing the effect of the lubricant of the engine cylinder. If the kerosene returns to the liquid form—I won't attempt to trace how it could—if it returns to the liquid form after reaching the steam cylinder and mixes with the cylinder lubricant, a very slight quantity of kerosene will very largely reduce the viscosity or body of the lubricant. In some steamships the injection of kerosene has been abandoned because of this deterioration of the lubricating effect of the cylinder lubricant, so much of the kerosene going out of the boilers with the steam that no practical good was derived from the oil in loosening scale. The latter was always loosened, however, by the direct application of kerosene with a swab.

These facts make me think that kerosene may be quite universal in its effect if it is made to reach the scale, but that boilers differ in the matter of allowing the kerosene opportunity thoroughly to attack the scale.

In regard to the statement that petroleum sinks to the bottom of the water and bakes upon the surface of the boiler, reducing the conductivity and therefore causing burning, it is quite a common practice, I believe, in steamships to pump in petroleum on vertical surfaces and let it drip down over surfaces showing corrosion; and large quantities put in for such purpose have not given any such trouble as the paper describes. I know of other cases like the one the paper describes in which the petroleum has apparently baked on the surface, and the opinion is prevalent that a slight quantity of petroleum thus deposited will largely increase the lack of conductivity of the scale, but the best data regarding such cases indicate that the animal ingredient mixed with petroleum in steam cylinder lubricants is more responsible for these deposits than the petroleum itself.

I recently had cause to think that the true way to arrive at what to put into the boiler is to analyze the scale, and not the water. A very scientifically prepared compound based on an analysis of the water, I knew to fail; but a very remarkable instance has lately come to my notice in which, everything else failing, a compound adjusted to the scale was successful.

Mr. Geo. H. Babcock.—Before Mr. Lyne closes the discussion I will say that something over twenty years ago, I think, I knew of petroleum being used in Providence, where the scale is very bad, by first emptying the boiler, then putting in a quantity of kerosene, and pumping water into the boiler. The kerosene floated on top of the water and touched every part of the scale. That was a Harrison boiler. It was the only way they could get at the scale, and it proved to be quite efficient in that case.

Mr. Schuhmann.—May I ask what that scale consisted of?

The President.—Mainly sulphate of lime.

Prof. Denton.—How long did it take to do that? How slowly was the water made to rise?

The President.—Pumped up at the ordinary speed of pumping up a boiler. The boiler was in the Providence Tool Company's works.

Mr. Lyne.—I desire to say that I have some scale taken from the boilers upon which we made these experiments. That scale had been soaking in kerosene oil for over a year, and there is no perceptible action in softening. It seems to be only by the boiling process that it is disintegrated and loosened, and prevented from forming in the boiler. I am also aware that it is not a new thing, and that almost every conceivable substance has been put into boilers for the prevention of scale. I can recall one instance in New York where a friend of mine thought he would try some potatoes, and inside of a week he could get potato skins out of almost all the pipes. Wherever he opened a valve he found potato skins.

In regard to the use of crude petroleum, I have not received a reasonable answer when the question has been asked as to why it should be used. It has a heavy body, and I think it objectionable in a boiler. With kerosene there is not sufficient body to cause any harm. A common laborer attends to our boilers in that particular. He simply puts the kerosene into the receiver. It does not require any special skill.

I have been asked to explain what I said about the safety valves with a cast-iron bonnet. Perhaps you may not all be aware that there is sold in the market a cheap form of safety valve with a cast-iron stem fitting through a cast-iron bonnet. Now I have known such safety valves to cause boiler explosions. By putting a little oil and graphite onto that stem, say once in two or three months, it will not stick. Then with reference to the breakage of water glasses. With our boilers, before we commenced to use kerosene

oil, there was a very marked corrosive action above the water-line, and the flanges of the safety valve were wasting away. The water glasses would become grooved at the top, and we could not keep one in more than two months at a time. Since we commenced using kerosene oil there is no evidence of corrosive action, and these water glasses are now just as good as ever. The last one we put in has been in over a year. They do not leak, and we see no grooving or chemical action, as was evidenced before. Of course I am aware that it is impossible to make a universal medicine for all diseases, and, in my opinion, the scale should be analyzed before any chemical compound is put inside a boiler, to be sure that it will not attack the iron. I have known boilers to be injured by putting some compounds into them, and where they were, so to speak, poison to the boiler, causing corrosive action, etc.

I want to say further, that, in my opinion, it is not right to set a shell boiler in a horizontal position, because I have known cases where crude petroleum and kerosene oil have been used and the scale was precipitated on the fire surfaces, causing the plates to bag. The boiler should be inclined, to give the scale a chance to slide away from the hot surfaces.

Mr. L. G. Engel.—I rise to defend the potatoes. I think that potatoes are in common use in some parts of the country, and I know of at least one case in which they were used with good effect. If the gentleman will kindly tell us what we are to use to dissolve a silicious scale out of a boiler, there would not be any occasion to use potatoes. I believe potatoes act very much as the white of an egg does with coffee. I cannot see the necessity, either, for putting potato skins into a boiler. The essence of the potato is all that is necessary. The potatoes mashed up have a sufficient effect without the skin.

CCLXXVI.

THE MILLING MACHINE AS A SUBSTITUTE FOR THE PLANER IN MACHINE CONSTRUCTION.

BY JOHN J. GRANT, PHILADELPHIA, PA.

(Member of the Society.)

FINISHING surfaces by rotary cutters, or, in machine shop vernacular, milling, has not received the attention from mechanics and proprietors of machine and tool factories which it deserves. There appears now, however, an awakening among engineers to the importance of using the milling machine as a substitute for the planer, and it is safe to prophesy that the places of the two machines will be reversed, and the planer become the jobbing tool instead of the milling machine, as now. During the years 1861 to 1865, inclusive, more was done toward the development of the milling machine than in fifty years of previous experimenting. The use of the milling machine has made it possible to produce the parts of guns, pistols, sewing machines and other articles of a similar nature, at a trifling expense as compared with the planer, or the old and tedious method of jig filing. That it has attained a very high state of perfection in the above branches of machine work is well known to all mechanics: that it should have been neglected, and not developed in the field—much wider and of quite as much importance—of general machine building, is no less a mystery.

There is no reason why the milling machine should not be used to finish the various parts of machine tools, locomotive or stationary engines, and other large machinery. It may be asserted that there is no part of a locomotive now finished on the planer which cannot be done on the milling machine, if properly designed for such work, and at a cost of from one-half to one-tenth, besides, in most cases, producing far better work and nearer to interchangeability.

In conversation with master mechanics, superintendents of shops, and others interested in such matters, the reason given for

not using the milling machine has been the cost for cutters; and in return the answer should be that milling cutters are not as expensive as the single pointed planer tool, to produce a given amount of work. This is known from an experiment made by myself, which was as follows, and although not absolutely correct, is sufficiently so to make the relative cost of producing work on the planer and milling machine obviously in favor of the latter.

One hundred pieces of cast-iron 16 inches long, large enough to finish $1\frac{1}{2}$ inches by 1 inch, used for lathe racks, were given to the man in charge of the milling machine, and an equal number of the same pieces to the man in charge of the planers. The cutter used on the milling machine was simply a plain, spiral cutter, of $2\frac{1}{4}$ inches diameter by 2 inches in length, costing to make in the shop, including stock, labor and shop expenses, two dollars and ten cents. This cutter was sharpened but once, and then after the completion of the job, which consisted in roughing the four sides of the 100 pieces. The periphery speed of the cutter was 26 feet per minute and the feed $3\frac{5}{10}$ inches per minute; time consumed, 44 hours, 39 minutes. Two milling machines were used—one or roughing and one for finishing cuts. The cutters required grinding at the end of the job, and so were chargeable to it; the time required to grind them was 22 minutes. The wages of the boy running the machine was 9 cents per hour. The total cost for finishing the 100 pieces on the milling machine was as follows:

44 hours 39 minutes, at $4\frac{1}{2}$ cents each per hour...	$3 99
Sharpening cutters, 22 minutes at 30 cents per hour....................................	11
Shop expenses at 35 per cent. of labor............	1 39
Total cost...............................	$5 49

The cost of the same number of pieces finished on the planer was as follows:

Planers used were two 16 inches square by 3 ft.; platen running 24 feet per minute cutting speed; return 2 to 1; the feed was as coarse as could be used on the roughing cut,—that is, about 22 per inch. Time consumed on each planer, 25 hours, 46 minutes; wages of man running the planer was 25 cents per hour. The total cost for finishing the 100 pieces on the planer was as fol-.ows:

24 hours 35 minutes each machine, at 25 cents per hour	$6 03
Grinding and setting tool 19 times, 1 hour, 21 minutes	33
Shop expenses, 35 per cent	2 22
Total cost	$8 58
Balance in favor of milling machine	3 09

This, you will bear in mind, is showing the planer to its best, and the milling machine to its worst advantage. Where the milling machine makes the best showing is in irregular work, which in the planer requires the constant attendance of the workman—such as planing close to shoulders, cutting bevels and T slots, and jobs of a similar nature.

The advantage of the milling machine is then seen, as the cost for attendance is, often, not one-tenth, owing to the fact that a much cheaper workman can run several machines. The cost of tools in each case was as follows:

Milling cutters, first cost, $2.10 each	$4 20
Grinding once, 22 minutes	11
Total cost	$4 31

Planer tools, first cost, 42 cents	$0 84
Re-dressing by smith, once	16
Grinding and setting roughing tools 19 times, 1 hour, 21 minutes	33
Grinding and setting finishing tool 6 times	10
Total cost	$1 43

Expense of keeping tools in order on milling machine	$0 11
And for planer	59
Balance in favor of milling machine	48

This does not include the time of workman's loafing in the smith-shop, this trouble being entirely done away with in the milling cutter. The milling tools after this trial showed no perceptible wear, or difference in size, and it is safe to say that they

would finish fifty such lots of 100 pieces. The cost for tools in that case would be about as follows:

Milling machines, first cost	$4 20
Grinding 50 times at 11 cents	5 50
Total cost	$9 70

Planer tools, first cost	$0 84
Re-dressing roughing tools 50 times at 16 cents	8 00
Grinding and setting 950 times at 1 73-100 cents	16 43
Finishing tool 10 times at 16 cents	1 60
Grinding 300 times at 1 73-100	5 19
Total cost	$32 06

Balance in favor of milling tools	$22 36

This calculation on the life of milling cutters is based on what has actually been done in the American Sewing Machine Company's works, where I now have charge. Since February 15th of this year, up to the present writing—Sept. 14th—there has been finished by one set of cutters about 15,000 sewing machine beds, the cut being about 6 inches long. These cutters have been ground but five times and show but a very little reduction in size. Common sense teaches that a continuous rotary cutting motion will produce more work than a reciprocating or intermittent one. If inventors would turn their attention to the further development of the milling machine, for general machine work, instead of the quick return of planers, they would benefit themselves and be doing the mechanical world better service. Some of our large machine and tool builders are using the milling machine to their great advantage. The following may be mentioned who, to my knowledge, are using regular and specially designed milling machines to their profit. Brown & Sharp Manufacturing Co., The Pratt & Whitney Co., The Baldwin Locomotive Works, The Straight Line Engine Works, and others.

In conversation with one of the members of the Society, he mentioned a milling machine shown to him in Scotland, having a face mill or cutter 8 feet in diameter. This machinewas used to face off the ends of beams, sheets, and such work.

In the construction of machine tools it is possible to mill every part of an engine lathe, a great part with common straight cutters

and the balance with special ones. Modern cutter grinding machines, with their attachments, have made it possible to keep cutters sharp and in perfect shape, so that the old and expensive methods of grinding by hand, or annealing and working over, need not be resorted to, and modern ideas of the sizes of cutters necessary to do work make their first cost much less. A cutter of 2 inches to 2 1-2 inches is now used where formerly it was thought necessary to use one of from 4 to 6 inches diameter.

The field of regular and special milling machines for small work being pretty well worked over, what is wanted is more special machines and fixtures, built for and adapted to the finishing of parts of such machinery and tools as are now finished on the planer.

In the shop at Flushing, Long Island, with which I was formerly connected, milling machines and special milling fixtures

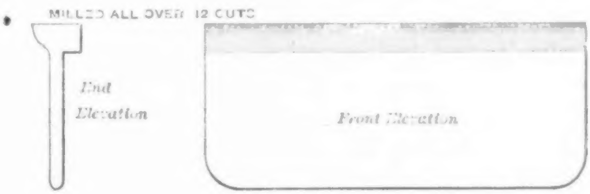


FIG. 57.

were adapted for nearly every part of the 14 inches swing engine lathe. The only part of this lathe not milled was the bed and a small part of the carriage, and this was under consideration when the partnership was dissolved. As to the cost of milling some of the work, without going into details, we will take the matter of the feed apron, on which there were taken 12 cuts, it being finished all over, being shaped as in Fig. 57. The entire cost of finishing this apron, 16 inches in length and 7 inches wide, was but 7 cents, with the extra advantage of each and every piece being an exact duplicate. Should there be any tool builders present, they can tell the cost of planing the apron shown.

A man employed in the shop offered to finish 6 ft. 14 inch lathe beds for 60 cents each if I would build for him a milling machine such as had been sketched out. We were at that time paying $3 each for planing them.

I have been informed that the Pratt & Whitney Co. are considering the feasibility of constructing a milling machine for this

purpose: should they do so, it would be safe to guarantee its practicability. The Putnam Machine Co., of Fitchburg, Mass., have, acting upon my advice, made a milling fixture for an 84-inch square planer, for facing up the ends of planer and lathe beds when it is necessary to join them together. This arrangement works perfectly, the ends being milled off before the bed is removed from the planer platen. When bolted together the two pieces are as near a true plane as it is possible to make such work, while the time consumed was less than it would take to remove the bed and place it crosswise on the planer platen.

In several tool-building shops, fixtures are used for finishing the ends of lathe beds, they being bolted to the bed and run by a roundabout belt and having automatic feed. The cost for this work is but little more than the cost of attaching the fixture to the bed and removing it after the work is done.

There is hardly any limit to the devices for milling fixtures. Large shafts can be splined when in position; key-ways cut in large fly wheels; and in locomotive construction and repairs, an immense amount of money could be saved in this way.

A large special milling machine has just been finished for use in the Straight Line Engine shop at Syracuse, N. Y. In a communication received from the President of the Company a short time since, he mentioned the fact that the machine was just completed and he was at that time engaged in milling out the T-slots for bolt ways in the platen, which was being accomplished at the rate of 2 inches per minute. A few words from him in relation to its merits as compared with the planer would, no doubt, be very interesting, and also the same from the President of the Ferracute Machine Co., of Bridgeton, N. J., on an immense milling machine which they are constructing for their own use.

The advantages of the milling machine over the planer are many, among which are the following: Exact duplication of work; rapidity of production—the cutting being continuous; cost of production, as several machines can be operated by one workman, and he not a skilled mechanic; and, cost of tools for producing a given amount of work.

It is not possible in a short paper to show more than a few of the advantages, but if an interest is awakened in the possibilities of the milling machine, in cheapening the cost of machine work of all descriptions, the object of this paper will have been accomplished.

DISCUSSION.

Mr. H. R. Towne.—Mr. President, I would like to draw out further information on this point, which is a very interesting one. I understand that the author's advocacy of the milling machine includes that tool, whether used with inserted cutters or solid cutters. Where the solid cutter is used there are two forms of tooth which are in use: one admitting of grinding and sharpening without altering the form of the cutter materially, and the other not; the former having but a few teeth, well supported, and admitting of considerable redressing, the latter having many teeth at much closer intervals, and not admitting of dressing. I would like to ask Mr. Grant whether his experience covers the use of both forms of cutters, and, if so, which it would indicate to be the better in the long run?

Mr. Chas. Potter, Jr.—I have had some experience in the use of large milling machines during the last three years.

I think we have *one* of the largest, if not *the* largest milling machine in this country. It is an invention of our own, constructed for our especial use (printing presses), and will take work seven feet wide and sixteen feet long. It has seven cutter heads, five using horizontal or upright spindles with disks and inserted cutters, and two using solid cutters with spiral or straight cut. The durability of the cutters has far exceeded our most sanguine expectations. For our use we deem it fully equal to four of the best kind of planers of similar size.

Prof. J. B. Webb.—I should like to ask Mr. Grant whether, in milling machines of that class, it is customary to have a grinding machine attached to the milling machine itself, so that the cutters may be ground without taking them out of the machine, or, whether they are ground separately and reset by the gauge?

Prof. J. E. Denton.—The paper promised that we should hear from Mr. Smith and Prof. Sweet about some new machines. I should be glad to have that promise realized.

Prof. John E. Sweet.—I would like the indulgence of the society for a few moments while I describe the machine to which Mr. Grant has called attention, and I do this believing you will be interested in it. It is a novel machine in so many respects, that when people come into our works and see it, one man says, "Well, you have got a new planer." The next one, "That is rather a novel milling machine." Another may come in and see us doing a

different class of work, and say, "That is a pretty good boring machine." Another will say, "You have a novel radial drill;" and another, "That is a regular profiling machine." It is, in fact, a planing machine, with a vertical spindle in the place of an ordinary planing tool. To describe the machine: the bed is cast with the side plates, but not with cross-girths, as a planer is made, but with a solid top and bottom, so that it is a square box with a bottom, top, and sides. The object of putting the iron in that way is to prevent torsion. The bed, instead of being twenty-four inches deep, with a four-inch table, is twenty inches deep, with an eight-inch table. The table is cast in a box form with a complete top, a complete bottom, and three vertical webs. We use cutters as large as eighteen inches in diameter, and as small as half an inch, and the man who runs the machine says he can manage a 3-16 drill easily. The table weighs two and a quarter tons, and the man, with a crank ten inches in length, runs the bed with perfect ease, and it runs an inch and a half at each revolution. The cross-head weighs a ton, and is counterweighted so nicely that the man can run it up and down with the ten-inch crank comfortably. The cross-head is guided on one post by a guide four and a half feet in length, and simply rests against the other post to keep it square with the bed of the machine. The counterweights are suspended by band-saws (without any teeth) about one and one-half inches in width. The posts are like the planer posts except that they are cast hollow for the counterweights inside. The counterweight bands run over sheaves two feet in diameter, ground to exactly the same diameter, fastened together with a rigid shaft, and the journals are simply rolling journals running upon flat surfaces. They only have to move about an inch and a half. The machine feeds the bed in both directions. It also feeds up and down by power as well as by hand. We use it for boring, drilling, milling, and planing. It was made particularly for our engine work. The cylinder of our engines being cast on the frame, gives us a great deal of small work to be done on the heavy castings, which would have to be moved in the usual shop tools. The idea of this machine was that we could move the castings slow and the cutter fast, save power, and do the work much faster. We do everything in about one-half time that it would take on a planer, but the engine work is especially fitted for it. We succeed in making a steam-tight joint between the steam chest and the cover. We are able to cut slots, as Mr. Grant says, two inches to the minute. We have milled off surfaces at a speed of eight inches

a minute, and the maximum, when the worm gearing is used, is at the rate of $\frac{7}{16}$ to a revolution, but less when the spur gear is in use.

Mr. W. F. Durfee.—Quite a number of years ago (1848) there was a milling machine built in England by Messrs. E. B. Wilson & Co., of the Railway Foundry, Leeds, from the designs of Mr. Robert Willis, the foreman of the works. This machine was intended for milling out and finishing complete, at one operation, solid forged crank shafts of the English locomotive, and the general ideas involved are illustrated by the accompanying sketch (Fig. 75):*

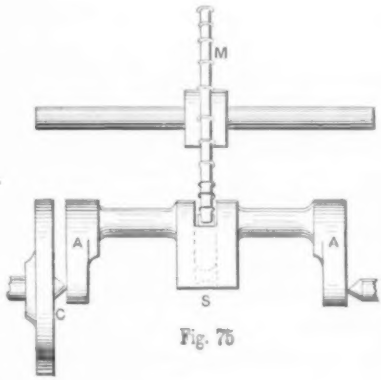


Fig. 75

The axle was supported by centers C, C, by means of temporary crank arms, A, A, the crank slab, S, lying horizontal, directly opposite the mill, M, which is made with inserted teeth or cutters, and has a diameter of fifty-one inches, and a circumferential speed of twenty-eight feet per minute. After all the adjustments are made, the crank, with its supports, is moved toward the mill M, until enough of the crank slot is cut out; the horizontal movement of the crank is then stopped, and it is given a motion of rotation about its supporting centers (the mill M still continuing to revolve), which, of course, must be located on the prolongation of the axis of the crank pin, as finished. The movements described result in the cutting out and completion of one crank block of twelve inches throw in 9½ hours.

Mr. John T. Hawkins.—I am inclined to believe that the generalization of Mr. Grant, to the effect that the milling machine should or will occupy the position now held by the planer, or that their present fields will ultimately be reversed, must, to a considerable extent, be modified, and particularly in performing some grades of work; that is to say, in the cases of large castings of light dimensions, requiring to have not only some of their surfaces dressed into shape, but also be straight and true when finished. In many such cases it is not practicable to use milling cutters, whose effort is exerted in a line parallel to, or whose axis is at a

* This machine is fully illustrated by a scale engraving facing page 97, and is described on page 104 of Vol. I., *Practical Mechanic's Journal.*

right angle to, the surfaces to be dressed; as, for example, in milling out a wide-bottomed groove with more or less vertical sides. In all such cutters there must be a very considerable downward pressure, or a pressure at a right angle to the main surface. In many such pieces of work as indicated it is not easy to support them at a sufficient number of points to insure their being straight when finished, under pressures coming from such a cutter, even if more than one cutter be used so that a very light cut may be taken to finish it, and the work be reset for the finishing cut, as must generally be done on the planer. In the latter case, too, the expense or first cost of cutters will be doubled. A planer tool, operating upon a comparatively narrow strip of surface at one time, if properly formed, exerts little or no pressure downward or at right angles to the surface planed, and therefore will not cause such a casting to spring away from it, as a milling cutter would. It would be difficult, in such a discussion, to enumerate the various varieties of work to which these considerations would apply; but, having daily to deal with precisely this class of work, and making more or less a study of the question as to how it may be done cheapest, particularly as between a planer and a mill, I am persuaded that the milling machine can never supplant the planer to the extent indicated by Mr. Grant. I admit that the field of the mill may be still very largely extended, as it has been in the past few years; but that the milling machine can ever become the jobbing machine to the exclusion of the planer now occupying that place, I do not believe. I would like to hear from Mr. Grant, however, when he replies, as to whether any experiments have been made to show the comparative pressure of wide-faced mills at a right angle to the surface to be dressed when their axes are parallel thereto.

Mr. John J. Grant.—I would say in answer to Mr. Hawkins, that the class of work he mentions as not being able to do is one of the very few kinds which can be accomplished on the planer better than on the milling machine.

As to the amount of downward pressure by a cutter, I have not made any tests and do not know of any having been made, to determine it. I should think that such work ought to be done in institutions such as the Stevens Institute School of Technology and others. Since the paper on the milling machine was written, I have received the following from the President of the Putnam Machine Co., of Fitchburg, Mass.: "The rack cutter is doing

splendidly, and you would be surprised to know the amount of planers which it is saving; as you will remember we formerly cut all our racks with planers."

The rack cutter referred to was designed by the writer for cutting racks up to 2 pitch (diametral), which it does at a single cut with milling cutters, at a cost of one-quarter that at which it is usually done on the planer, and far more perfectly.

Replying to Mr. Towne in regard to what is called the involute cutter or common spiral cutter made on a milling machine, I would say that for anything in an irregular shape I would use that form of cutter; but for any other purpose, I would use the common form of cutter.

For very wide, thin surfaces, I would use a face mill or rotary planer, that is, a number of inserted cutters, around the face of the disk, the work either set up against an angle iron, or turned the other way, as Professor Sweet mentioned, on his machine.

Replying to Professor Webb, I would say that on machines using a straight cutter I would put a fixture for grinding on the bed of the machines, so that each tooth should do its own amount of cutting; but where the cutters are irregular in form this cannot be done, and it is necessary to grind with a special fixture.

CCLXXVII.

A NEW METHOD OF STOCKING AND RELOADING COAL.

BY JAMES M. DODGE, PHILADELPHIA, PA.
(Member of the Society.)

THE conveyors employed for this work are of two varieties: The first, called the flying-extension, consists of an endless chain, to which are attached flights or scrapers, forming a chain conveyor, at the lower end of which is a sprocket wheel situated under the railroad track, and the other end passing around a traction wheel secured at the upper end of a pole, which is held in an upright position by suitable guys. (Figs. 58 and 59.) The conveyors of this kind which are now in use are about one hundred and fifty feet long, with the upper end located from fifty to seventy-five feet above the ground. The scrapers are $8'' \times 20''$ in size, and are placed at intervals of two feet on the conveyor chain. There is no support for the lower strand of chain between the foot and head wheels. The upper strand is supported on idler wheels at intervals of fifty feet. These wheels are either suspended on wire cables or supported by light trestle-work, if convenient. The province of the flying-extension is to take coal from the dump situated above the lower end and convey it toward its head wheel, thus forming a pile of coal the general shape of which is conical, with the apex under the lower strand of chain, and if the conveyor is fed until it has conveyed coal to its upper end, the apex will be directly under the head wheel, forming a pile, say sixty-five feet high and three hundred feet across its base, and containing about twenty thousand tons. The pile of coal so formed is in the best possible condition to be reloaded, as there is no trestle work or other timber obstruction, excepting the pole, in it. In the event of it being advisable to use the same apparatus at another place after its having built one pile, the only portion remaining in the pile would be the pole, the value of which would be about $15. The apparatus described has a capacity of about two tons per minute, and this could be increased almost indefinitely, if required. It is difficult to make a comparison between the cost of stocking coal by this method and

the ordinary plan of using hand labor after the space under the trestle has been filled up, because it is practically impossible to make such immense piles of coal by hand. The average cost, how-

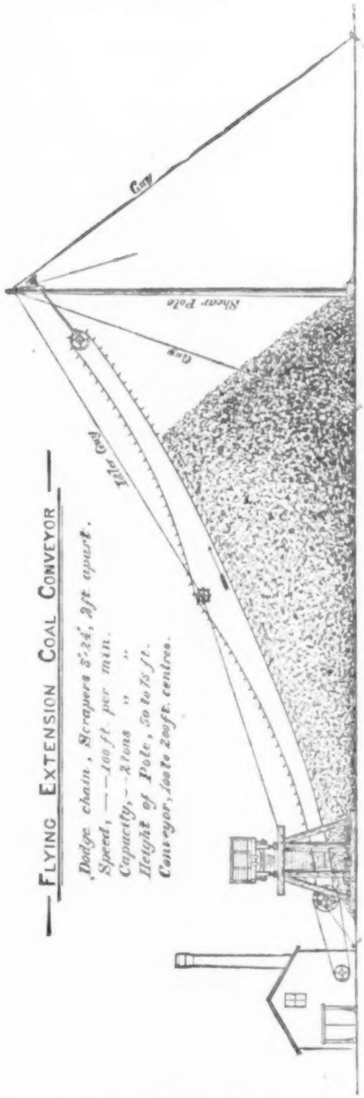


FIG. 58.

ever, of stocking coal on either side of a trestle to a distance of say twenty feet is about thirty cents a ton, whereas the cost for stocking coal with the flying-extension is but a fraction of this

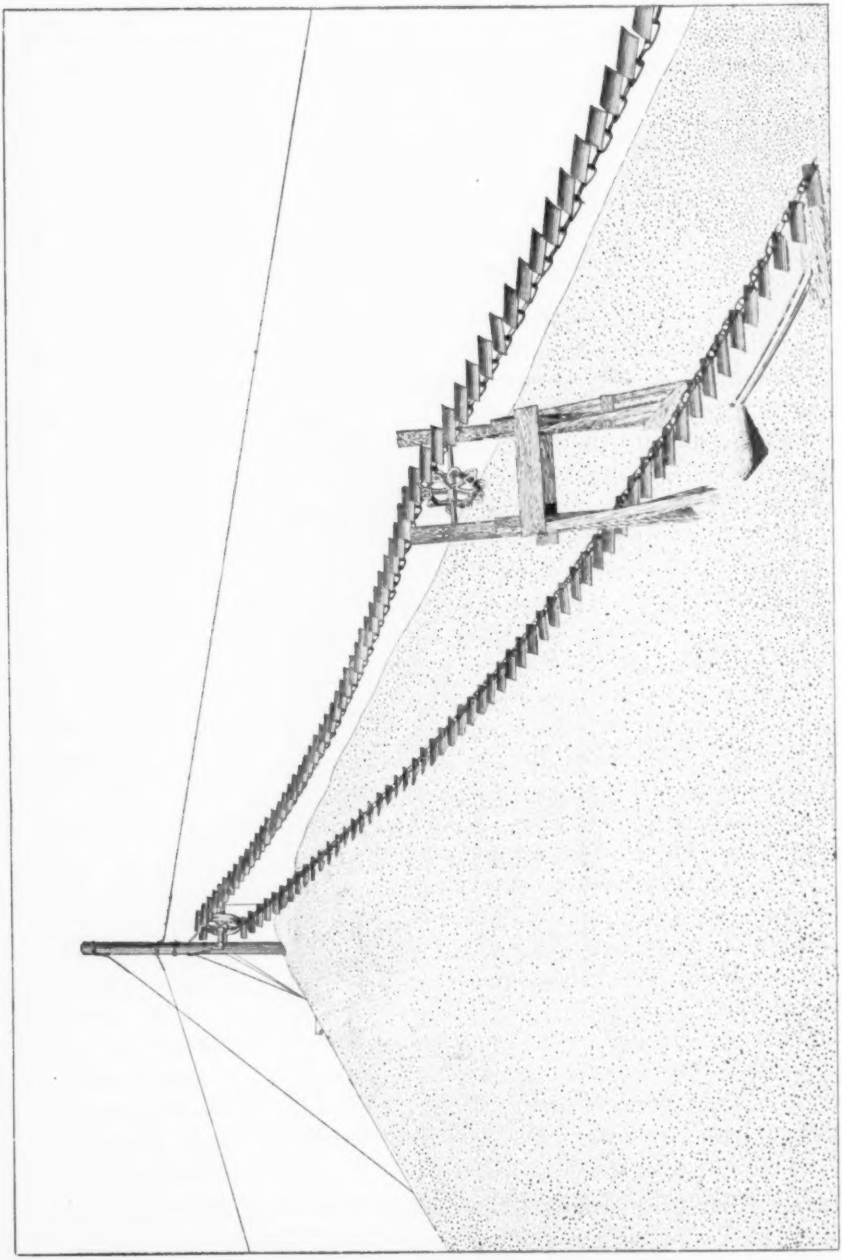


FIG. 59.

amount. There are four of these conveyors now in use at the wharves of the Philadelphia and Reading Railroad Company at Port Richmond, Philadelphia, and others in process of erection.

For reloading the coal into cars after it has been stocked, a conveyor is used which is so constructed that it may be moved sideways toward the base of the pile, and kept running continuously while it automatically attacks and conveys the coal toward the trestle from which it was originally dumped, at which point it discharges the coal into an inclined conveyor which elevates it to a loading-pocket, from which it is tapped into cars. The reloading conveyor is so constructed that it can be swung to the right or left, and is capable of operating on either side; consequently, by locating it between two flying-extensions it would be able to reload the coal stocked by either of them. By means of the flying-extension and reloading conveyors it is possible to store immense quantities of coal on vacant land at some distance from the sea-coast, and cheaply reload it and deliver it at tide-water as called for, instead of storing coal under expensive trestle work and upon valuable dock property.

DISCUSSION.

Mr. W. F. Mattes.—Mr. Dodge's illustration shows the coal stored in a conical pile. He plans a conveyor, for reconveying that coal from the pile to the railroad, and I would like to ask him how he proposes to gather up the coal on the exterior of these piles.

Mr. Dodge.—This reloading conveyor swings around the vertical line of the mast, removing all the coal excepting that in a relatively small crescent. If the pile contained 25,000 tons, this little crescent would contain about 1,000 tons. It would not pay us to have a reloading conveyor long enough to get that, because laborers can trim this small amount into the conveyor.

Mr. E. F. C. Davis.—I would like to ask what the effect is found to be on the coal itself—whether the coal is not more or less destroyed and rendered somewhat unmarketable by the friction of the coal particles on themselves, and also how large a size of coal this is adapted to—whether they can use it for as large as egg, or whether it is limited to stove. I would also like to ask about what is the steepest angle on which Mr. Dodge can make them work.

18

Mr. Dodge.—I would answer the first question about the breakage of coal by stating that the Philadelphia and Reading Road made us contract to pay for any breakage of coal in excess of their average, and we received a report, after handling 31,000 tons, in which they complimented us and said they had less breakage than formerly with hand labor. We have dumped a car of steamboat coal into a conveyor and carried it up on the pile without any trouble, although if we were going to handle very large lumps we would use heavier scrapers and heavier chains than we are using. Up to furnace coal or broken coal the devices we have now are heavy enough. If we lift the coal 60 feet it will run out about 120. We could run the conveyor up at a much steeper incline than that, but there would be no advantage in it. The size of the pile is governed, of course, by the amount of property which we have to cover. In this case the head wheel being 80 feet above the ground, the base of the pile would be about 320 feet across.

Mr. Davis.—I wanted to get at the steepest angle of the chain to carry the coal up.

Mr. Dodge.—I could not tell the angle except this way, that we are running, for instance, 40 feet high and only 8 feet out to the perpendicular, although to do that we have to have very large scrapers and make the trough high at the sides. If we wanted to elevate perpendicularly, we would use a bucket elevator.

Mr. Davis.—I understood that the chain simply dragged the coal over itself.

Mr. Dodge.—That is true, but I say if we want to run up more vertically than the natural inclination of the coal, we would have to use the trough.

Mr. Davis.—Don't you find the capacity is very much decreased as you increase the pitch?

Mr. Dodge.—Decidedly.

Mr. Davis.—That is one thing I wanted to get at, as to what pitch you can make it practicable to handle coal with a system without a trough. What I was interested in was, how far you could go without the necessity of using troughs.

Mr. Dodge.—We have formed one pile 50 feet high at a distance of 100 feet from the trestle. Although we use no regular trough, we put down every 10 or 15 feet old pieces of boiler shell with the round side up, so that it would ease the scrapers a little. We did not notice that there was any special difference in

power required whether we use a trough or not, as we did not have an indicator on the engine.

Mr. Davis.—We stock a great deal of coal at the other end of the road at the mines, but we have found it necessary to use troughs 8 inches, 16 inches, and 24 inches in width. We scrape uphill with them as high as 7 inches to the foot. Of course we do not carry quite so much with a chain that way as we would if it was flatter; but we do it in that way as a matter of convenience to get up the side of the hill. We found one point about it which I thought would give you trouble. We never drive the chain at the lower end if we can avoid it. We always drive the chain from the outer end. We have less friction and much less liability to get foul and break the chain. We carry out power by rope transmission and drive at the outer end, and we do that as far sometimes as 1,600 feet from the engine. We do that without any inconvenience in all kinds of weather, with manilla rope. We found them better practically than the wire ropes. We find it necessary to put the troughs in, and that is one of the largest items of first cost—the wear and tear of them is very inconsiderable and the wear of the coal is very slight indeed. That is the reason why I ask about coal running over itself, because you know how coal is injured for the market by the abrasion of the corners, and I had always supposed that it would render the coal entirely unmarketable. We have to be very particular indeed about handling coal.

Mr. Dodge.—I would say that in our catalogue we make the same statement; a conveyor 400 feet long, driven from the head, will do perfect work. The object of my system is to get immense piles of coal without any obstructions in them, thus enabling the reloading conveyors to operate.

Mr. Davis.—We do not use trestle-work. We rest the trough on the pile itself. It is self-supporting. We simply extend out two 40-foot sticks of timber, and by these two long sticks of timber and the troughs themselves combined we can extend that out some 25 feet or so and run out coal, and it tumbles over the end and makes a support itself, and then we make another extension.

The President.—I would say in connection with this that Mr. Dodge has laid here on the platform some specimens of the chain, and I want to call attention particularly to the very nice little "dodge" for giving more surface of wear to a chain.

CCLXXVIII.

A RAILROAD BED FOR BRIDGE STRUCTURES.

BY O. C. WOOLSON, NEWARK, N. J.
(Member of the Society.)

INCIDENT to the building of the elevated railroads in New York City, in 1878 and 1879, the author of this paper, who was then employed as an inspecting engineer by the New York Elevated Railroad Company, began the study of the different plans of superstructure proposed, including the different designs of road-beds to be placed thereon.

Of the many plans submitted, there were some embodying certain requirements of the Rapid Transit Commission, while omitting others, but none which seemed to fill every requirement as specified by that commission. Section 57 of the specifications of this commission reads:

"It is the intention and spirit of these specifications to provide in *every respect* for a first-class structure, and no omission of specific requirements to this effect, if any exist, shall in any case be construed in any way to invalidate this general requirement."

This section and others led the author to the conclusion that time and brains were not to be spared in providing such regulations for the building of the elevated railroads as would prove a monument of skill and a testimony of prosperity which all should be proud of.

It is not necessary to go into the many details of construction which taxed the skill of the engineer in meeting the requirements of the Rapid Transit Commission, but several matters will be discussed in reference to the road-bed of the structure.

Fig. 60 represents a cross section of the single-track road-bed then adopted by the New York Elevated Railroad, the ties being $4'' \times 6''$ and spaced $12''$ in the clear. The outer guard rail was $6'' \times 11''$, and the inner rail $6'' \times 9''$. These rails were fastened down by $\frac{3}{4}''$ through bolts driven from the top, the heads and washers being let into the timber a little below the surface, over which was rammed cement to make the hole water-tight.

The inner bolt was intended to take a clip on the under side to fasten the tie to the upper chord, thus serving a double purpose, while the bolt through the outer guard rail came through the tie only to take a nut and washer.

FIG. 60.

These guard-rail bolts, being placed alternately in and out, gave opportunity to alternate with them ⅝" lag screws both inside and outside the girder, and a clip to fasten down the tie only, and in many instances where it was inconvenient to place a clip, the chord was drilled to take the lag screw.

In laying a section of this road-bed it must be assumed that it is all contract work, and the business is to be put through with dispatch. The ties are first distributed carelessly along the top of the girders ready for the first gang of men, who proceed to space off and mark them for truss fastenings. This has to be done with reference to guard-rail fastenings, which come in later on in the erection, and is a continual source of annoyance and mistakes in calculations, and is not, therefore, straightforward work. Following comes the rail gang, whose leveler finds a varying thickness of ties, and he forthwith proceeds to adze the high ties down till his straight-edge (which by the way is only about twelve or fourteen feet long) shows an approximate level bed just in the line of the rail which his particular gang is to lay. One unfamiliar with this whole construction would be justified in assuming that an approximate level bed is all-sufficient, but later on it will be shown how great an error that assumption is, although this approximate level is all that will be obtained in this method of construction. Remembering that this leveling is contract work, and subject to all the ills incident to such work, and that it is performed at the very lowest

cheap-labor price, the leveler who is adzing for the rail gang only will not, therefore, strike in his adze at a point to accommodate the guard-rail gang which follows him, but will leave them to cut and hack the top of the ties to suit themselves, which means that every tie which has to be leveled receives from four to eight oblique cuts on its upper face, which, in many instances, are more than simply clean cuts, but rather are disintegrating slits, part or wholly across the face of tie.

Assuming now that the rails are spiked down and the guard-rail gang are prepared to fasten down the guards, the importance of an absolutely true level guard-rail bed is manifest. The outer guard-rail bolts go down through the guard and tie and have nut and washer underneath, but do not catch on to the truss, but are simply intended to fasten the guard down onto the cross-tie. We must assume the facts as to our approximate level bed for these guard rails, and that means that a variation of from $\frac{1}{16}''$ to $\frac{1}{4}''$ is to be found everywhere. Therefore, when the guard rests upon two high ties, with an intermediate tie $\frac{1}{4}''$ below, the whole force of this intermediate guard bolt is not to force the guard down upon said tie, but to lift the intermediate tie up off the truss and up against the under face of the guard rail; and that is exactly what it does. I have many times examined the effect of a train of cars passing over such a *suspended* tie, and I am free to say it is anything but flattering to engineering skill.

The tendency to raise all low ties up off the truss is the same, whether there is much or little difference in depth between the low and high contiguous ties. So long as there is *not actual contact* between the upper face of the ties and the lower face of the guard rails, the guard bolts are going to exert themselves to tear the lag screw or tie bolt away from its fastening to the truss, and, in my opinion, the practice is unmechanical and inexcusable.

During the latter part of my connection with the elevated railroad in New York City, I designed a system of road-bed which should fill more completely the requirements of the Rapid Transit Commission and also furnish a road-bed as nearly analogous to a well-ballasted surface road as it was possible to make.

Before proceeding to explain the system adopted, permit me to quote from one authority which encouraged me to proceed in my task. From this authority I received much encouragement by his kindly counsel. He was the first man I ever consulted concerning my theory, and the first man to whom I went with my drawings

and model, and I look back to his pleasant manner and hearty sympathy with my little problem as a sunny spot in my practical experience. This friend was Alexander L. Holley.

In Mr. Holley's standard work, "American and European Railway Practice," he says: "A great variety of materials are used as ballast on English railways. These are broken stone, burned clay, cinder, sand, shells, broken bricks and culm or small coal. The preference turns between broken stone and gravel, etc."

"The ballast has four distinct offices to perform. It must, first of all, distribute the bearing of the track over the surface of the earthwork, * * * and lastly it must, by its character, give a certain *elasticity* to the road. * * * Very hard materials do not meet the last-named condition of an elastic absorbent. * * * On one part of the Manchester and Leeds line, the bottom of a rock cutting was dressed to a surface and the rails spiked directly to it. A few weeks' experience was sufficient to cause the rails to be taken up, to be relaid in the usual manner.

"European engineers attach the greatest importance to the use of ballast, and the comparison of the cost of keeping up roads, both with and without ballast shows how important it is not only that it should be used at all, but in the best manner and of the best quality.

"*Firmness without rigidity is the great requisite.* The best railways in the world—those which do the most business at the least cost—are the best ballasted.

"If we desire to have a *uniform elastic track,* we may rest assured that we shall never derive the required quality from the road-bed, but only from a uniform elastic medium, interposed between a perfectly rigid foundation and the rail. Therefore, perfect rigidity of foundation—or 'superstructure'—is not only unobjectionable, but it is a positive condition of a smooth and permanent way."

These quotations are directly to the point at issue. My problem was to design a "uniform elastic track," which should be "firm without rigidity," and I willingly leave it to the judgment of this practical body whether I have not succeeded.

Fig. 61 represents a cross-section of a single-track road-bed, including longitudinal girders and top of column. It will be noticed that instead of the tie being made of one stick it is made of two, one stick directly over the other and separated from it about one inch, and having no contact except at the middle and the ex-

treme ends; at these points a hard-wood block, cut from the same material as the tie, is put in and a bolt runs through the end ones, while the middle one has simply a dowel which binds the two sticks together and forms a compound tie. This bolting and doweling is all done before the ties are delivered to the superstructure.

The members of this flexible tie were originally four by six inches, but the practice now approves four by eight inches or five by seven inches, while the old style of tie, Fig. 60, is now six by

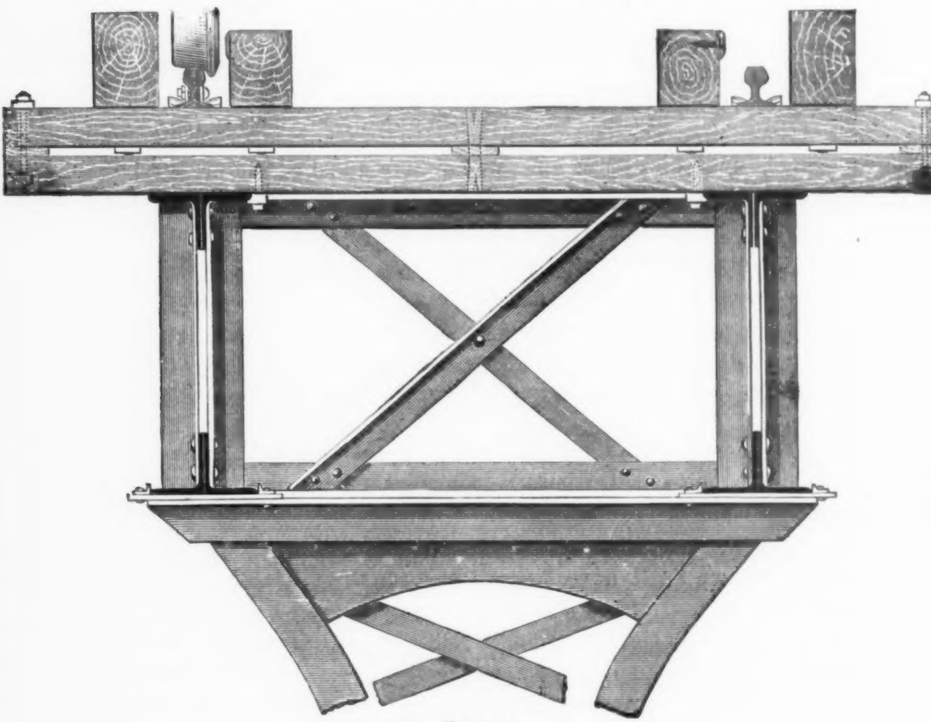


FIG. 61.

six inches and six by seven inches. The spacing may be the same as with the old tie, or it can be varied. This has no practical influence within any probable variation.

The guard rails (they act as beams in my system, and will therefore be called beams), or rather guard beams, are the same size timbers as in Fig. 60, but their fastenings are altogether different.

It will be observed that the bolt which goes down through guard beam does not go through the lower member of the tie, but is screwed into a plate nut within the slot or space between the

lower and upper member. The head of this bolt is within a thin cast-iron cup washer driven into the beam while covered with hot asphaltum. No cement is used, thus permitting the bolts to be always accessible.

The tie is fastened to the chord of the truss the same as in Fig. 60, so far as the lag screws and clips are concerned, but it will be readily seen that this fastening to the truss has no relation to the guard beams, as in the case of Fig. 60.

It is my practice to put in four five-eighth-inch bolts for the guard beams, for one of the specific requirements of the guard beam in any case is to knit the whole top of the superstructure together in the most thorough manner possible.

In laying a section of this flexible or compound tie, as with the old tie, it is contract work, and is to proceed right along without delay. First the ties are distributed carelessly along the top of the girders ready for the first gang, who proceed to space off. This gang, however, pay no attention whatever as to how the guard beam is to come on, but simply place their ties equidistant and so that the clips can come on at the bottom. Therefore there is no calculation necessary; the man simply scribes for his clip, turns over and bores the hole, and the tie is ready to be fastened to its place, and is not to come up again. This can be called straightforward work.

Following comes the rail gang, but there is no leveler necessary, and not an adze is to be used, and for this reason: Although there will be found unequal heights of ties before the guard beams are put in place, the moment these beams are bolted to the upper member of the tie it immediately brings the low ties up snug to the under side of the guard beam, and there is no getting away from it. The result is, that it gives as level a rail-bed as the guard beam is straight, which, in the combined result of four guards, is as level as a floor, and this result is obtained without cutting a gash in the sticks anywhere.

One thing yet remains to be done, and that is, one blow must now be given each rail spike to see that every one is home, because some of the ties have been pulled up to the guard since rails were laid.

To proceed now to analyze the working of the flexible road: In the first place, every bolt is under a proper strain and doing its work as designed. There are no ties suspended midway between guard beam and truss. The load of a passing train, instead of be-

ing received upon a solid stick and its weight transmitted directly to the superstructure at that immediate point, is received upon a yielding stick which springs down a little (about one-eighth inch), thus bringing a certain amount of its load to bear upon a contiguous tie, by reason of the flexing of the guard beam to which the first loaded tie is bolted or hung. This flexing continues over a large area of road-bed, thus distributing the load over the same area of superstructure, and it is not necessary to argue the advantages of such distribution among engineers.

Now, what is the effect of this flexible road-bed on the rolling stock? Referring back to the quotation from Holley, we must all agree, not from this authority alone, but from every example that we ever had in this country, that all testify to the necessity for a flexible road-bed.

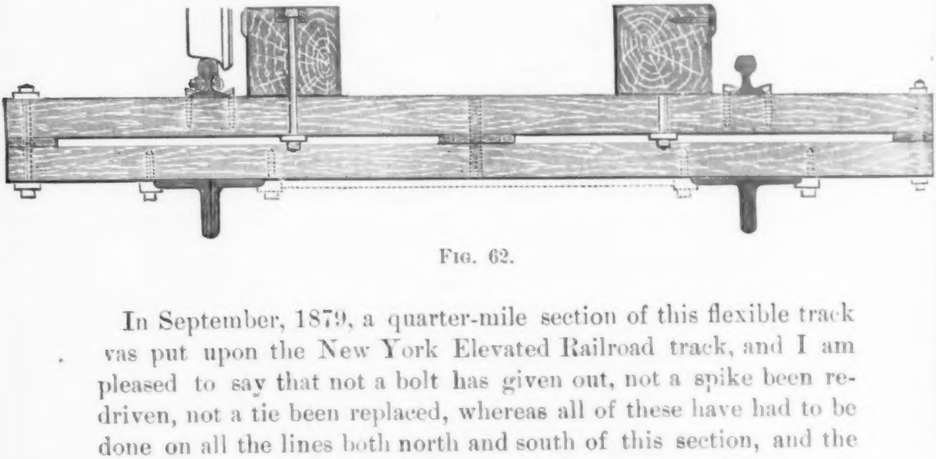


FIG. 62.

In September, 1879, a quarter-mile section of this flexible track vas put upon the New York Elevated Railroad track, and I am pleased to say that not a bolt has given out, not a spike been re-driven, not a tie been replaced, whereas all of these have had to be done on all the lines both north and south of this section, and the rails laid on the flexible ties look as if they were good for twenty years to come. This is a most remarkable showing, but the testimony of the roadmaster is free to all to investigate.

Fig. 62 shows a cross section of flexible road-bed with only one single heavy guard beam on a side, in which construction the guard bolt does not go through the upper member of the tie, but comes down each side of it and through a strap placed in the slot.

Fig. 63 shows an adaptation of this flexible road-bed system to a trunk-line bridge. In this case the timber (used in the elevated railroad system as a guard beam) is much heavier and has no function as a guard timber, but only as a distributor of the passing load. The action, however, of this system is precisely that of Figs.

60, 61 and 62. In this case the bolts which hang the timber to the *upper* member of the tie go between the ties the same as in Fig.

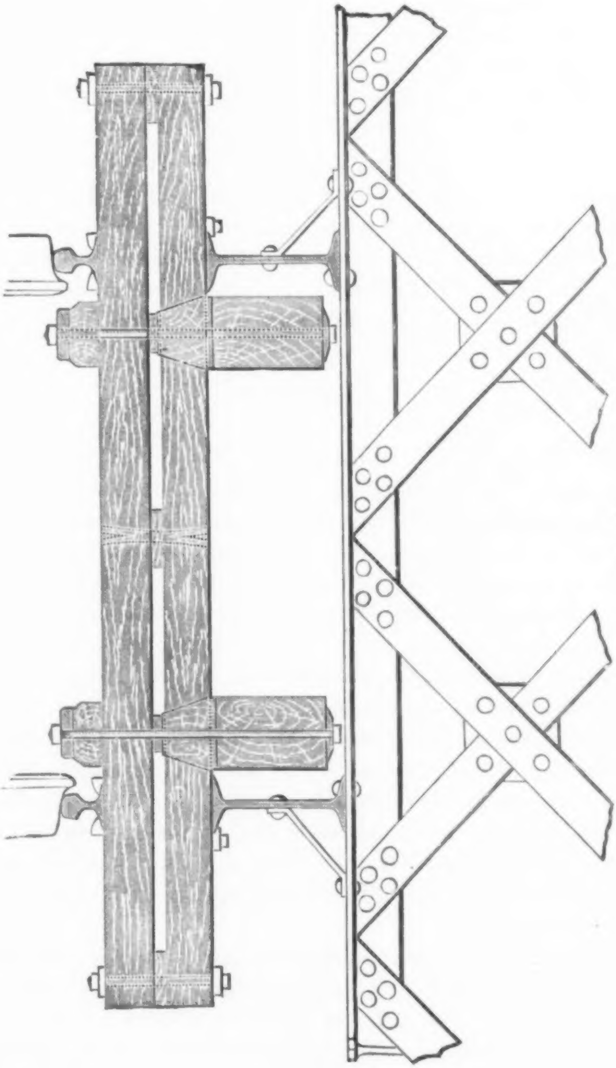


FIG. 63.

62. The thin longitudinal stringer and strap which rests upon the tie just inside the rail performs two functions—that of a strap to hang the lower timber to, and that of a guard rail. The inclined

portion which rests upon the distributing timber is only a block placed there to fill the space between the said timber and a longitudinal iron strap which catches the lower face of the upper member of the tie, and altogether clamps said upper member when the long through bolt is screwed up. A free space is thus left between said distributing timber and the bottom side of the lower member, and it is readily understood that both members of the tie are free to spring vertically, but in no other direction.

In closing I desire to make this statement: That with a flexible road-bed as here designed there would be a very large saving in maintenance of bridge structures, and also a higher rate of speed could be allowed with less injury to both bridge and rolling stock than with any system which I have yet seen, and the fullest criticism is solicited from practical engineers as to the proposed plan.

DISCUSSION.

Prof. J. E. Denton.—I had occasion to ride over the stretch of elevated road which has this tie on it, and to verify the fact that there is a perceptible difference in the smoothness of the running on the road as you stand on the car. The roadmaster called my attention to it, and I found it was easily verified. It seems to me that the principle must be admitted by every one that if a track is slightly irregular, the train running along it is like a projectile which has slight obstructions in its path, which must be depressed in order to allow it to move on, and the elastic medium under the rail allows that yielding, and therefore reduces the wear on the rail and the disintegration of the tie. I understand Mr. Woolson to claim, in observing the result of his track on the elevated road, that the depreciation in the disintegration of his tie is less by a sensible amount. I do not see why such should not be the case and a difference in wearing power be secured by the tie by avoiding shock.

Mr. Woolson.—I have been asked a question or two with regard to the expense of this tie. I do not believe it is any more than the Rapid Transit Commission intended should be put on the structure. The cross ties of a bridge structure of any kind are not simply to put the rails upon, but are manifestly for the purpose of tying or knitting, so to speak, the whole top of the structure together. Therefore I claim that with a tie of this kind, where we absolutely avoid having suspended ties either up against

the guard beams or on the truss, the structure is knit together more securely, and so far as expense is concerned the figures which were made at the time as comparing what I considered the *ne plus ultra* for a road bed as compared with a road bed as it then existed, might mean about 50 per cent. more. But I know this—that in an effort to improve on this, the construction has been made more expensive.

CCLXXIX.

THE INFLUENCE OF SUGAR UPON CEMENT.

BY HARRY DE B. PARSONS, NEW YORK.
(Member of the Society.)

THIS paper contains the results of some experiments made by the author, assisted by Henry Hobart Porter, Jr., E. M., to determine the influence of sugar upon both natural and manufactured cements.

The addition of saccharine matter to cements, for the purpose of obtaining increased strength, is not new; it was first brought to the notice of the author during the past year by articles and letters published in the scientific periodicals. Mr. Guildford Molesworth, the well-known engineer, wrote from Simla (India), August 28, 1886, to Mr. Samuel Crompton, as follows: "With regard to your addition of sugar to mortar, it is a practice that has been in use in the Madras Presidency from time immemorial." It has been the usual custom in that country to add a small quantity of the coarsest sugar—"goor" or "jaghery," as it is called—with the water used for gauging the mortar. There are also in some of the old English records statements where sugar and other substances were added to mortar for the purpose of imparting extra strength. (See *Railroad and Engineering Journal*, January, 1887.)

These tests were made to conform to the system recommended by the Committee of the American Society of Civil Engineers (Transactions, November, 1885). The machine used was a "Riehle Brothers' Standard Cement Tester," the form of the mold and the shape of the jaws being those adopted by the above committee.

The cement was passed through a No. 74 sieve, having 5,476 meshes to the square inch; was thoroughly mixed with a hand-trowel, and placed in the molds without pressing or ramming; the greatest care being taken that the briquettes should be made under like conditions, in order that the tests might be relative one to another. The briquettes were exposed to the air for 24 hours and then placed in water, where they remained till broken by the Riehle Tester. This water was syphoned off every third or fourth

day and replaced with fresh. A constant temperature was maintained between 60 and 70 degrees Fahrenheit.

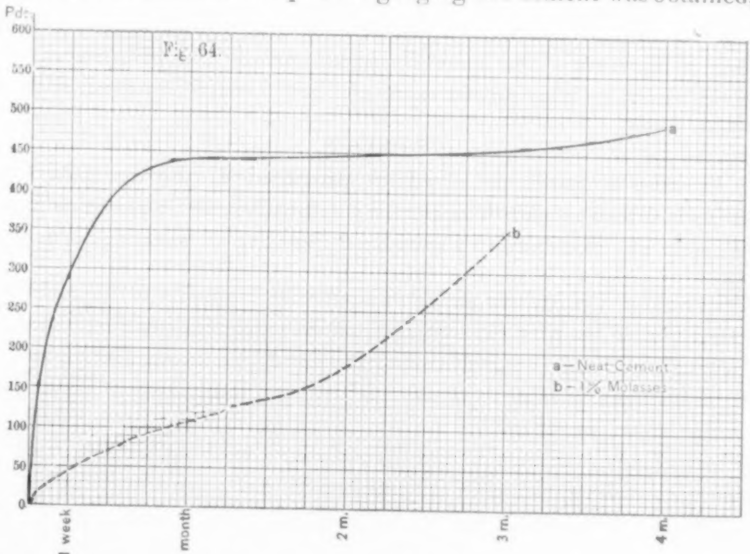
The tests were divided into three classes—Series A, B, and C.

SERIES A.

In these tests the sugar was added in the form of refuse molasses from a sugar refinery. This refuse, as analyzed by W. D. Horne, chemist, contained as follows:

Cane sugar	49.00 %
Carbonate of potash	10.00 %
Water	22.50 %
Vegetable and mineral impurities	18.50 %
	100.00 %

For each briquette a quantity of this molasses was taken to furnish the requisite percentage of sugar; then water was added till the required amount of liquid for gauging the cement was obtained,



namely, 35 per cent. by weight. The cement used was "Dyckerhoff & Soehnes' German Portland." These tests were somewhat imperfect, owing to the fact that the standard solution of molasses was found to alter in chemical composition and could not be kept constant. This action was so great as materially to affect the strength of the briquettes, and thus a large number of tests were discarded as being unreliable. However, the series of tests which contained one per cent. of molasses, gave results as recorded in

the accompanying table, the figures representing the tensile strength in pounds per square inch:

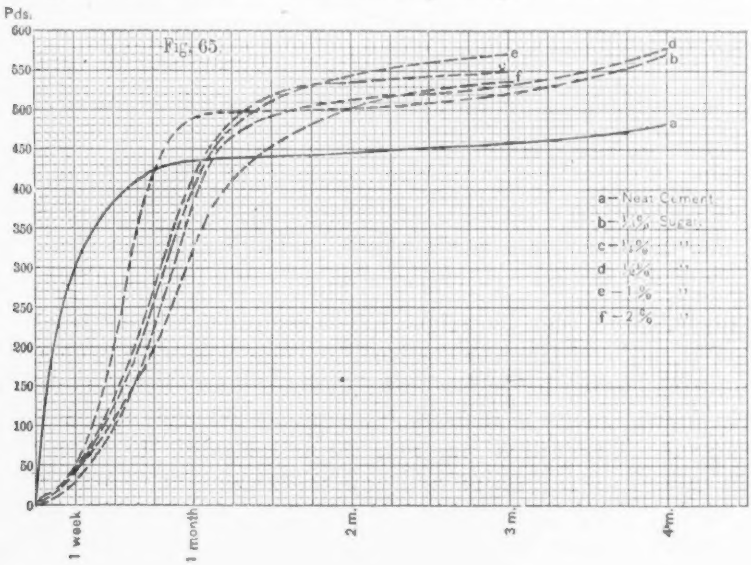
TIME OF TESTS.	1 Day.	2 Days.	1 Week.	2 Weeks.	4 Weeks.	2 Months.	3 Months.	4 Months.
Neat	70⅓	160	298⅔	388	436	446	456½	485¼
With 1% molasses....	20	27		74¾		178¾	353½	

These tests are graphically represented in Fig. 64.

Here it will be noticed that while the curve of neat cement rises steep and full, the curve representing the mixture of cement with one per cent. of molasses is considerably lower; rising, however, at the end of two months very noticeably and approaching the curve of neat cement, which the writer is led to believe, by his experience, it would have crossed if the tests could have been extended further. The figures given are the averages in each case of from four to six tests. The general appearance of the briquettes was good, although after a time most of them became covered with a slimy mold.

SERIES B.

In order to avoid the ill effects produced by the molasses, this series of tests was made with pure crystallized sugar. A known weight of sugar was dissolved in a measured quantity of water to form the



standard solution. The proper quantity of this solution was added to the water for working up the cement, as in the test where molasses was used. The manipulation of the briquettes, was in every way similar to that of "Series A." The cement used was the same, namely, Dyckerhoff's German Portland. No trouble whatsoever was experienced with the solution of sugar or with any mold on the briquettes. The amount of sugar used for each briquette is the stated percentage on the weight of cement therein. The results may be tabulated as follows, the figures representing, as before, the tensile strength in pounds per square inch:

Time of Tests.	1 Day.	2 Days.	1 Week.	2 Weeks.	4 Weeks.	2 Months.	3 Months.	4 Months.
Neat..................	$70\frac{1}{3}$	160	$298\frac{2}{3}$	388	436	446	$456\frac{1}{2}$	$485\frac{3}{4}$
With $\frac{1}{8}$ % of sugar...	9		$51\frac{1}{2}$		489		$522\frac{3}{4}$	$565\frac{3}{4}$
" $\frac{1}{4}$ % " ...	$4\frac{1}{3}$		$41\frac{1}{3}$		411		$552\frac{3}{4}$	
" $\frac{1}{2}$ % " ...	2		$34\frac{1}{2}$				$527\frac{3}{4}$	$571\frac{1}{2}$
" 1 % " ..	2		47		$403\frac{1}{2}$		$572\frac{3}{4}$	
" 2 % " ...	2		50		334		$537\frac{3}{4}$	

This series of tests is graphically represented in Fig. 65.

SERIES C.

This series of tests was made with F. O. Norton's Rosendale (natural) cement, with the same standard solution of sugar as in "Series B." The manipulation and treatment of the briquettes were the same as before, with the exception that 40 per cent. of water was used for gauging.

The results obtained were as follows, and are graphically represented in Fig. 66:

Time of Tests.	1 Day.	1 Week.	2 Weeks.	4 Weeks.	2 Months.	3 Months.
Neat....................	$47\frac{2}{3}$		62	$118\frac{1}{2}$	$225\frac{1}{2}$	$255\frac{1}{3}$
With $\frac{1}{8}$ % of sugar.......	15	$25\frac{1}{3}$	27	74	$243\frac{3}{4}$	$299\frac{1}{2}$
" $\frac{1}{4}$ % "	$27\frac{1}{2}$			99	238	$301\frac{1}{2}$
" $\frac{1}{2}$ % "	5			76	$154\frac{1}{4}$	264
' 1 % "	1				34	$181\frac{3}{4}$

From a comparison of the curves of strength, we are able to note very clearly the effects produced by the addition of sugar.

With the molasses (Fig. 64) the cement was retarded in setting, and very much more so than when the same cement was used with the addition of pure sugar, as in Fig. 65. This was probably due to the great quantity of impurities which the molasses contained; and perhaps, also, to some active chemical operations which may have taken place before the cement became thoroughly set. The same slow setting was also exhibited by the F. O. Norton cement, when large percentages of sugar were used (as for example say 3 and 4 per cent.), the cement requiring at least 48 hours before it became sufficiently hard to remove from the molds. In fact, some of the briquettes of the Norton cement, when mixed with only 2 per cent. of sugar, were so soft at the end of 28 days in water that they would not stand handling, but crumbled when touched. In cases where more than 2 per cent. of sugar was added to the Portland, and more than 1 per cent. to the Norton cement, both were rendered practically useless. The sugar did not seem to have any chemical effect upon the briquettes, for on examining the surfaces of fractures, crystals of sugar were easily detected. These crystals varied in size, were sometimes single and

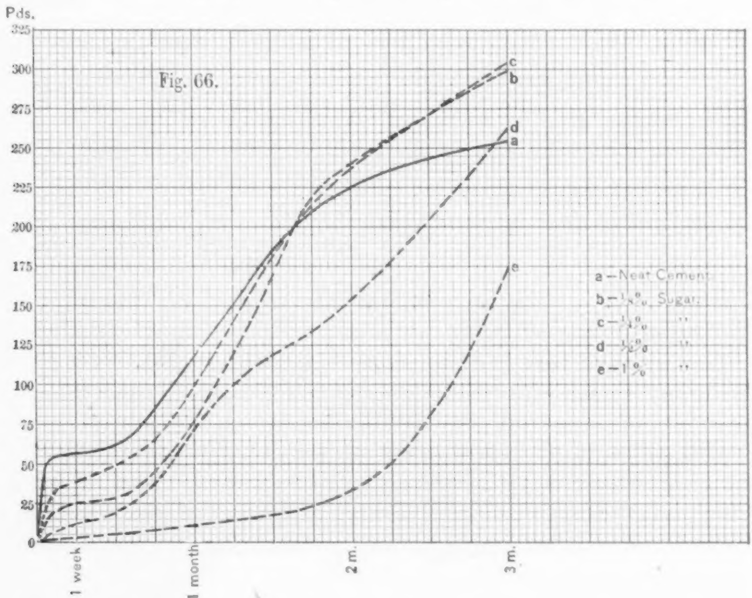


Fig. 66.

at others were clustered together; generally they were found in the air-holes throughout the briquette. Of course the sugar on the outside, or very near the surface of the briquettes, was dissolved by the water in which they were placed. On the contrary, the sugar in the middle of the briquettes seemed to have collected in these small air-holes and formed its crystals as the cement set. The reason why the sugar should give increased strength to the cement appears to the writer to be more mechanical than chemical. That is to say, the sugar by its presence in the briquette appears simply to retard the setting of the cement, and thereby permits the chemical changes in the cement to take place more perfectly. In order to insure the greatest accuracy, all tests were rejected whenever there was the least cause for suspicion; thus there are recorded herein only about 70 per cent. of the tests made.

DISCUSSION.

Prof. J. E. Denton.—The fact shown by these experiments, I believe, is, that the addition of sugar gives about the same tensile strength of cement as is obtained with cement and sand only. Mr. Parsons has gone through a very elaborate and careful series of experiments, which reflect a great deal of credit upon his ability in this respect; but can he give an answer to this question: How far has sugar ever been used practically, and what kind of work would call for the use of it? In practice, cement must be mixed very fast and by a very rude kind of labor. I cannot conceive what kind of work would pay for the trouble and care necessary to make the sugar addition as carefully as a chemist must weigh out his ingredients. I think the point is mainly to get the sugar out of the cement, not to put it in.

Mr. L. G. Engel.—I should like to ask Mr. Parsons, as he said he could not keep a standard solution of molasses, how long a time elapsed between the different experiments, so as to understand whether he managed to keep a standard solution of sugar. I raise this point because I recall an instance in which there was some trouble with gauges for sugar work, and the maker insisted that the gauges were right for testing purposes, and in order to prove his position he said he had used the same standard solution for eight years. Of course the solution must have been changing in all that time, and yet he continued to standardize his gauges by the same solution. Now, if any great interval of time

elapsed between the different tests, it is possible that the standardized solution of sugar may have altered in different experiments.

Mr. Parsons.—The solution used was fresh. The amount of sugar and the quantity of water were carefully weighed on a chemical balance. The standardized solutions of sugar were watched carefully so that they did not alter in their composition.

In reply to the other questions, I should like to state that the sugar gives from 15 to 20 per cent. increased efficiency, so to speak, and it is for that reason that we want to add sugar, instead of keeping it out. These are the first experiments ever made—or rather, ever published—upon the strength of cement with the addition of sugar. Now, in so far as its utility is concerned, it is very much restricted; but possibly in work of this kind, it would be useful—for instance, in repairing some fancy stone-work or molding, where the surface of the stone is quite large but the fractured part is very small—to use the cement for the purpose of reuniting the broken parts. This has actually been done upon the Peterborough Cathedral in England. Then under heavy engines and machinery, where the area is small for the weight supported, and and it is desirable to use the strongest possible kind of cement. Then again in certain structures where the stones are subjected to very great tensile pull, such as is illustrated in the lower courses of light-houses, which are slapped by the waves, and then as the waves retreat from them, they tend to draw the stones out, making it necessary for the engineers designing such structures to dovetail these stones together. For bridge-work, and other large structures of that kind, the danger from using sugared cement is too great. It should only be used under certain peculiar conditions, where an expert or inspector can watch the amount and quantities put in.

CCLXXX.

AN INTERESTING INDICATOR DIAGRAM.

BY CHAS. E. EMERY, NEW YORK CITY.

(Member of the Society.)

DURING a recent inspection designed to ascertain which of the older vessels of the U. S. Revenue Marine Service should be replaced, the writer, when steaming over historic waters covered by the guns of Sumter and Moultrie on the U. S. revenue steamer "McCulloch," noticed that the exhaust dragged, and observed that the steam chest was quite small for the size of cylinder. Instructions were given to test the piston and to make some slight modifications in the piping to enable indicator diagrams to be taken not only from the cylinder but from the steam chest and condenser. A sample diagram as taken by Asst. Eng'r E. G. Schwartz, U. S. R. M., is herewith presented. (Fig. 67.)

This particular vessel was originally a large tug boat transferred from the navy after the war, built up to form quarters for a boarding revenue cutter, and afterward lengthened to increase the accommodations. The engine was of the vertical inverted type, with cylinder thirty inches in diameter and thirty inches stroke of piston. This size of cylinder had evidently been placed on a frame built for a smaller engine and the valve chest retained of the same size as was ordinarily used on the smaller cylinder. The cylinder ports were only fourteen and a half inches long and two inches wide. There was an independent cut-off on the back of the main valve operated by right and left hand screws, the main valve being operated, as is customary, by a link motion for reversing. The indicator was of the Richards pattern, old type.

The speed of a marine engine may vary perceptibly from one revolution to another on account of slight movements of the vessel, so that it is rare to have the diagram of one stroke follow the previous one precisely. The double lines in the diagram herewith presented are due principally to this cause, although there are defects due to the indicator itself and the string connection,

which, however, are not of great importance in relation to the purpose in hand. With the light spring used, all defects are greatly magnified. The hull of the vessel was unsound, and the machinery was being run with such repairs only as were abso-

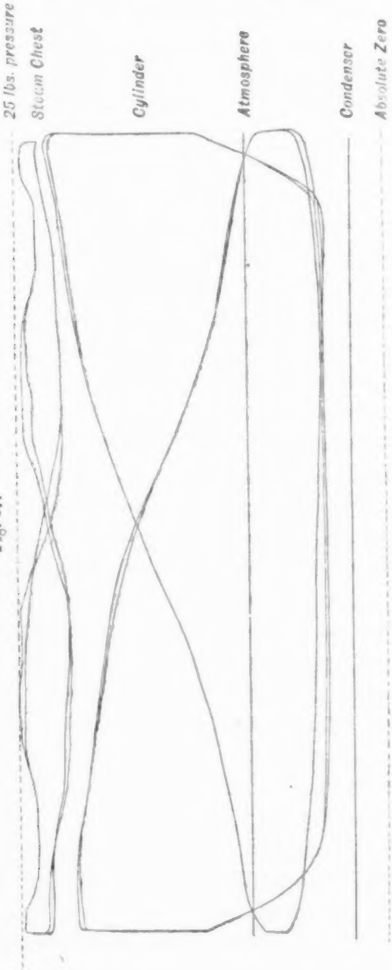


Fig. 67.

lutely necessary until a decision was reached as to the advisability of general repairs; hence the report was not surprising that on trial the piston was found leaky and the main valve somewhat so. These features, however, together with the conditions due to proportions of parts makes the evidence of such defects in the diagram

of special interest. The diagrams from the two ends of the cylinder will be readily recognized and show no special features other than might be expected with comparatively contracted passages and a leaky piston. A line has been drawn a distance corresponding to 14.7 lbs. below the atmospheric line to fix the position of zero or full vacuum, and another corresponding to 25 lbs. to show the boiler pressure as indicated by the gauge. The horizontal line above the absolute zero was drawn by the indicator when connected with the condenser, and shows the so-called "vacuum" in the latter to have been 11.3 lbs., corresponding to 23 inches of mercury or 2 inches less than indicated by the vacuum gauge, which was probably incorrect. The vacuum in the cylinder is, however, much less than this, or at the lowest point of the diagram only 8.5 lbs. The upper diagram shows the pressures in the steam chest at different parts of the double stroke. As will be observed, the pressure in the chest drops abruptly several pounds when steam is admitted to the cylinder, then falls still more, so that the line runs nearly parallel to the steam line of the diagram, which of itself shows a fall of pressure due to "wire-drawing," but near the point of cut-off the pressure in the chest rises rapidly, and at about three-quarter stroke runs for a little distance evidently *higher than in the boiler*, then falls lower than the boiler pressure near the end of stroke, and rises again above it slightly before the return steam stroke commences; the operation being repeated on the return stroke. The pressure in the boiler must evidently be less than that due to the crests, and greater than that due to the hollows at the ends of the diagram, or less than 25 lbs. as indicated by steam gauge.

It is interesting to study why the pressure in the chest apparently rises higher than in the boiler. It does not seem possible that the indication can be due to any large extent to vibration of the indicator pencil. A number of diagrams taken at the same time all show the same features. It is believed that the phenomena are due to the *vis viva* of the steam in the steam pipe. This pipe is $5\frac{1}{2}$ inches in diameter inside, and 28 feet long, with six bends in it, counting the changes of direction in the stop valves. For nearly half the stroke the steam is moving through this pipe at a high velocity, and the motion must be kept up for a little time after the cut-off valve closes to restore the pressure, and this is done so quickly that an increase of pressure in the chest is required to check the velocity. This increased pressure causes a return cur-

rent to the boiler of which the velocity is checked by the increased pressure there, and an actual fall of pressure in the chest, thus causing motion again in the direction of the chest where the pressure is again banked up just before the main valve opens at the beginning of the next stroke.

Indicator diagrams taken by the writer from good marine engines generally show an initial pressure only two to three pounds less than that in the boiler. A vacuum of ten pounds in the cylinder may be considered the average, though eleven and often twelve pounds are obtained in good compound engines.

A steam-chest diagram taken in a previous instance from a better class of engine did not prove of special interest, but as there are cases where such diagrams would be of great value, the writer suggests that in future the indication of an engine to ascertain its condition should not be considered complete without taking diagrams from the steam chest and exhaust pipe, in addition to those from the cylinder. The exhaust diagram shows at once the amount of back pressure and the steam-chest diagram the available pressure from which the cylinder draws its supply. The steam-chest diagram may be particularly valuable to show how the loss of pressure from the boiler to the cylinder is distributed, and to what extent modifications in the shape of the diagram are due to defects in the steam supply.

The blank used on the back of the diagram in the United States Revenue Marine Service is also presented on the opposite page.

The writer found that engineers, in filling up the revolutions on blank, generally counted for a minute, and frequently, therefore, included one revolution too many by not allowing for the beginning of the next minute. To obviate this, provision is made on the printed blank for recording the reading of the counter at a particular minute before placing the blank on the drum, and another line is provided for recording the reading of counter again after diagram is completed, when the average, if the engine be allowed to run for several minutes, eliminates the error referred to and gives much more accurate general results than ordinarily obtained. The remainder of the blank will be understood without explanation.

No. 2026.

U. S. REVENUE STEAMER ..... **McCulloch**

Scale of Indicator ..... **20** ..... *pounds per inch.*

..... *Engine.* ..... *End of Cylinder.*

Date, ... **Nov. 9th** ..... *, 1887.* *Hour,* ..... **5.00** ..... *P.M.*

Reading of Engine Counter, before taking diagram at ..... 5 h ..... **06** min. ..... ***P.*** M. **93,123**

" " " " after " " " ..... 5 h ..... **11** min. ..... ***P.*** M. **93,483**

Differences, ... 5 min., ... **360** Revs.

Revolutions per minute **72**

Steam pressure ..... **25** Lbs.	Temperature of Sea Water ..... **62** Degs.
Vacuum ..... **25** Ins.	" Discharge Water ..... **104** "
Position of Throttle ..... **10** Tenths.	" Feed Water ..... **130** "

Force of the Wind ..... **6** | Speed of Vessel ..... **13** Knots.

Direction of the Wind, ... ***Fresh wind on Stbd Bow,*** .....

Direction and description of the Sea, ... ***Sea smooth*** .....

Sail, ...

Remarks, ...

.....

..... *Engineer.*

DISCUSSION.

Mr. C. T. Porter.—This is an ordinary diagram from a cylinder. The steam pressure is 25 pounds, which is nowadays far from "interesting." The initial pressure obtained in the cylinder is less than this by $3\frac{1}{2}$ pounds at one end, and $6\frac{1}{2}$ pounds at the other end, the latter loss being more than one-fourth of the whole pressure above the atmosphere.

The cut-off is so gradual that the point of cut-off cannot be found. The vacuum is remarkable for its badness. That maintained in the condenser is $11\frac{1}{2}$ pounds; in the cylinder the average vacuum is about 8 pounds.

This ought to be an unusual diagram. Unfortunately, it is, especially in marine engines built for the government, a very common one.

The point of interest is the diagram from the steam chest. This ought to be taken from all engines. Such has been my own practice for twenty-five years. Examples of these diagrams from steam chests of high-speed engines are given in my treatise on the development and application of force in the steam engine.

These diagrams are very instructive. They reveal at once what proportion of loss or fall of pressure is due to insufficient pipe area, and what to insufficient port area and valve travel.

This important use of the indicator is well illustrated in this diagram. At the commencement of each stroke, we observe a fall of pressure in the chest. A further fall is seen as the piston advances, changing gradually to a rise as the port is slowly contracted. This mounts to the boiler pressure after the current in the pipe has been finally arrested.

We have then one reaction or pulsation of the elastic steam, and a rise again to the boiler pressure before the stroke is quite completed. These pulsations of the steam are gentle and lazy, owing to the gradual manner in which its momentum is arrested by the cut-off motion.

These diagrams vary endlessly, according to the length and size of the steam pipe, and the slowness or suddenness of the admission and cut-off, and the dryness or wetness of the steam.

After a sharp cut-off in a high-speed engine with a long steam pipe, I have seen the pressure rise in the chest to five pounds

above the boiler pressure, and four or five pulsations succeed one another before the stroke was completed.

I hope this paper will render this most interesting and instructive use of the indicator much more general than it has been.

Mr. Emery.—It is gratifying to have Mr. Porter confirm the statement that the pressure in the steam chest may rise above that in the boiler. It is so stated in the paper from consideration of the diagram, though not so shown by gauge, which, however, was probably in error after long use.

It was considered interesting to find the numerous defects in the condition of the engine so well shown on an indicator diagram from the cylinder when supplemented by one from the steam chest.

CCLXXXI.

AN IMPROVED FORM OF SHAFT GOVERNOR.

BY FRANK H. BALL, ERIE, PA.

(Member of the Society.)

IN the development of steam-engine governors, from the earliest forms employed by the first designers down to the latest production of modern engineers, it is noticeable that almost without an exception centrifugal force has been employed in every successful design.

The only apparent success, without centrifugal force, has been in the direction of fluid governors, but even with these it has been found that the variations in the condition of the fluid used produce a corresponding departure from the desired speed, and the comparatively greater power required to drive the fluid governors is also a factor in the unfavorable verdict which practical use has established.

Although the mechanical constructions of centrifugal governors present an infinite variety of designs, yet certain fundamental elements appear in every case. In every governor will be found a weight or weights made to revolve around an axis, and having a latitude of motion to and from the axis, which motion is made to control the steam supply. It will always be found, also, that the centrifugal force developed by these weights is opposed by gravity, or springs, acting as a centripetal force.

The action of the governor is therefore the result of a balancing of these forces.

Fig. 68 illustrates a very large class of governors in which the gravity of centrifugal weights is made to oppose their centrifugal force.

Springs are sometimes used in this construction to produce additional centripetal force.

Fig. 69 shows another type of governor, such as is commonly used on the main shaft of engines. In this form the gravity of one weight neutralizes the other, and springs are depended on

entirely for centripetal force. In all these illustrations the weights are shown in their initial position, or the position nearest the axis, and the outer position is indicated by dotted lines.

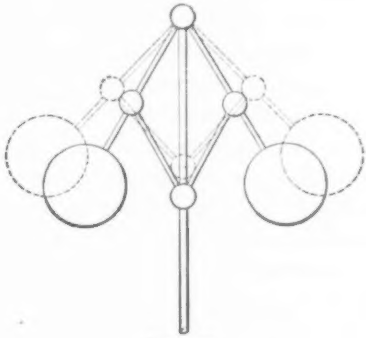


FIG. 68.

In the operation of a centrifugal governor, any intermediate position taken by the weights must be one where the opposing forces are exactly in equilibrium.

As the weights move outward from their axis the centrifugal

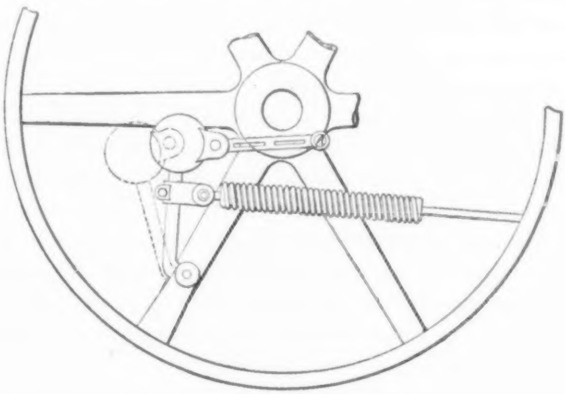


FIG. 69.

force increases directly as the distance from the axis is increased.

The opposing or centripetal force may also be made to increase in exactly the same ratio or scale. If so arranged, then, when the speed is such that the forces are in equilibrium in one position of the weights, they will also be exactly balanced in every position

from one extreme to the other, and an engine having a governor so adjusted might be expected to show the same rate of speed under every condition of load.

Unfortunately, however, a difficulty arises in the nature of instability; for if adjusted as described, then whenever in any position of the weights the equilibrium between the opposing forces is disturbed by a change of speed, and the weights are thereby caused to move in the direction of the greater force, the same lack of equilibrium will exist when the weights have reached the extreme limit of motion in that direction; and when the weaker force shall have become the stronger, by reason of the rapid change of speed that must follow, then the weights will move immediately to their extreme limit in the opposite direction, repeating this process constantly, and producing an effect that is called "racing" or "hunting."

To prevent this, and to enable the governor to take all the intermediate positions with stability, it is necessary to arrange the centripetal force so that it will increase or decrease more rapidly than the centrifugal force when the weights move from one position to another.

If so arranged, then a slight movement of the weights in the direction of the greater force will bring them to a position of equilibrium, and a still further change of speed will be required to move them to the next position.

This condition of stability is therefore obtained at a sacrifice of uniformity of speed, and the greater the variation of speed between the extreme positions of the governor, the greater the stability which will be obtained.

Every designer of centrifugal governors has had to face this problem and decide just how near an approach to perfect harmony between the opposing forces would be safe, knowing that if he approach too near he must pay the penalty of instability, which is a fatal error.

The question as to how near this theoretically perfect condition it is safe to adjust a governor depends somewhat on certain conditions.

Light balance wheels, extreme changes of load, and friction in governing mechanism are all elements which prevent a near approach to the condition of adjustment that should produce uniform speed.

The inertia of the centrifugal weights is also an obstacle to fine

adjustments, as it first prevents quick response and then overreaches the mark.

For this reason high rotative speeds are desirable, as the necessary centrifugal force may be obtained from small weights having little inertia.

A perfect governor should be free from unnecessary friction, and have as little weight to the moving parts as possible.

Its function is essentially to weigh quickly and accurately two opposing forces, just as a steam engine indicator weighs the force of steam opposed to the spring, and yet there are those who load a governor down with weights other than centrifugal weights merely to produce centripetal force, when a suitable spring would produce the desired force without the incumbrance of weight and consequent inertia.

Recognizing the necessity of sacrificing theoretically correct regulation, or isochronism as it is technically called, for the sake of stability with centrifugal governors, numerous attempts have been made to obtain a better result by other means.

The fluid governors were found to come under the same general law as centrifugal governors, so far as the necessity of sacrificing isochronism for stability is concerned, and in addition to this, other defects appeared which have already been mentioned.

Several attempts have been made, in this country and Europe, running back through a period of over thirty years, to construct a governor without centrifugal weights which should produce uniform speed by a weighing of the load. It is safe to say that not one of these was ever a success, for the obvious reason, if for no other, that they contained no provision whatever for governing against changing boiler pressure.

In 1878 the writer conceived the idea of constructing a centrifugal governor on the shaft of an engine having all the functions of the best forms of centrifugal governors for governing against changing boiler pressure, and also so arranged that the driving pulley, instead of being keyed to the shaft, is driven by the governor in such a manner that the load is made to act as centripetal force.

With this arrangement the governor may be adjusted for ample stability, and the effort of the load adjusted exactly to correct the change of speed that otherwise would take place.

Fig. 70 illustrates the arrangement for weighing the load.

The curved arm A is keyed to the shaft and the belt wheel is driven from the connection at the end of this arm.

The effort of the engine is thus made to act as centripetal force, and by a suitable adjustment of this force the governor weights are brought into the required position at every stage of load to compel the engine to maintain a constant speed.

This combination proved to be successful for giving exact regulation against changes of load.

With changes of boiler pressure, however, this governor acts only

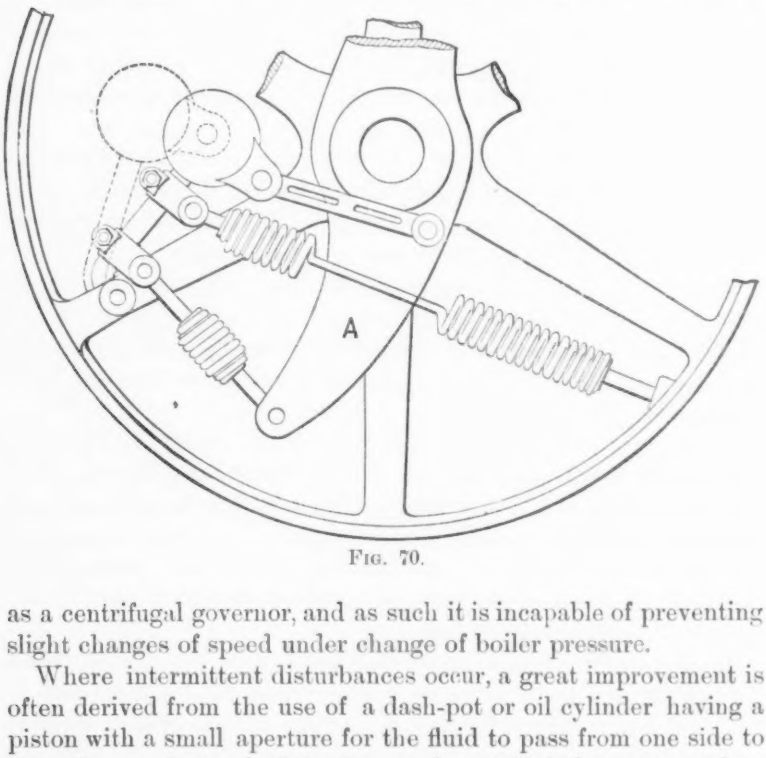


FIG. 70.

as a centrifugal governor, and as such it is incapable of preventing slight changes of speed under change of boiler pressure.

Where intermittent disturbances occur, a great improvement is often derived from the use of a dash-pot or oil cylinder having a piston with a small aperture for the fluid to pass from one side to the other, and attached to the moving part of the governor in a manner to prevent sudden motion.

Rankine describes the use of a dash-pot in his book entitled *Machinery and Mill Work*, published in 1859, in Section 364.

Most of the builders of Corliss engines have found it desirable to make use of a dash-pot in their governors, owing to the inertia of the large slowly revolving weights, and also to the intermittent disturbances of the releasing valve-gear.

Among the shaft governors, or shifting eccentric governors according to the classification in the Patent Office, the Buckeye Engine Company, of Salem, O., seem to have been the first to apply dash-pots.

They found that with large engines, where, owing to the slow rotative speed, the forces of the governors were weak in comparison with the intermittent disturbing elements to be overcome,

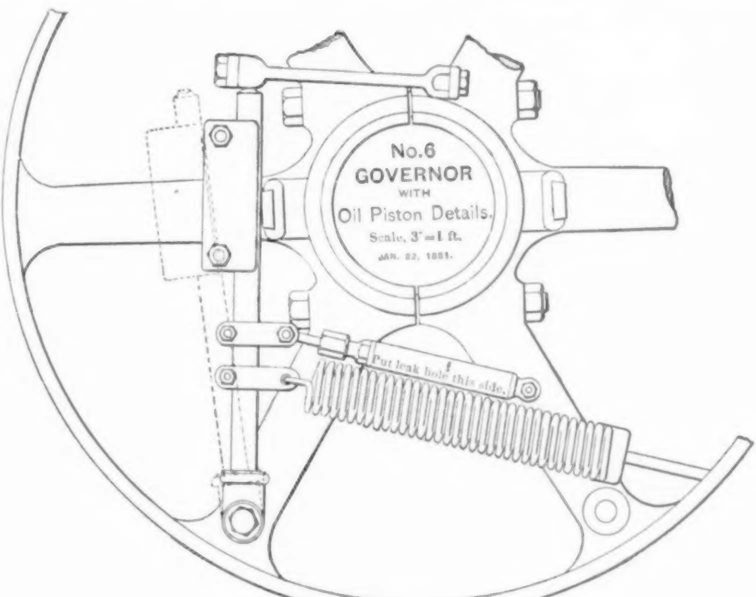


FIG. 71.

that a dash-pot would materially improve the action of the governor.

In 1878 they made use of dash-pots in one of their governors on an engine at the Syracuse Iron Works, of Syracuse, N. Y.

A more uniform action of the eccentric was thereby obtained, and from this experience the same difficulty in other engines was remedied in a similar manner.

In 1879 they put similar dash-pots on an engine at the works of the White, Potter & Paige Manufacturing Company, of Brooklyn, N. Y., for the same purpose, and in 1880 applied the same remedy to a pair of engines at "Dows' Stores," Brooklyn, N. Y., following it up in 1881 on engines at the Hartford Carpet Company, and at

the Pacific Mills, Lawrence, Mass., and many others which have not come under the writer's notice.

Fig. 71, taken from a working drawing of a Buckeye governor with a dash-pot, shows the method of its application.

It is here attached to the arm which carries the centrifugal weight, and through its connection with the eccentric is made to steady the action of the eccentric.

Profiting by the experience of the Buckeye Co. and others, A. L. Ide, of Springfield, Ill., designed a shaft governor in 1882 having a dash-pot for preventing irregular motion of the eccentric.

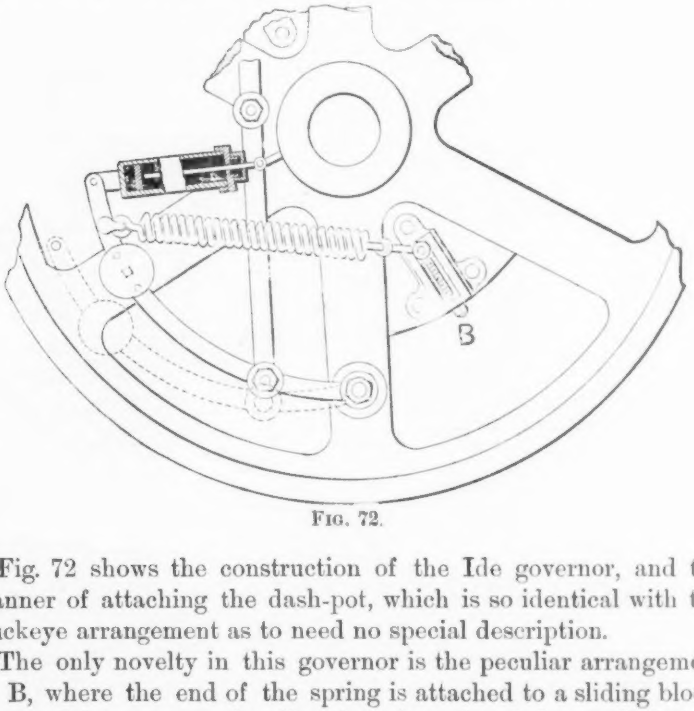


FIG. 72.

Fig. 72 shows the construction of the Ide governor, and the manner of attaching the dash-pot, which is so identical with the Buckeye arrangement as to need no special description.

The only novelty in this governor is the peculiar arrangement at B, where the end of the spring is attached to a sliding block, operated by a screw, the direction of motion being such that by this single operation a double effect is produced—that of shortening the arm or lever by which the weight is controlled, and at the same time increasing the initial tension of the spring.

In centrifugal governors where the forces are weak and easily disturbed, this dash-pot permits a finer adjustment, and therefore makes the governor more nearly isochronous; but it also makes the action sluggish, and it is questionable whether without the

dash-pot the governor cannot be made fully as near isochronous by increasing the forces of the governor. If so the dash-pot is the less desirable remedy and amounts to a positive damage.

A dash-pot governor of any of the kinds described, adjusted to be strictly isochronous, must depend entirely on the resistance of the fluid for stability, and this resistance must become an important factor in the governor. Consequently the sluggish action must be exaggerated or increased, thereby producing disastrous results. When a governor is thus arranged and adjusted, if in the operation of the engine it becomes necessary for the governor to move from one position to another, it cannot do so quickly, but must drag along tardily, during which time the engine is getting badly off its speed.

Therefore the former instability in the nature of rapid and extreme fluctuations of speed is changed by the dash-pot to more moderate fluctuations, each of which covers a longer interval of time.

Dash-pot governors such as have been described do not appear to be capable of giving satisfactory results under a finer adjustment than is employed with strong centrifugal governors without dash-pots. At least such would be a fair inference from the fact that the great mass of governors are still made without dash-pots after more than a quarter of a century of experience with their use.

During the past year the writer has discovered a new application of a dash-pot to centrifugal governors, which seems to be entirely free from the difficulty formerly encountered.

The principles involved may be understood by reference to Fig. 73. The governor here shown is one of the ordinary forms of shifting eccentric governors.

The introduction of the spring S between the dash-pot and the movable part of the governor is the new feature. Its operation and effect are as follows:

Suppose the long spring D be drawn up until its initial tension, in distance of stretch, shall correspond exactly with the distance between the center of gravity of the weight and the axis of revolution. This is what is called "full theoretic tension."

The condition is the same as would be obtained if the weights were first placed at the center of the shaft, and after attaching the spring without any tension the weight was then moved out to the position shown.

With this relation between the position of the weight and the

tension of the spring, the increase and decrease of centrifugal force caused by moving the weight to or from the axis of revolution would exactly harmonize with the changes of resistance of the spring due to said motion; and if the two forces were in equilibrium in one position, they would be so in every position at the same speed. This condition, as has already been said, should be expected to give uniform speed of the engine at every position of the governor, but has been found impracticable on account of its instability.

The object of the dash-pot and spring here shown is to allow

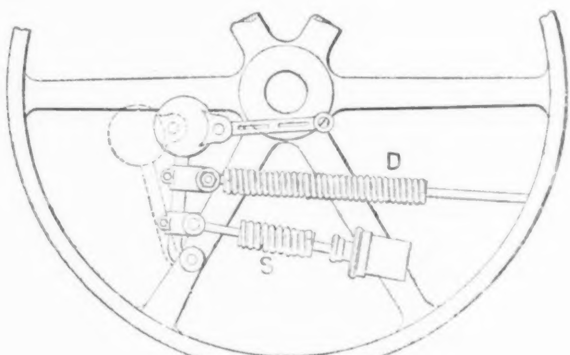


FIG. 73.

the theoretically perfect adjustment of the long spring, and to furnish ample stability without making the governor sluggish, or in the least preventing a quick and delicate balancing of the forces.

This spring S is arranged for both compression and extension, and has a range of deflection sufficient to allow the full motion of the governor, from one extreme to the other, without regard to the motion of the piston of the dash-pot to which it is attached.

The resistance of this spring S, having no initial tension, is entirely out of harmony with the other spring, and combined with them produces exactly the effect when motion takes place that is obtained ordinarily in centrifugal governors, by using springs with less than the full theoretic tension, and if the dash-pot piston should remain stationary, the same change of speed would be found between the extreme positions of the governor, but by reason of the movement of this piston, the tension on spring S is released, and it then ceases to be a factor in the speed, which is only the

result of the long spring, and as has been previously shown it must be the same at every position of the weight.

This theory, though somewhat obscure, seems to be correct, and its practical operation under careful tests proves it to be so.

Governors are now made of various types, embodying this principle, and have been found to compel the same number of revolutions per minute of the engine under any condition of load or boiler-pressure within the full capacity of the engine.

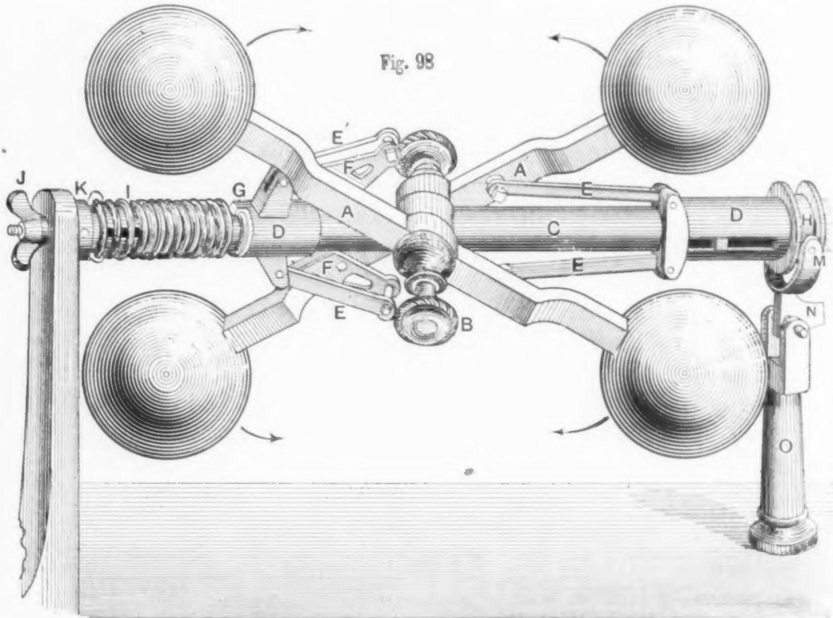
DISCUSSION.

Prof. J. B. Webb.—Both the ideas mentioned in the paper struck me as admirable and ingenious. The idea of putting the fly-wheel separate from the shaft and connecting it by springs is certainly a beautiful one, and the combination of dash-pot and spring allows the governor to stand in any position, and therefore with any cut-off, for a given speed of the engine, which is a thing not obtainable in an ordinary centrifugal governor, but can theoretically be obtained in a fluid governor.

Mr. J. T. Hawkins.—As a slight contribution to this subject, I might mention a fluid governor which I saw in operation upon a beam engine running the works of the Messrs. Hoe & Co. as long ago as 1852 or 1853, if my memory serves rightly. It was a condensing engine of about five feet stroke, making about 60 turns per minute, and was fitted with the Sickles cut-off, cutting off, probably, at from $\frac{1}{4}$ to $\frac{1}{2}$ stroke. The governor operated through the cut-off, and, as I remember, it was perfectly isochronous in its operation. It consisted of a cylindrical chamber, open at the top, partially filled with oil. Within this cylinder were two small, single-acting pumps, connected at their lower ends. One of these pumps was operated by the engine, making some fifteen or twenty strokes to one revolution of the engine. This pump took oil from the chamber at the bottom, at one side, through a valve opening inward, and delivered it to the second pump at the bottom, on the opposite side, through a valve opening outward. A pipe led from this first pump to a valve opening inward at the bottom of the second pump; and at the opposite side of the bottom of the second pump was a simple orifice, without valve, for the discharge of the oil back into the chamber. The plunger stem of the second pump was made so as to receive weights which might be varied in amount; and on the same stem were secured tappets which operated upon the two sides of a lever connected suitably with the cut-

off mechanism, the space between the tappets being such as to permit of the plunger rising and falling without striking the cut-off lever when the engine ran at normal speed. As the oil could flow only at a given rate through the fixed orifice in the second pump under the load or weight placed upon the plunger stem, any slight increase in the speed of the engine would cause the oil to be pumped into the space below the plunger of the second pump faster than it could escape through the fixed orifice, which would cause this plunger to make its vertical excursions between higher

Fig. 98



points until the engine arrived again at its normal speed, after which it would continue to work between these higher points, corresponding to a shorter admission of steam by the cut-off; and for a slight slowing of the engine the converse would take place. The speed at which the engine should run was, of course, determined by the weight applied to the plunger of the second pump.

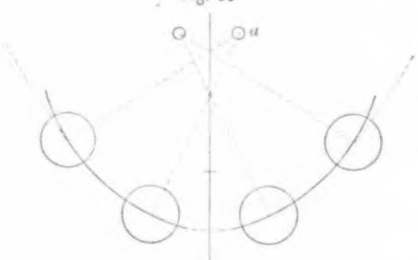
In this governor, while it required a variation in the speed of the engine to effect a change, the speed must be restored to the normal as soon as the admission of steam became just equal to the performance of the work demanded by the change of load upon the engine; and, being positively operated by an inelastic fluid,

it was not in the least affected by the resistance offered by the tripping mechanism of the cut-off.

Prof. Webb.—Mr. Lyne has just mentioned an idea, but he is too modest to bring it up. He remarked that that was a sort of a cataract, so that in the original engines we had that fluid governor.

Mr. Geo. H. Babcock.—There are two very nice things in this governor. One is the idea of a spring being adjusted in such a way that the increase of resistance of the spring is exactly equal to the increase of the centrifugal force of the weight. I presume that Mr. Ball is familiar with the fact that some thirty odd years ago Mr. Silber built a marine governor of that kind (Fig. 98).* He used a horizontal shaft, on which he pivoted one or two arms with a ball at each end. These were connected by a rod to a collar, against which he put

Fig. 99



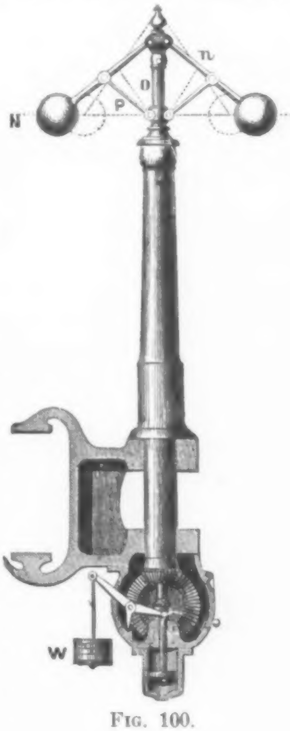


FIG. 100.

a spring adjusted exactly like Mr. Ball's. This spring was of such a length that at every position of the balls it was extended or compressed, as the case might be, just in proportion to the distance of the balls from the center; that is, if the center of the balls could have been made to coincide with the center of the shaft, the spring would have been normal. The centrifugal force, therefore, exactly counterbalanced the spring at a given speed at all positions of the balls. He found the difficulty which Mr. Ball found—that is, that his governor was all up and all down. It worked very well, however, on a marine engine, because that is

* Copied from the *Scientific American*, July 19, 1856.

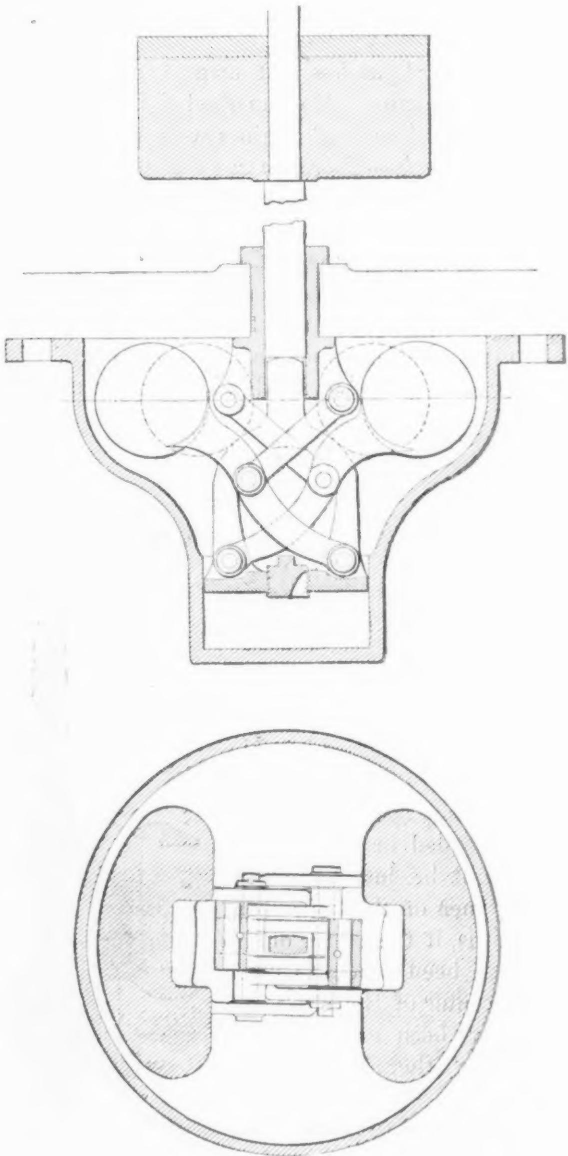


FIG. 101.

just what you want with a marine governor. But Mr. Ball's addition of a dash-pot with an interposed spring is a stroke of a genius.

By it he retains the isochronism with the necessary slowness of motion.

Now, it is well known to all that a governor ball which rises in a parabolic curve will be isochronous. It is also easy to find a point, like *a*, Fig. 99, for instance, which will be practically a center for so much of the curve as you wish to use. A governor made thus with crossing arms will be isochronous, the centrifugal force being resisted only by the weight of the balls. I have used a governor of that construction considerably. I have also used a governor in which the balls were hung so as to move in a plane vertical to the shaft (Fig. 100.) The levers upon which the balls were hung produced a straight line motion. Thus the weight of the balls formed no part of the resistance to the centrifugal force. Then, by a proper attachment of a counter-weight (W) or spring, I could get any action I wanted, making the resistance bear any relation to the centrifugal force which was desired. By combining the two I produced a governor which could be adjusted to any speed, and at the same time be practically isochronous (Fig. 101.)

Prof. Denton.—Why have these perfect devices not been retained in use? We never see them.

Mr. Babcock.—Well, I built hundreds of them and they worked very well, and many of them are in use to-day; but I found it necessary, in every instance, to throw them out of isochronism sufficiently to avoid racing. In the case of Fig. 99 it is done by placing the center so that the balls would travel a little out of the true curve. In the case of Fig. 100, it was done by the arrangement of the weights upon the bent lever.

Prof. J. E. Denton.—After the compliment paid to Mr. Ball's device by President Babcock, nothing that could be said by any one of less experience can be added to this line of thought. But what the president has stated regarding his practice with isochronous governors suggests the following inquiry:

If the perfect or isochronous governor must be deliberately deprived of its power to act in complete accordance with an absolutely isochronous law, cannot the ordinary centrifugal governor be adjusted so as to approximate to the same degree of perfection to which the isochronous forms must descend in order to avoid the "hunting" action, which is explained in the paper?

The production of thoroughly satisfactory incandescent electric light affords the severest test of the degree of perfect action of

steam-engine governors, and, under the stimulus of the demand of the electric lighting business, governors have received their highest practical development. As a result, it appears that engines with plain centrifugal spring governors will run unloaded within a fraction of one per cent. of their speed at full load.

I believe that the attainment of this result is not dependent upon any peculiar arrangement of the governor, but upon the nicety with which the governor is adjusted. I have in mind a 25 H.P. Buckeye engine, which, by repute, was not possessed of the "particular kind" of centrifugal governor capable of satisfactorily controlling speed for incandescent light work.

The engine was designed and in use for supplying power to machine tools, and no particular demand had been made for close regulation of its speed. Consequently, when, upon an important occasion, the engine was applied to run 200 incandescent lights, the variation of speed occurring when the load was changed was fully 15 per cent., and the opinions regarding the lack of fitness of the governor for incandescent lighting were apparently confirmed.

But, upon sending to the makers of the engine for an expert mechanic who understood "nursing" the governor into the best state of sensitiveness, the engine was made to run at 284 revolutions with 27 H.P., and 286½ revolutions when unloaded, and the variation of speed during the change from one of these speeds to the other was as satisfactory as any demand of practice seems to require.

I have noted similar experience to the above with several of the best high-speed engines in the market, and I have reason to believe that the same view applies to the possibilities with the centrifugal gravity governor in large power plants of 500 H.P. and upward, notwithstanding that many engines of this class have, apparently, to call to the aid of their centrifugal governor various devices, such as dash-pots, half-moons of mercury, rolling weights, etc.

I do not wish to be understood, from this expression of views, as looking upon Mr. Ball's invention as a useless device. He is in the engine business, where alone a complete knowledge of the requirements in a governor can be obtained, and his paper shows him to have been a critical student of the principles upon which the action of governors depends.

My object is to attempt to define wherein the value of the improvement must lie, so that we may have Mr. Ball's confirmation

or criticism upon such definition, which is that the invention provides a possible means of

1st. Securing the same degree of perfection as the best centrifugal spring governors attain, but with less skillful adjustment than such governors require, and with less liability to loss of sensitiveness with continued service.

2d. Securing a more perfect action of existing centrifugal governors whose original proportions or design requires the application of a supplementary device (such as instanced above) to secure good regulation.

Mr. Ball can, I believe, cite practical applications of his device which afford positive testimony bearing upon these two conclusions.

Mr. Walter C. Kerr.—The question brought up by Mr. Denton is in the line of what I intended to say. He asks why ordinary governors cannot be made to give the regulation desired, and why these various supposed perfect forms do not give the regulation which they ought.

I think the principal reason has scarcely been mentioned in this discussion. It is friction. About one-half of all bad regulation is due to governor friction. Every one in the engine business knows that weights, springs, and lever arms can be so proportioned that when the governor is in a proper state of lubrication, it regulates in a thoroughly satisfactory manner, and the same governor when not properly oiled gives poor regulation; also that no changing of leverages, springs, etc., or the addition of extra parts, will bring a poorly lubricated governor into condition for satisfactory regulation.

Some forms of governors have their friction greatly diminished by excellent constructive features, and others by superior lubrication. I do not know what governor Prof. Thurston referred to in his paper, as I was not present, but it is probably Prof. Sweet's, as he made some experiments with the Straight Line engine. I happen to know something of the action of this governor, having worked on it in its earliest days, when friction was excessive, and when various plans were experimented with to improve it. They all failed, but when the friction was taken out of it by the excellent construction now used, it needed no dash-pots, supplemental springs, or other contrivances.

The most important question to ask in connection with governors is, What kind of regulation do we want? And the answer is:

First, a reasonably small variation in speed, whether the engine is light or loaded, said variation being measured in revolutions per minute. This variation can be made small or large, at the discretion of the manufacturer, and one or two per cent. variation is considered sufficiently close on engines used for ordinary manufactories. Second, a very small acceleration for an instant following a change of load, and where the most delicate regulation is required, this instantaneous acceleration is of much greater consequence than the difference in revolutions between light and loaded conditions.

In work requiring very delicate regulation, the injury is done during the period of this acceleration, and the period is the time between the change of load and the starting of the governor to move, and is proportional to the friction. It is therefore plainly evident that devices which are added to the governor, which act only after the governor has started to move, cannot in any way affect the acceleration of the engine during the period above mentioned. We therefore cannot look to auxiliary devices as a means of preventing the most serious consequences of bad regulation, and while they may be ingenious and mathematically correct, their operation is too late to obviate the difficulties which they are designed to remove.

Further than this, the addition of various devices usually increases the friction, and thereby may become even harmful where very close regulation is desired. Under such circumstances it is quite possible for a governor regulating within 2% to have its regulation reduced to 1%, when the regulation is measured in revolutions per minute, and at the same time the acceleration of said governor is found to be many per cent. greater than before the auxiliary devices were applied; and in such delicate adjustment of speed as is necessary for incandescent lighting, it is possible to thus produce a very injurious action on the lamps.

I believe that if more attention was paid to exactly what good regulation consists of, there would be less attention paid to devising appliances whose operation must be subsequent to the starting of the governor, and more attention paid to relieving the governor of the resistances which tend to prevent its starting from rest.

Mr. Hugo Bilgram.—It seems to me that one very important point has not yet been considered, namely, that the steam engine works intermittently. Especially in automatic cut-off engines there are periods within which the governor has absolutely no control over the engine. It has a chance to control only at one instant

during each half-revolution, namely, at the moment when the steam is cut off, and it is inoperative until the steam is cut off at the following stroke. Neither a rising or a falling of the governor can, in the intervening time, affect the speed. Let us suppose that a certain position of the governor corresponds with a development of 100-horse power, and imagine that just immediately after cutting off, 40-horse power were thrown off the load, the engine developing power at the rate of 100-horse power during the half-stroke following, the speed of the engine will accelerate, say for an illustration, by 10 revolutions. If now the governor were constructed so that by this increase of speed the cut-off will be set to a point corresponding with less than 60-horse power, the regulation will be greater than necessary, and a reversal of the regulation must follow, resulting in a rising and falling of the governor. But if the governor has been adjusted so that the increase of speed during one stroke due to the said change of load would just bring the cut-off to a point where 60-horse power will be developed, the resulting adjustment will be completed. I have always held that the fly-wheel is a part of the governor, and that both should be adjusted one to the other. Otherwise the governor may be either too sensitive or not sensitive enough. The change in the momentum of the fly-wheel should, as near as practicable, equal the change of work developed during one stroke.

The idea of using a dash-pot and spring in connection with an isochronous governor occurred to me some six years ago, the spring giving the necessary stability, the dash-pot permitting a slow release of the strain of this spring.

At that time I recognized a disadvantage of such, as compared with stable governors, in permitting a greater variation of speed if a given load is suddenly removed and then added again. Supposing a speed of 100 revolutions to be raised to 110 by the removal of a portion of the load, the isochronous governor will slowly reduce the speed again to 100. And when the original load is restored, a slowing down to 90 turns will result, which in time will again be corrected by the action of the dash-pot. Thus the variation will be between 90 and 110, when it would only have changed from 100 to 110 and then back again to 100 with a regular stable governor.

Mr. Lewis F. Lyne.—My neighbor, Professor Webb, has paid a very high compliment to my modesty, and he seems determined that I shall say something. Perhaps I can throw a little light on the

governor question by stating that I know some of the reasons why the so-called first-class governors have failed. The Huntoon I know to have failed on account of the lack of proper attention. It is a delicate mechanism. If you do not use the proper kind of oil in it—that is oil free from gum—it will not work satisfactorily. We have in our place a Buckeye engine, 18″ by 36″. The governor on that engine was condemned by our engineer; he could not do anything with it; he could not make the engine run steadily at all. I took it in hand and examined the governor and found three-quarters of an inch more tension on one spring than on the other. There was also half an inch more leverage on one of the arms than on the other. I released the strain on the springs, then took a pair of dividers and adjusted the levers exactly alike. I then put an equal tension on the springs, and by adjusting a proper relation between the leverage and the tension on the springs I got a practically steady regulation—so near so that we have had no trouble at all. We throw on 35 or 40 horse power at a time, or throw it off, and we do not see any difference in the speed of the engine to interfere with our lights. Electric lighting, of course, requires close regulation. Governors that have given entire satisfaction in the hands of men who understood them, if put into the hands of men who do not understand them—engineers who have given the subject no consideration—will fail every time. I think I can say, in answer to Prof. Denton's question, that the chief cause of the failure of much of the good apparatus that has been produced has been due to the lack of a proper understanding of their mechanism, or a lack of proper care.

Mr. E. F. C. Davis.—I think I can answer an inquiry of Prof. Denton's, to some extent, by saying that it had not been nearly so difficult to pick up a good governor to answer every practical purpose while new as it had been to find a governor that would keep up that class of work for any length of time. The want of durability is a thing that has thrown a good many good governors out of the market. Without regard to the matter of care, which all governors ought to have of course, a good many governors wear out fast; so I think Prof. Sweet's step is in the right direction, the eliminating of the friction, which is necessarily the destructive element in governors, and I think that his governor has a very long life before it for that very reason.

Mr. J. T. Hawkins.—Referring to remarks of Mr. Bilgram, the features which he mentions would perhaps have been of some

moment in some of the old long-stroke, slow-piston-speed engines such as we used to build; but I think it becomes of no practical consideration in the engines of the present day, with their high speed of piston, short stroke, and consequently vastly greater number of revolutions made in a given time. It is a common thing nowadays for a stationary engine to make anywhere from 300 to 500 revolutions per minute, or from 600 to 1,000 strokes of the piston; and in such a case it would seem that the time during which the governor has no control over the engine is so extremely small that no possible engine, with any fly-wheel at all, could fail to be perfectly governed for that reason. In the compound or triple-expansion engine, or, indeed, in a two-cylinder single-expansion engine coupled to the same shaft, in either of which the admission occurs in the different cylinders at different periods of the shaft's revolution, it would be of still less account.

If we instance a modern single-cylinder engine operating an electric-light plant—than which perhaps there is nothing that calls for closer governing—we have in many such cases engines running at over 300 revolutions per minute. In such an engine, cutting off at, say, $\frac{1}{4}$ stroke, the governor loses control during $\frac{3}{4}$ of $\frac{1}{600}$ of a minute, equal to $\frac{3}{40}$ of a second. In such a case, I think, considering the inertia of the fly-wheel usually adapted to such an engine, the loss of control during the expansive action of the steam is entirely too infinitesimal to be worth a moment's consideration. In fact, in such an engine as I described as having the fluid isochronous governor, making about 60 revolutions per minute and cutting off at about $\frac{1}{4}$ stroke, control by the governor would be lost during only about $\frac{3}{8}$ of a second, a period entirely to short within which to affect a heavy 18 or 20 foot fly-wheel such as this engine had.

Mr. J. B. Ladd.—Mr. President, I have been looking at the sketch on the board of what I think you mentioned as the first isochronous spring governor (Fig. 98). I think you gave the credit of the invention to Mr. Silber. I have in mind that Mr. Charles T. Porter constructed the first spring governor which is truly isochronous. Mr. Porter's governor consisted of two balls connected by links to a spiral spring as outlined in this sketch, and the spring was so proportioned and adjusted by giving to it an initial compression, that in resisting the radial motion of the balls, it balanced their centrifugal force for all positions.

This governor was designed by Mr. Porter for marine work, and I think that it is the first isochronous spring governor made.

The President.—Do you think that was before Mr. Silber's?

Mr. Ladd.—I think so, yes.

To return to the point last discussed, I would like to say that my experience has been exactly the same as that related by Mr. Kerr, that the friction is the most deadly enemy of all the governors; and further, to call attention to the fact that friction is detrimental, not simply because it is friction, but because it is a *variable quantity.*

*Mr. Geo. H. Babcock.**—In regard to the question which has arisen as to the originator of the idea of giving a spring which is employed to resist the centrifugal force of the governor balls such an initial tension that its subsequent increase of resistance shall be in a ratio equal, or nearly equal, to the ratio of the increase of centrifugal force, it is no more than fair to all parties to say that in Mr. Porter's patent, granted June 18, 1861, he distinctly claims this invention, which is *prima-facie* evidence that he was the original inventor. That Mr. Silber had the idea, or even understood the principle involved, I have no direct evidence, but that the apparatus shown (Fig. 98), and which was published several years before Mr. Porter claims to have made the invention, is capable of being used in that way is certain, as is admitted by Mr. Porter. Moreover, the description accompanying the said illustration says: "The tension of spring *H* is increased or diminished at pleasure by turning knob *J*, which moves the claw collar *K* out or in, thus rendering governor more or less *sensitive* as desired." This would seem to indicate, but it does not *prove*, that Mr. Silber understood the principle involved, otherwise he would have been likely to say it would increase or diminish the *speed.* The following letter from Mr. Porter may give further light upon the subject. I should be sorry to discredit without good reason his priority in this matter, and I make free to state, knowing personally all the parties, that the fine mathematical reasoning involved is most in accord with Mr. Porter's mode of thinking:

SCHENECTADY, N. Y., *January* 9, 1888.

MY DEAR MR. BABCOCK:

I fancy you gave Silber credit for something he never thought of, namely, an isochronous centrifugal governor. We must put ourselves back to the year 1855. A high-speed governor had not then been made; strong counteracting forces, enabling a governor to develop power to operate a valve on a minute change of velocity, were unknown. The influence of friction on a governor, and the means of avoiding it, had not been thought of.

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* Added since the meeting.

The governor shown in *Scientific American*, it is evident from the spring represented, was a feeble one, revolving at the ordinary slow speed. A variation of from 10 per cent. to 15 per cent. in the speed of the engine was undoubtedly required to cause the governor to move through its range of action. On less than this it would not operate the large throttle valve without oscillation. As then commonly constructed, governors required this amount of variation, and often more than this. That amount of variation would not be observed at all on those paddle-wheel engines.

The adjustment applied to the spring enabled the sensibility of the governor to be increased, but this could only have been done in a slight degree. Too much sensibility meant loss of stability and oscillation. When we talk about sensibility of governors nowadays, we mean something nobody at that time dreamed of. The fact that Silber very soon abandoned that form of governor shows that he had no idea of its possibilities, *which high speed was afterward to reveal.*

On full reflection, I think my claim to have first stated the principle of isochronism in governors in which a spring is employed to resist centrifugal force, and to have first constructed governors on this principle, in which strong counteracting forces are employed, and so a close approach to isochronism is made practicable and actually realized, is well founded.

Very truly yours,

CHAS. T. PORTER.

Mr. W. R. Warner.—I was in an engine shop some few years ago, and they made a statement to me of a fact which I at once stated I thought to be impossible. They showed me an engine which had been sold to a saw-mill, and it was governed to run with maximum load 194 turns a minute. They made the statement to me that with the entire load thrown off, it would run 192 revolutions per minute, just the opposite of what I supposed an engine would do when the power was taken off, and to prove it to me, we took a watch, a counter, and counted it correctly. The speed was 194 revolutions with the full load on. When the entire load was thrown off, as he stated, it was 192. Now, I would like to ask Mr. Ball if his system of governing is so correct that he can over-correct it—so to speak—and make his engine run slower without any load, and if so, then is it easy to find the mean position, and make it exactly the same with full load or no load.

Mr. Ball.—I hesitated a little about presenting this paper, because I felt it was a little in the nature of advertising one's goods and wares; but inasmuch as the society has shown some interest in the matter, I am very glad to answer the gentlemen who have spoken.

The Huntoon governor has been mentioned as being an isochronous governor. Those which I have seen have the chain which holds the weight wound on a scroll. The fact of its being wound

on a scroll makes an increasing resistance as the governor changes its position, requiring necessarily a change of speed to move the governor on to another position, so that the governor is made to produce just the same effect as is ordinarily produced in centrifugal governors.

In answering Prof. Denton's question as to why these isochronous governors are not used, I would say that it has been my observation that while governors are sometimes adjusted to be very nearly isochronous, the danger of instability is so great that it has been ordinarily impracticable to use them. I have heard of governors that have been over-adjusted, as Mr. Warner speaks of, so that the engine would run faster when it had its load on; but with a governor of that kind it would be almost impossible for the weights to take any intermediate position. The only way that I can imagine the centrifugal weights of an isochronous governor (without any dash-pot and spring, such as I have described,) to be stopped in an intermediate position, would be if the weights move so slowly that the speed of the engine should change before they reach an extreme position, and therefore stop, then, in an intermediate position. Otherwise if there was an excess of centrifugal force in one position, the same excess must exist when the weights reach their extreme position, because the two forces are exactly in harmony.

I want to say one word about the friction of the governor, of which Mr. Kerr has spoken. There is no doubt, whatever, that friction is, generally, an enemy to all governors. The question of friction, however, has not very much to do with the use of this dash-pot and spring. Of course a governor without friction would be expected to give finer and better results than one with friction, although there are cases where the friction of the governor is overcome by the resistance of the valve. The effort of the valve to move the governor from one position to another, often overcomes the friction; so, if there is the slightest tendency to move, that movement takes place. As a rule, however, friction is a serious obstacle to close governing. But after we have eliminated all the friction possible, and obtained the finest possible adjustment, then the question arises, Is it possible to have the governor do any better? and in saying it is good enough for all practical purposes, I am reminded of a civil engineer named Lovejoy, who used to live in Buffalo. In some of his work he was rather careless, and when spoken to on the subject, always said that it was near enough for all practical purposes. I remember seeing his picture, and underneath

the name was inscribed, "The engineer who surveys near enough for all practical purposes."

Engineering is a pretty exact science, and, as mechanical engineers, I think we ought to be careful about saying anything is "good enough for all practical purposes" unless it is pretty near right, and unless a better result can only be obtained by objectionable complication and expense.

Mr. Kerr criticises the dash-pot and spring because it does not anticipate the movement of the weights. He implies, also, that the device might make the governor more tardy to act. This is not the case, because the interposition of the spring allows absolute freedom of the weights to begin motion in either direction.

Let us suppose it possible to adjust the ordinary governor safely to a 2% variation of speed. Then, if the main springs are adjusted so as to make the governor isochronous, the dash-pot spring should offer just sufficient resistance to make a 2% variation of speed if the dash-pot piston remained immovable, and the governor would then act in all respects like the 2% governor without dash-pot. If, however, the piston moves through the fluid just to that extent, it wipes out the 2% variation, without any danger of instability.

CCLXXXII.

TOPICAL DISCUSSIONS AND INTERCHANGE OF DATA.

(XVIth Meeting).

No. 282.—50 and 51.

Have you used driven wells successfully; of what sizes and depths, and singly, or in groups?

What is the best form of pump to use with driven wells, where the lift is ten to twenty feet and air is likely to get into the suction? Should the pump be single or duplex, and with piston or plunger?

Mr. J. G. Briggs.—This method of obtaining a supply seems to be coming into general notice. In my opinion its success depends entirely upon the locality. This section of country (Terre Haute, Ind.,) for miles has a splendid bed of water-bearing gravel underlying it at a depth from the surface of from twenty-five to forty feet, about on a level with the bed of the Wabash river. It appears to me that the point in Query 51 in regard to "air getting into suction pipes" is not necessary to be discussed. There is no reason why air should get into the suction pipe, if the wells are properly driven and connected. At our pumping station we can draw over 2,500 gallons per minute from 10 eight-inch wells 24 feet deep. The pumps stand idle for weeks at a time, and we have no trouble with air. They are for convenience driven in what was formerly the bed of the river, twelve feet apart, and are connected together and to the pumps by a twelve-inch cast-iron pipe and a tee of my own design (Fig. 91). The wells are put down to the required depth, the tees screwed on, and are connected by ordinary water pipe, with a sleeve joint. The cap on top can be taken off for examination and for sand pump-

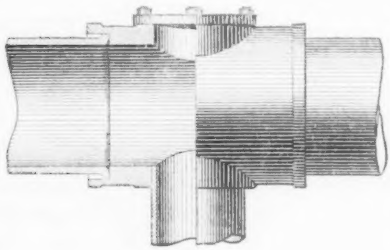


Fig. 91

ing, if necessary. I adopted this plan, as the expense for large sizes is much less, and of course they are more durable than wrought pipe. Although so close to the river, we only get shore water, cool, clear, bright, and—hard, very hard. We commenced using them a year ago during a heated term. Every one at first was delighted. The papers complimented us on our unusual thoughtfulness and liberality. It was nice to draw a glass of cool water from a faucet, after, of course, letting a bucketful run. But after the first Sunday the steam users began to growl. Then private families. It was hard to bathe in, and burned out the water-back in the stove. The barbers could not make a good lather, and, finally, the barkeepers, who had been its warmest advocates, began to find fault. They had to put soda in to wash their glasses. The citizens never appreciated the beauty of straight Wabash water before. We only use the wells now in case of an emergency, or when the river is very muddy. A mile below, the distillery obtained nearly one million gallons from 3 eight-inch wells sunk on the same level. The Ellsworth paper mill, five miles from here, obtains half as much more from 6 eight-inch wells. The mill is situated about 300 yards from Otter Creek (a small river), which ceased running in July, for the first time in years, leaving large, deep pools of water, the favorite resort of blackbass, which, to the great disgust of the local fishermen, are now no more. For when the mill started in September the water was entirely drawn from the pools by means of the driven wells. Without any rain since, the product of the wells is constantly increasing, opening new channels, and drawing the water from a long distance, as is shown by the wells going dry in the vicinity. This demonstrates the danger of using water from wells in any large town or city.

In regard to the kind of pump best adapted to this work, I cannot see that the mode of supply cuts any figure, with one exception. It takes a long time, sometimes years, to get the fine sand all out. This sand cuts a piston pump very badly. For that reason I prefer, on a new well, to use a plunger pump. On three different sets of wells, under my observation, there are used a Dayton duplex pump, a Clapp & Jones water-packed, and a Hooker-Colville inside-packed plunger, all having the same relative efficiency as if draughting from an open well. All driven wells should have a large vacuum chamber between the wells and the pump, and a vacuum gauge on top, by which the speed should

be regulated. It is as necessary as a steam gauge to a boiler, unless there is a very large margin of supply. You cannot look into your wells, or sound them by any other method. It is very severe on suction valves when the lift is too high by this method. I would not allow over 24 inches under any circumstances, and would, if practicable, put in more wells if the gauge shows over 20 inches. Steam pump manufacturers are not partial to using their pumps on this mode of supply as a general rule, for many a first-class pump has come to grief by trying to draw more water than there was in the wells. I read an article lately showing a disposition of wells in such a manner that the pump would draw equally from each, which is a good idea, for when they are in a line the draught is hardest on the first ones. For this section, or where there is a good supply of underground water, I prefer 6-inch or 8-inch wells. To any person contemplating using 8-inch wells, I would be pleased to give the use of my tee patterns if desired.

Mr. C. J. H. Woodbury.—The question of the lift of pumps asked in the second interrogatory is merely what would be similarly asked in the case of any pump having a lift of ten or twenty feet, except that in the case of the driven wells the frictional head is increased by the small diameter of pipe generally used for such purposes, and also by the interstitial friction of the water passing through the earth to the bottom of these wells. A short time ago I made simultaneous experiments with three pumps of different types, all of the same manufacture, all new, with the same steam pressure and the same distance above the water, to determine which of the three forms was the best for a certain draught. The steam cylinders were fourteen inches in diameter, the water cylinders seven inches, and the stroke twelve inches. They were all made by the Knowles Steam Pump Company, at Warren, Mass. The single acting pump readily drew its supply of water from the source, which was fourteen feet below the center of the pump. The duplex piston pump drew its supply with some difficulty, and the outside plunger pump would not draught until it was artificially primed. In other matters than that, I have always found that a single acting pump can draught water more successfully than any other type.

On the first interrogatory, I would say that I have had some experience with driven and tubular wells, and have found that the draught in connection with such wells is usually excessive, and can best be managed by a single acting pump. At Brooklyn, the

Knowles pumps at the Clear Stream Station, draw about thirty feet by the gauge.

From the Deane Steam Pump Co., I learn the following respecting the draughts of some of their pumps, which I give as examples of what draughts a single acting pump may use:

	DRAUGHT.	PIPE.
Louisville & Nashville R. R. Co.	20 feet	
Canada Cotton Mfg. Co.	26 "	300 feet.
Penn Salt Co.	20 "	2,000 "
Brayton Engine Co. (2" pipe)	12 "	1,900 "
Vanderbilt University	26 "	765 "

At Lynn there are twenty-eight wells from twenty-five to sixty-five feet in depth, on which the draught to the Knowles pump by gauge amounts to twenty-seven feet, at times when the draught is only about twenty feet by measurement. As in the case cited in the discussion just presented, the water has fallen in those wells since they were first used, and the supply diminished, showing that the natural water supply had been taxed. In September, 1880, the supply furnished by these wells per day was 700,000 gallons. In the winter of 1881 it had been reduced to 450,000 gallons, and in 1883 to 300,000 gallons, and dug wells had been exhausted at varying distances from the tubular wells according to the direction of the sources of water supply. In one instance a well was at a distance of 4,000 feet. Some of the difficulties from the driven wells have been the contamination by surface water; another difficulty has been from the grit getting into the pumps, which is not removed even by the sand chamber, and also by the sand packing around the perforations at the bottom of the tubes, forming a compact mass impermeable to water, and furthermore the quality of the water is sometimes injured by a fungoid growth.

Prof. F. R. Hutton.—I might state that one of the members of the society was in its office a week or so ago, and was speaking about the driven-well practice in Cuba. He said that it would be a great help in the sugar-house work done in Cuba if it could be made possible to run driven wells through clay formations; that wherever they can strike gravel, the difficulty is a very simple one; but it becomes practically insurmountable when they come into the clay which underlies a great deal of the sugar plantation areas of Cuba, and that he would be very glad if any of the members of the society could make such suggestions in the discussion of this inquiry as would lead to helping him out of the difficulty

from the bending of even the best and heaviest pipes in attempting to drive them through close clay.

Mr. Woodbury.—There are two classes of difficulties arising from the use of tubular wells in clay, the one being that the water is forced up into the pump by the atmospheric pressure in a tubular well in accordance with the same physical laws as in any other well, and if it strikes an underground source of water in such a manner that the force of the atmosphere cannot be applied upon it when the pump is operated, the result will not be satisfactory. That has been obviated by sinking other wells in the vicinity to obtain the pressure of the air upon the source of water supply. For borings in clay a very convenient and easy method is by the water drill, which consists of a tube roughly closed at the end by a hammering, so that the water, as ejected from it, will form a sheet. A larger guide pipe is first driven a foot or so in the earth and this other pipe pressed into it, and connected with some source of water under pressure usually furnished by a pump through a cross pipe at the top connected by a T, and the inner pipe is slowly worked back and forth, the water passing down through that against the clay or silt, or any fine material, which is disintegrated, comes out in the annular space between the two pipes, and in this manner wells are bored any reasonable distance up to 150 feet. That is what is sometimes known as the Robinson process of sinking wells.

Mr. Jas. McBride.—I would state that about six years ago I had occasion to put down a driven well, and got an ample supply of water from it. I think it was a two or a two-and-a-half-inch tube, and this supply lasted for a long time. Afterward we excavated for about one hundred feet around the well, taking off six or seven feet of earth for the cellar of a new building; we left the pipe standing where it was put down. This cellar floor was covered with six inches of concrete, and over this an inch and a half of asphalt. Afterward when we attempted to get water from the well we failed to get any. I could not account for it, so I reversed the action of the pump and forced the water back into the earth, thinking thus to clear out the pipe. The result was, that the water was forced up around the column foundations in the cellar. We have been unable to get any supply of water from it since. I think that this covering of the cellar floor would go to show that a supply of air had been cut off from the surface of the water.

Mr. W. M. Barr.—In regard to sinking wells through clay, I have had occasion quite recently to sink a well in Philadelphia, in which we found it impossible to drive a large pipe through a tough clay, but we succeeded in getting through a bed some fourteen or fifteen feet thick by using the ordinary drill such as is used for drilling rock, proceeding through the clay in precisely the same way that we did through the soft rock underneath.

Mr. Oberlin Smith.—I would like to ask some of the members who know about drive-wells whether they are ever screwed in after the manner of a "screw-pile"—or like a corkscrew (if any of the members know what that is)—instead of being driven right down. It seems to me that this might perhaps be done—even in clay.

Mr. Geo. H. Babcock.—There is a driven well in the city of Plainfield, N. J., worthy of note. Nearly all the water in that city is obtained by driven wells. The conditions are very favorable. The soil is mainly gravel and sand, and quite porous to surface water. Below 15 to 20 feet it is saturated with water down to the bed rock, from 60 to 100 feet, this saturated portion being alternate layers of sand and gravel. This water has a constant flow to the south or southeast. The well I speak of is driven right through the bottom of a cesspool, and the water is pure and good! It belongs to the mayor of the city, and was an experiment to prove that surface water does not affect the wells. The cesspool, which is built of loose stone with gravel bottom, has been in use seven years. Five years ago this well was driven twenty-eight feet deep through the bottom, the cess-pool being pumped out for that purpose. The water has been used in the kitchen part of the time, but mostly for watering the grounds. Several times, and as late as last summer, it has been analyzed and absolutely no contamination is found from the proximity of the cess-pool, and no filtration down beside the pipe.

No. 282.—52.

Are roller bushings expedient in journals at low velocities and under high pressures?

Mr. W. R. Warner.—I thought it might be of interest to you to learn of a problem that the firm with which I am connected have tried to solve in the past year. It was in connection with

the work on the Lick telescope. The tube weighed four and a half tons. It was fifty-six feet long. As you know, in such constructions, the tube must be supported from one side, that is, it cannot be held on trunnions, but it must be supported near the center and from one side. It is necessary for this tube to work very easily. In fact, the friction, as far as possible, must be removed, as the tube must move very smoothly in following the star at which the observer is looking. It seemed almost impossible to make it move easily enough in the ordinary way by using friction rolls. So instead of that we used the method of surrounding the axis close to the tube with a series of rolls two and a half inches in diameter and three inches long, with a result which seemed very satisfactory. This tube, weighing four and a half tons, when balanced on these rolls, would turn by a pressure of four pounds at the end—one finger would move it very easily; so that the problem was as completely solved as could be asked.

Another effort to solve a similar problem in a different position where the rollers hardly would do—for it was the end-thrust on a heavy weight—was accomplished by using hardened steel balls running in circular concave tracks which is the same principle used in bicycle wheels. In this problem, simply to test its working, we placed a weight of two and a half tons on forty one-inch balls in the two circular tracks, and this two and a half tons was turned by a pressure of one pound at a radius of three feet. That hardly could be criticised, and we find that about a satisfactory proportion, and we could depend on it every time. Almost all of the friction in these cases was reduced to rolling friction instead of sliding friction.

I give the society the benefit of these two experiments, as they seem to be a little out of the common line of experience. The $4\frac{1}{2}$ tons was moved by 4 pounds, the $2\frac{1}{2}$ tons by 1 pound.

Mr. O. C. Woolson.—I would like to ask if these balls were in a certain annular space, or whether they were separated and kept separate, and what the diameter was?

Mr. Warner.—The diameter of these balls was one inch. They followed the ring in which they were running, the diameter being an inch. The groove in which they ran was $\frac{1}{2}$ plus $\frac{1}{32}$ inch radius, so that it was practically a plane surface, bearing only on the top and lower edge, and the balls worked together so that the whole ring, when they were pressed together, left only $\frac{1}{32}$ of an

inch between the last two balls. In the case of the rolls, they were not together, but had their axis run on little steel balls $\frac{4}{10}$ of an inch in diameter, so the $2\frac{1}{2}$ inch rolls were held in a live ring so that their surfaces did not touch each other. There was no lubricant; everything ran dry. The best result was with the balls. I would state, however, in reference to the amount of weight allowed, that greater weight could be put on the rolls. We found it safe to put on the balls something less than a thousand pounds to each ball, while on a roll having its bearing surface its full length—3 inches, of course—we could put a much larger weight.

No. 282.—53.

What is the best material for lining brake-straps on elevators, cranes, etc.?

Mr. E. D. Leavitt, Jr.—We find basswood or poplar very good. We have tried at the Calumet and Hecla mines various materials, and invariably the harder woods have failed to give good satisfaction. I should suppose that the same law would obtain in regard to this question. In fact we have run small brake-straps, and the softer woods, as I said, have given the best results.

Mr. A. H. Raynal.—I should like to ask Mr. Leavitt if he has ever tried fibrous vulcanite?

Mr. Leavitt.—Yes, sir, we have. At the time we were having a great deal of trouble with the wood on our brake-straps some vulcanized fiber was sent us for trial, and it proved to be worse than anything we ever tried. It was a good deal like the case of a friend of mine who wanted to send us a stamp shoe. We usually wear out a stamp shoe weighing 800 pounds in about six days. This gentleman thought he could send us one that would last six months. We told him he might do it at his own expense. He did so, and his stamp shoe lasted thirty hours.

Mr. E. F. C. Davis.—I would like to ask Mr. Leavitt whether he finds it better to line his brake-straps at all; that is, whether it is better to have the wood material on the drum, or have the strap lined with wood, running over a turned surface of cast-iron on the drum?

Mr. Leavitt.—We do not put the wood on the brake straps, but on the drum itself, on account of the greater convenience in changing. The straps are 8 inches wide. Our wood lining is 4 inches thick, and at the end of a month we have worn it down sufficiently to necessitate its removal. Another thing which increases the wear, probably, is that we have to use water. Constantly running down from 3,000 to 4,000 feet, the brake-straps would get so hot as to set the wood afire if water were not used. No doubt that adds very largely to the wear. We do not use the wood on end. It is more convenient for us to saw out the soft wood to correct shape with the grain, and I do not know that we have ever tried using the wood on end to any large extent. We have made up our minds, after long experience, that we must change at certain small intervals, and consequently use the plan which is easiest.

Mr. J. H. Cooper.—I have used white ash and maple with good results, such that I had no reason to experiment for better material; end grain was put to the rubbing surface wherever practicable. I have not tried softer woods for comparison, but followed the practice of the old millwrights, who according to my knowing always use the harder and denser woods where pressure and wear are to be resisted. For example: best hickory for inserted cogs, maple, apple, birch and beech for slides and slip-ways, and lignum-vitæ for heaviest friction pressure.

From the article on "Frictional Gearing," by E. S. Wicklin, in "Use of Belting," I take the following: "For driving light machinery running at high speed, basswood has been found to possess good qualities, having considerable durability and being unsurpassed in the smoothness and softness of its movement.

Cottonwood has been tried, but is more affected by atmospheric changes. And even white pine makes a driving surface which is, considering the softness of the wood, of astonishing efficiency and durability. But for all heavy work, where from 20 to 60 H.P. is transmitted by a single contact, soft maple has, at present, no rival.

Driving pulleys of this wood, if correctly proportioned and well built, will run for years with no perceptible wear."

We must distinguish, however, between the cases where the surfaces engage and where the slipping is complete.

No. 282—54.

What is the best way to secure tight fit of set-screws tapped into heavy parts of a machine ?

Mr. J. H. Cooper.—If this means the fit of the set-screw in the tapped hole, and if it don't fit tight enough, the remedy is, make a better fit. But if it means that the screw becomes loose by reason of the strains and workings upon it, then hold it fast by a jam-nut, as is done with other bolts. But set-screws frequently become inoperative from causes foreign to themselves; a loose fit of a collar or pulley hub may defeat the holding of a set-screw. To remedy this cut the metal away in the bore opposite the screw for about 60° as shown in Fig. 92. This will give the hub two lines of bearing instead of one and prevent the twisting action which contributes much toward the loosening of the set-screw.

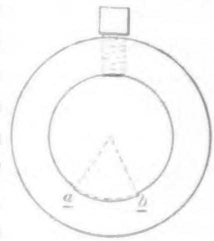


Fig. 92

Mr. H. R. Towne.—I apprehend that the question would cover this point—that the variations in machine screws, as we get them from the makers, are considerable; the variations in holes tapped are also considerable, depending on the wear of the tap, and it is the sum of these errors which makes loose fits in set screws. It is often a question how best to obtain a very snug, tight-fitting screw on machinery running at high speed, which shall not work loose. In my own experience I have had occasion to deal with this question, and I find there is no way—at least I have not found any practical way—better than this (I wish I might)—to have the screws made a little above gauge and put them through a sizing die after purchasing them; and even then, on work where it is important to have the screws absolutely tight, to use only quite new taps and screws which had been gauged to size in this way. The working loose of a set-screw, on a piece of machinery running at high speed, is a very serious matter, and it is one of those little matters of shop practice which sometimes determine whether or not a machine is satisfactory in use. I wish some member present who has had larger experience in this matter could suggest some good way of securing uniformity in such screws.

Mr. L. F. Lyne.—I have been troubled a great deal with

dynamo machines in having the set-screws work loose, and the way I managed to keep them tight is to get my set-screws first. I run the taper taps in so as to make a tight fit on at least three or four threads. Since doing that I have not been troubled by their getting loose.

Mr. Hugo Bilgram.—There are cases in which the method just suggested may be objectionable. I have had some experience lately in tapping a large number of holes in cast iron, into which brass screws had to be fitted very closely. They had to fit gas-tight, and yet turn freely. I found it impossible to keep the taps to size. The only way to meet the emergency was to fit each screw separately. Of course this can be done systematically by keeping the pieces in the order in which they were tapped, to be assured that none of those succeeding were ever larger than the preceding ones of the series. The screws having been made of slightly varying sizes, they could then readily be selected to suit the tapped holes.

Mr. L. G. Engel.—I think the place in which the most trouble occurs with set-screws is usually in pulleys. Nearly all the makers of pulleys persist in putting only two set-screws in the hub, and it seems to me that should be changed, at least for large pulleys. The standard thing is, I believe, to put two set-screws in. I invariably have four put in, two on the feather and two one-quarter way round the shaft, except when the latter becomes impossible by reason of the pulley being split. Then again there are some instances in which a pulley will crawl along a shaft, and it is almost impossible to keep it in place. In that case I turn the set-screw off at the end and then bore the shaft and drive the set-screw into the shaft. I must confess, however, that I can recall very few driven into the shaft, apparently good fits, in large whole pulleys, which have not at some time worked loose.

Mr. W. H. Doane.—I would say that in machinery of high velocity we have had some experience of this matter of set-screws. The best thing that we have found, assuming that the screw is a perfect screw in a proper hole, is to get the bottom of the set-screw so that it bites pretty sharp, and then we get pretty good results. There are many places where it is desirable, as one of the gentlemen has said, to sink the point of the set-screw into the shaft a little bit. I cannot tell you why it is, but it seems to retain the set-screw in position. It does not seem to work back. But when we drive at 5,000 or 6,000 revolutions a minute, the

only way by which we can hold the pulleys on the shaft is by cupping the lower end of the set-screw or by placing a small key nut—the set-screw just hollowed out to fit the shape, and the set-screw to go down on that. That makes a very tight fit.

Mr. W. F. Mattes.—Sometimes we get caught in an emergency away from home, and have to do things that are not exactly mechanical. I have been annoyed at times by the brass stems of pump valves working loose. The stems were of small diameter—probably an inch and a quarter—and were screwed about two diameters into brass seats. Of course the fitting was imperfect, for after they came out I could screw them back easily with my finger. In such cases I found a remedy by simply blunting the edges of the thread gently with the hammer. A very slight treatment that way cures the evil every time.

Mr. Engel.—I would like to say that another plan with large valves is to put a lock nut on the other side.

Mr. Mattes.—Perhaps you cannot get at the under side of the plate.

Mr. Engel.—Possibly. Referring to cupped screws, they are *necessarily* of hardened steel, which some object to (although I use them myself) because it is difficult to drill them out if they break. The cup screw is not, I think, advisable in a place where you wish to sink the screw into the shaft. It is better then to turn an iron or machinery-steel screw where it enters the shaft slightly conical—at least conical at the end, so that it will nearly fit the hole made by an ordinary drill of the standard size.

Mr. Geo. E. Whitehead.—I find that the principal wear on the taps is on the top of the thread, and most of the fits that are made have a bearing only at the top of the thread. If you make the screws so as not to bear at the top, but have the friction come on the angle of the thread, and make them two or three thousandths over size, I do not think they will come out very easily.

Mr. O. C. Woolson.—I want to say this regarding set-screws, that it is not an uncommon thing to find steel set-screws which have a little nick in the point, and being hardened they act as a very nice tap, and no matter how nice your hole is made originally, you screw in this set-screw with a good wrench, getting a stiff pressure on it from a good strong mechanic, and he will re-tap that hole very perfectly; but at the same time he has made it so large that the set-screw will never stay there, or any other standard set-screw. The fact of having that nick there did the whole busi-

ness, and you get "left," as the saying is. Every set-screw should be cupped, but many screws necessary to be used as such are not cupped, but it is not a difficult job to chip a countersink in the point even if a drill or drill-press is not at hand. Should the screw be hardened, of course it is necessary to draw it a little on the point only.

Mr. Lyne.—The gentlemen have touched upon the cup-pointed steel set-screw, and so far it seems to be condemned. We have used a great many half-inch steel set-screws to hold pulleys and armatures. I have found that by setting down a cup set-screw the vibration of the pulley or the armature upon the shaft will gradually form a round or spherical shape inside the cup point of the set-screw, making it loose and splitting it. Consequently such a screw is worthless. Now to overcome this difficulty I have taken the half-inch set-screws and ground off the knife-edge so as to leave a thirty-second of an inch flat surface; then set that down on a corresponding flat surface on the shaft, and it does not come loose. I am substituting as fast as I can $\frac{5}{8}$ set-screws for $\frac{1}{2}$ inch, preparing them as described by grinding off the sharp knife-edge. I have not had any of those work loose. To guard further against any such contingency, I put jam-nuts on the set-screws.

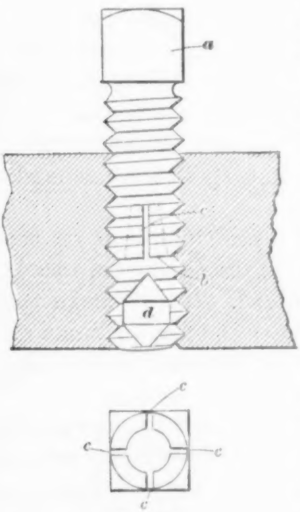


Fig. 93

Mr. Arthur Falkenau.—In a case in which I had considerable trouble in keeping a large set-screw tight, I used the following device (Fig. 93). I took a steel set-screw *a*, drilled a little ways into it as shown at *b*, and made four slits *c* with the hacksaw. A double conical piece of steel *d* was dropped into the tapped hole, and the set-screw run down on that, so that the pressure spread the end of the set-screw, and thus tightened it in the hole. I found that this answered very well.

Mr. James McBride.—Since the discussion has turned on the holding of pulleys with set-screws, I will say that I have had considerable difficulty with set-screws, in holding pulleys. We had some hundreds of pulleys in our establishment when I took charge

of it some years ago, which were invariably fastened with set-screws. Some of the screws had cut a groove two-thirds of the way around the shaft; I simply abandoned the use of set-screws entirely, and had all my pulleys keyed. The pulleys are all sizes, from six inches to ten feet in diameter. It costs a little more at first to do it in this way, but it pays in years, and you never have to do it but once.

No. 282-55.

"What kind of pig iron gives the best results in light castings where easy tool-treatment is the essential rather than strength?"

Mr. H. R. Towne.—I am responsible for this question, and perhaps it will contribute to bring out some discussion on it if I state it a little more fully. In some kinds of the smaller machine products there is so much machining to be done that a very soft metal is important. I made some tests of different specimens of cast iron with reference to this a little while ago, and my lowest figure for the number of seconds required to drill a given depth with a given drill was 47, the piece of metal experimented with being a soft gray iron very carefully annealed. I got at approximate results with other annealed castings, rising from 50 to 75 seconds under different conditions. In the endeavor to accomplish approximately equal results with unannealed castings I failed to get anything, as I now remember, below 150, and very commonly got 200, and in some cases 300. Ordinary machinery iron would have probably run nearer 500. These figures indicate that there is a very wide range indeed in the possibilities of softness of gray iron castings. At present, we are dependent for the best conditions on the use of the annealing furnace.

If any others have made experiments in this direction it would be interesting to know what mixtures of iron or methods of treatment are found to produce this quality of extreme softness without resort to the annealing furnace to secure it.

I may mention in this connection that during a visit to England last summer I was at the Carron Iron Works—one of the oldest in Scotland, where they make a specialty of producing very soft metal and making very fine castings. They had some very beautiful exhibits there of art-work cast from their metal, and among other things surprised many of the visitors by showing a

piece of thin cast iron, perhaps $\frac{1}{16}$ of an inch thick, with holes punched in it by an ordinary punching press, the holes being in some cases not more than $\frac{1}{8}$ of an inch in diameter, and showing a perfectly sharp edge on the upper side, the same as we get from a piece of steel.

Mr. O. C. Woolson.—That last remark of Mr. Towne's is a very interesting one. So far as doing away with the annealing furnace is concerned, I am inclined to think that it is impossible to do it under the present conditions of molding, and the different mechanics that we are obliged to have. But where there is a great deal of work being turned out of a light character having thin flanges, etc., it is impossible, no matter what the mixture may be in the furnace, to do away with case-hardened spots, and chills, and as to working those without annealing, it seems to me it is impossible to do it. Still, having in mind Mr. Towne's last remark, it would lead me to think that there is some room for science there yet, but I have never seen it done.

Mr. E. F. C. Davis.—I am under the impression that considerable could be effected in the way of making iron soft by treating it in the cupola. I have gone so far as to think that there must be a profitable field for research in that direction, and I would be very glad if some one could give us the benefit of experience in that line. I think the character of the blast has quite an effect in making the iron softer, or harder.

The President.—I had hoped to have some information for the society upon this question and the one in regard to the use of molding machines, which has been deferred. Some members of our society are connected with the Singer Manufacturing Company, which has extensive experience in making light castings and working them with milling machinery, and also with molding machines in the foundry, but they did not respond to my request for discussion on the subject. I have been told that they are obliged to use one particular brand of iron for light castings. They say that they have quite a number of times tried other brands with the hope of reducing the cost, but in every case the increased cost of tools for working has very largely exceeded the decrease of the cost in the material. Consequently they have gone back to the original brand. I am sorry I cannot tell you what that brand is. I do not know that it would do you any good, for I understand they take the entire output of the furnace.

Mr. L. F. Lyne.—I have seen specimens of the Salisbury iron

of Connecticut, so tough after castings were made that it could be forged. The blast, the fuel, and all those things have a great deal to do with it; but my opinion is that you have got to put the right mixture of metal into the cupola in order to get it out, and I do not believe that you can get a good, nice, smooth casting without pure material; you have got to make a proper mixture. I am sorry that I do not see a gentleman present who is posted on that subject. I expected to meet him here, but he is absent. He could give us valuable information in regard to making soft castings sound and very strong.

Mr. W. F. Mattes.—With reference to the tendency of castings to chill in the mold, we might perhaps take note of the experience of car-wheel men. I have had considerable experience in making charcoal iron for car-wheel purposes, and we found that iron to chill well required to be low in silicon, and if it were a little high in manganese so much the better. We know that ores high in manganese are with great difficulty reduced to a very rich gray iron, although recently I have known an experimental case where irons had been produced containing 2% of manganese, which were richly graphitic. This is higher than anything I have known to be obtained before. But we also know that irons very low in silicon will chill in car-wheel chill-molds. They also show a tendency toward chilling in green sand. Therefore, if I were to select a foundry mixture for this purpose, I would have it low in silicon and high in manganese.

No. 282.—56.

What is the effect of adding small per cents of wrought-iron or steel scrap in the foundry cupola or ladle?

Mr. Chas. H. Morgan.—I have nothing to say about small per cents; but I have had a little experience with a percentage of steel scrap as high as from 27 to 33 per cent., having made some castings for gears of about 3 inches pitch, from 2 to 7 feet in diameter, and the mixture which was used was 55½ per cent. of No. 2 Franklin pig iron. This Franklin iron is made by a blast furnace near Utica; some 17 per cent. of Salisbury pig iron—No. 2—and 27½ per cent. of Bessemer steel scrap. We had test pieces made which were 1½ inches in diameter and

about 10 inches long, and they were turned down to a size which would give us a square inch of area, and the test pieces were broken in an Emery testing machine. Of the two samples we tested, one gave 32,180 pounds and the other 32,150 pounds breaking weight. The castings turned well, the teeth of the gears were cut with no difficulty, and have worn most excellently for about one year. Last spring, in visiting the Pennsylvania Steel Company's works at Steelton, I asked the superintendent, Mr. F. W. Wood, if he had had any experience in using steel scrap, and he called my attention to castings an inch and a half thick in which I think he said there were about 33 per cent. When I saw the question which was to come up for discussion this evening, I wrote to him; and his assistant, Mr. E. C. Felton, gave me these facts, saying that they were making their rolls, using as high as fifty per cent. Bessemer crop-ends with fifty per cent. of their regular Bessemer pig, and having excellent results. He brought down some test pieces, of which I have a sample here, if any one would like to look at the fracture; and these test pieces gave very nearly 30,000 pounds to the square inch. Some one might be interested in the analysis. Mr. Felton gave me this for the analysis: Graphitic carbon, 2.69 per cent.; combined carbon, 0.4; silicon, 1.13; manganese, .44; phosphorus, .07; sulphur, .03 to .05. I give you these facts hoping that it may bring out some one else's experience in using a pretty large amount of steel scrap in the cupola. Perhaps some one might be interested to know how this steel scrap was melted down, for I have known some people who have made a failure of it. The foundryman who made these castings for me got his cupola hot before introducing the steel scrap. Then he put in his charge of steel scrap, and then his percentage of pig iron on top of the steel scrap, with a little more coal on top, and so went on. He considered it very important that his furnace should be quite hot before introducing the steel scrap.

Mr. H. R. Towne.—I have also been using a cast iron with Bessemer scrap in it, and as so many remarks have been offered which are of interest in this connection, I will add this to them. I have experimented with various percentages of scrap from 10 up to 50, and have found that the best results usually lie somewhere between 10 and 25 per cent. When using a larger percentage of the Bessemer scrap, I failed to get any increase, in the tensile strength at least. I have here the results of tests in two of these cases, in one of which there was 14 per cent. of the Bessemer

metal, the tensile strength being 33,000 pounds per square inch, and another in which there was 20 per cent., the strength being 38,000 pounds.

Mr. W. F. Mattes.—I would like to ask Mr. Morgan how this mixed metal compares in pouring in the mold with ordinary castings.

Mr. Morgan.—I cannot say as to the fluidity of the metal, as I was not present at the pouring of the castings; but I found the castings very sound and free from blow-holes.

Mr. E. D. Leavitt, Jr.—I should like to ask if the fracture of the casting shows a homogeneous structure; in other words whether the steel was sandwiched like a Washington pie or whether it was entirely invisible as steel.

Mr. Morgan.—I would say in answer, that the fracture was uniform.

Mr. A. H. Raynal.—At the De Lamater Iron Works, the addition of wrought-iron scrap to cast iron has been practiced for many years, when extra strong castings were required. The wrought iron is placed in the ladles in pieces not too small, and the iron from the cupola poured over it without stirring. From 5 to 20 per cent. has been used in that manner, and I have tested bars which showed an increase in strength by this method of nearly 25 per cent.

Mr. Wm. Kent.—I would like to ask if any advantage has been found in the use of rusty scrap or scale of iron in that connection. I think from theoretical reasons some advantage would be found from rusty scrap in small pieces, the oxygen of the rust or scale tending to burn out some of the objectionable silicon in the pig.

Mr. Towne.—I cannot answer the last question except to say that all the scrap that is usually cast into the cupola is more or less rusty. The metal, as we have used it, has been rather sluggish—not as fluid as softer irons. We have found some tendency to develop blow-holes in the castings. It requires pretty careful pouring, at just the right time, and often there is just a little uncertainty in the result—blow-holes which do not show on the surface at all. That I think is a difficulty which will disappear, just as it did in the case of steel castings.

Mr. E. F. C. Davis.—I would like to ask whether this steel mixture has been used in making car wheels, and whether there is any trouble to get the iron and steel to melt down together and mix together properly in the ladles.

No. 282.—57.

What makes the best molds for complicated steel castings to secure solidity and freedom from shrinkage cracks?

Mr. T. R. Morgan, Sr.—The question as put is so broad that it is almost unanswerable, but the intention of the question was no doubt to cover, if possible, those defects which have been met in the manufacture of steel castings, and the desire is to know how best they may be avoided. A reply can only be made in a general way, as there are so many conditions to be met with in the present demands for steel castings in such variable shapes—delicate and massive, simple and complex—that each and every size and kind has to have a study and a practice peculiarly its own to obtain the best results to meet the class of work for which such castings are intended. It is not intended by this reply to make the answer to this question more complex than the question is, but if possible to cast a ray of light toward helping others interested to look at this question in the manner so far found necessary, and the same processes will have to be continued to obtain the best practical results in this as in every line of manufacture.

It is well known to those who have had large experience in the manufacture of iron castings—large, medium, and small—that to obtain similar results in the varying kinds and sizes, molds of varying kinds to suit each kind and size have to be made. The kinds of material charged in the cupola also have to be changed when passing from small to large work to give even similar results. In large massive castings a much higher grade and harder material can be used than on the smaller and thinner castings. If a high grade of hard iron were used for small castings, on such castings as pulleys and other classes of thin work, they would shrink so much in cooling that they would break to pieces before or soon after leaving the mold; and even if such castings would stand without breaking, they would be so hard and dense that they could not be machined with steel tools. For large, massive castings, iron even of a higher grade and density could be used than the last specified, since in the large castings of considerable thickness the grain would be open, it would be easily machined with steel tools, and would prove just the kind of iron needed to give strongest and best results for such large castings. The sand for the molds and the wash or coating for the various sizes and

kinds of molds have also to be varied to suit each size and kind of casting. The temperature at which each kind and size of casting should be poured is another very important item, and the methods used for gating, and the place or places selected to run the castings, must also be studied, since some castings require one, and others many places to run the molten metal. The sizes and places for such runners are important, and when to run each, whether one after the other or together, is another matter which will tax the judgment of those in charge to obtain the best practical results, in combination with many of the other combinations specified above, and even after attaining the best results specified, it becomes necessary with some kinds of complicated castings to open up the molds at the proper time to prevent the casting from undue strains in cooling and shrinking. This requires long practice and good judgment on the part of the mechanic or mechanics in charge.

All of the above conditions specified for castings of iron must be duplicated in the manufacture of steel castings, but on higher and more exacting planes. In the manufacture of iron castings, most of the practice of the world has been carried on (governed in the many conditions necessarily specified here as to the use of mixtures of material) by grades bought from leading producers according to standards fixed by the breaking and eye test, using cupolas or air furnaces, with coke and coal to do the melting. All the operations depend on the crude tests last specified, and the reliability of the supply of the best materials from reputable manufacturers of coal, coke and iron. A great many responsible, reputable founders, having experienced, practical men employed in their foundries under such conditions, have turned out some of the most varied, complicated and best castings made for years. Some few of the modern and best have had their chemical laboratories, and as far as material is concerned, have gone beyond the general practice of iron founders, and are certainly entitled to the best results. But while these show by comparison the possibility of getting the best iron castings, just as wrought iron has been manufactured in rolling mills under similar crude practical conditions, the manufacture of the many grades of steel required for the best steel castings of varying sizes and kinds has made necessary all the higher conditions of chemical and practical knowledge combined. Steel castings, by whatever method the steel is melted—whether open hearth, Bessemer,

crucible, or some other—require each a different treatment to produce similar results. The chemical quality of molds and coating must be known and changed to produce similar results in two different kinds and sizes of castings, following with keener practical care every operation specified for iron castings. After this, practical machine and other testing records should be made, so that every known practical and chemical operation shall have been recorded for future use, with a careful watch also upon the practical use of castings made. All this takes time, just as has been necessary for the steel rail men to find out its necessary good qualities. Steel castings require higher class conditions than iron, depending more on the chemical than practical, but requiring both to a higher degree for steel than iron. The manufacturer having the best general conditions, and who will have the desire and determination to take none but the best stock, will certainly make the best steel castings. To enable him to do so, it will be necessary for him to get the best encouragement possible from the purchaser of steel castings, who should not expect the best steel castings at lowest competing price, but will be willing to pay more for the best castings made under careful and costly conditions than for castings not so good made under inferior conditions. This latter enters into the question as much as either and all of the others.

Steel castings made and annealed well have an average of at least four times the tensile and wearing qualities of best cast iron. With the many conditions specified here carefully attended to, the splendid results attained by leading manufacturers of iron castings in the leading countries of the world are possible, and I have not the least doubt that their success will be fully equaled in time in steel castings. Of this we have many satisfactory evidences already from some leading manufacturers of steel castings, who have turned out complicated examples both large and small. Being myself the president of one of the steel casting companies who make steel castings in large quantities for leading manufacturers and railways all over this country, we have refused to touch many important complicated steel castings, simply because the cost of making them would be greater than the return, when iron flasks only are used and some of a complicated kind have to be made specially for such work. All I have said here is from long experience and observation, with a desire only to answer the question in the only right, truthful, practical way, and not to leave

some or any conditions out. Any not being regarded may destroy all the others, which would favor the best results to obtain solidity and freedom from shrinking and cracks, as well as other best qualities entering into steel castings. To verify what we have said here, we have had many evidences of varying grades of castings run from the same ladle, in which some would prove to be solid and others would not. We always have found a good reason for the differences, and have used our knowledge to secure a remedy the next time, taking good care that when we had similar castings following to have them alike. Very important difficulties to be met with in the manufacture of steel castings come from the high melting temperature and quick cooling of steel, and from the fact that steel has about double the shrinkage met with in iron castings.

PAPERS

OF THE

NASHVILLE MEETING

(XVIIth),

MAY, 1888.

CCLXXXIII.

PROCEEDINGS

OF THE

NASHVILLE MEETING

(XVIIth)

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

May 8th to 12th, 1888.

LOCAL COMMITTEE—Gen. G. P. Thurston, *Chairman;* Prof. Olin H. Landreth, *Secretary.*

SECTION ON HALL AND HOTEL ACCOMMODATIONS—Dr J. P. Dake, *Chairman;* Norman Pierce, *Secretary;* Hermaa Justi, G. P. Thruston, A. W. Stockell, Dudley Gale.

SECTION ON EXCURSIONS AND ENTERTAINMENTS—Olin H. Landreth, *Chairman;* Robert Ewing, *Secretary;* Herman Justi, Minor Scovel, Wm. Stockell, Wm. Cassetty, Wm. T. Magruder, W. C. Smith, W. F. Foster, J. A. Jowett, W. L. Dudley, N. M. Pierce, A. B. Tavel.

SECTION ON FINANCE—L. T. Baxter, *Chairman;* G. M. Fogg, H. W. Buttorff, H. M. Pierce, J. M. Safford, W. M. Woolwine, A. W. Stockell.

SECTION ON ENTERTAINMENT OF VISITING LADIES—A. H. Robinson, *Chairman;* Mr. and Mrs. J. M. Dickinson, Mr. and Mrs. A. H. Robinson, Mr. and Mrs. J. M. Head, Mr. and Mrs. H. M. Pierce, Mr. and Mrs. S. A. Champion, Mr. and Mrs. G. W. Chambers, Mr. and Mrs. J. T. Howell, Mr. and Mrs. Thomas H. Fearey, Mr. and Mrs. E. E. Hoss.

SECTION FROM MERCHANTS' EXCHANGE—George S. Kinney, *Chairman;* George W. Fall, George W. Stockell, Benjamin Lindauer, J. W. Thomas, H. B. Buckner, A. J. Harris.

FIRST DAY, TUESDAY, MAY 8.

The First Session was called to order at 10 A.M. in the large hall of the Watkins Institute. The Secretary's Register showed the following members in attendance:

Baldwin, S. W. .... New York City.
Barrus, Geo. H. .... Boston, Mass.
Betts, Alfred .... Wilmington, Del.

Bond, Geo. M.	Hartford, Conn.
Cobb, E. S.	Terre Haute, Ind.
Cole, J. W.	Columbus, Ohio.
Coon, J. S.	Burdett, N. Y.
Dent, E. L.	Washington, D. C.
Firestone, J. F.	Columbus, Ohio.
Foster, W. A.	Corning, N. Y.
Gale, H. B.	St. Louis, Mo.
Gobeille, J. L.	Cleveland, Ohio.
Hand, S. A.	Toughkenamon, Pa.
Hammett, H. G.	Troy, N. Y.
Hawkins, J. T.	Taunton, Mass.
Hemenway, F. F.	N. Y. City.
Hewitt, Wm.	Trenton, N. J.
Huston, C. L.	Coatesville, Pa.
Hutton, F. R., *Secretary*	N. Y. City.
Kent, Wm.	N. Y. City.
Kirby, F. E.	Detroit, Mich.
Kirkevaag, Peter	Youngstown, Ohio.
Landreth, Olin H.	Nashville, Tenn.
Lane, J. S.	Chicago, Ill.
Lanphear, O. A.	Columbus, O.
Lewis, J. F.	N. Y. City.
Livermore, C. W.	N. Y. City.
Magruder, W. T.	Nashville, Tenn.
Mattes, W. F.	Scranton, Pa.
Miller, Wm.	Pittsburgh, Pa.
Middleton, Harvey	Louisville, Ky.
Minott, H. P.	Columbus, Ohio.
Nagle, A. F.	Chicago, Ill.
O'Connell, J. C.	Montgomery, Ala.
Parks, E. H.	Providence, R. I.
Parsons, H. de B.	New York City.
Pond, F. H.	St. Louis, Mo.
Reese, Jacob	Pittsburgh, Pa.
Roberts, T. H.	Detroit, Mich.
Rogers, W. S.	Cincinnati, Ohio.
Sanders, Newell	Chattanooga, Tenn.
See, Horace, *President*	Philadelphia, Pa.
Sharp, Joel	Salem, O.
Snell, H. I.	Philadelphia, Pa.
Suplee, H. H.	Philadelphia, Pa.
Stetson, G. R.	New Bedford, Mass.
Stillman, F. H.	New York City.
Sweet, J. E.	Syracuse, N. Y.
Tompkins, S.	Crozet, Va.
Warner, W. R.	Cleveland, O.
Warren, B. H.	Boston, Mass.
Watson, Wm.	Boston, Mass.
Whitehead, G. E.	Providence, R. I.
Whitney, B. D.	Winchendon, Mass.

Wilcox, J. F ....Pittsburgh, Pa.
Wiley, W. H....... ....New York City.
Woods, A. T..............................Champaign, Ill.
Woodward, C. M..........................St. Louis, Mo.

Several guests were also in attendance, and a number of ladies.

The first business was the report of the tellers of the Council, which was presented as follows:

The Council would also present the report of its tellers, as follows:

The undersigned were appointed a committee of the Council to act as tellers, under Rule 13, to count and scrutinize the ballots cast for and against the candidates proposed for membership in the Society of Mechanical Engineers, and seeking election before the XVIIth meeting of the Society in May, 1888. They would report that they have met upon the designated days in the office of the Secretary, and proceeded to the discharge of their duties.

They would certify, for the formal insertion in the Records of the Society to the election of the appended named persons, to their respective grades upon Lists No. 1 and 2, respectively pink and yellow.

There were four hundred and twenty-six votes cast in the ballot upon the pink list, of which ten were thrown out because of informalities.

There were three hundred and fifty-seven votes cast upon the yellow ballot, of which twenty-two were thrown out because of informalities.

The lists are appended below.

STEPHEN W. BALDWIN, } *Tellers.*
March 1, 1888. WM. H. WILEY, }

MEMBERS.

Birkinbine, John..............................Philadelphia, Pa.
Bixby, W. H..................................Wilmington, N. C.
Booth, Thomas C.............................New York City.
Bray, Chas. W........ .... Youngstown, O.
Carroll, Lafayette D..........................Birmingham, Ala.
Clark, Walter L......................... .....New York City.
Cole, Francis J...............................Baltimore, Md.
Coleman, Isaiah B ....Elmira, N. Y.
Coleman, Wm. H.............................Chicago, Ill.
Easby, Francis H.............................Wilmington, Del.
Foster, W. A..................................Corning, N. Y.
Gantt, Henry L........ ....Philadelphia, Pa.

Garrett, Wm. . . . . Cleveland, O.
Gillis, H. A. . . . . Meadville, Pa.
Hammett, Hiram G. . . . . Troy, N. Y.
Hildrup, W. T., Jr. . . . . Harrisburg, Pa.
Hillard, Chas. J. . . . . Pittsburgh, Pa.
Larkin, Fred. A. . . . . Milwaukee, Wis.
McRae, John D. . . . . Baldwinsville, N. Y.
Miller, T. Spencer . . . . Chicago, Ill.
Miller, William. . . . . Pittsburgh, Pa.
Müller, T. M. . . . . New York City.
Moreau, Eugene. . . . . Philadelphia, Pa.
Roberts, Geo. J. . . . . Washington, D. C.
Roberts, Percival, Jr. . . . . Pencoyd, Pa.
Roberts, Wm. . . . . Waltham, Mass.
Robertson, R. A., Jr. . . . . Providence, R. I.
Rogers, Winfield S. . . . . Cincinnati, O.
Sanders, Newell. . . . . Chattanooga, Tenn.
Scovel, Minor. . . . . Nashville, Tenn.
Scoville, H. H. . . . . Chicago, Ill.
Shackford, James M. . . . . Bloomington, Ill.
Shaw, T. Jackson. . . . . Wilmington, Del.
Sheppard, Frank S. . . . . Altoona, Pa.
Smith, Scott A. . . . . Providence, R. I.
Stevenson, Archy A. . . . . Johnstown, Pa.
Strale, Allan. . . . . Stamford, Conn.
Suplee, Henry H. . . . . Philadelphia, Pa.
Thissell, Earl A. . . . . Lowell, Mass.
Thomas, Charles W. . . . . Newark, N. J.
Tolman, Edward F. . . . . Worcester, Mass.
Tregelles, Henry. . . . . Salamanca, N. Y.
Trump, Chas. N. . . . . Wilmington, Del.
Unzicker, Herman. . . . . Chicago, Ill.
Vaile, J. Henry. . . . . Dayton, O.

ASSOCIATES.

Pierce, Walter L. . . . . New York City.
Smith, S. Decatur. . . . . Philadelphia, Pa.

JUNIORS.

Burchard, Anson W. . . . . Danbury, Conn.
Burns, A. L. . . . . New York City.
Cole, L. W. . . . . So. Shaftesbury, Vt.
Curtis, Ralph E. . . . . Boston, Mass.
Dockam, Edward H. . . . . New York City.
Firestone, J. F. . . . . Columbus, O.
Glasser, Chas. H. . . . . No. Tarrytown, N. Y.
Hildreth, Wm. O. . . . . Boston, Mass.
Hobart, James C. . . . . Cincinnati, O.
Taylor, Wm. M. . . . . Indianapolis, Ind.
Veeder, Curtis H. . . . . Calumet, Mich.

No new business being presented by the members, the professional papers were then taken up. The first paper was by Mr. Henry R. Towne, of Stamford, Conn., entitled "A Safety Car-heating System," which was discussed by Messrs. Babcock, Gobeille, Minot and Kent. A paper on the same subject by Mr. Wm. J. Baldwin, of New York, "Notes on Warming Railroad Cars by Steam," received no discussion. The third paper on this subject, by Mr. John T. Hawkins, of Taunton, entitled "Automatic Regulator for Heating Apparatus," was discussed by Messrs. Babcock, Dent, Kent, Warner, Gobeille, Reese and Minot.

A second paper by Mr. John T. Hawkins, "A Plea for the Printing Press in Mechanical Engineering Schools," was discussed by Messrs. Kent, Cobb, Hutton, Reese, Gobeille, See and Sweet.

Two papers belonging to the Economic Section of the Society's papers were as follows: By Mr. H. L. Binsse, of Newark, N. J., on "A Short Way to Keep Time and Cost," and by Mr. Geo. L. Fowler, of New York, on "Estimating the Cost of Foundry Work." The latter was discussed by Messrs. Dingee, West, Hawkins, See and Sweet.

The Topical Queries were then taken up to the hour of adjournment. Messrs. Cooper, Dingee, Suplee, Minot, Lanphear, Warner, and Parsons discussed the two queries on rope-driving transmission as follows: "What is the most economical speed in telo-dynamic transmissions for high and low power? and, What data have you for design of hemp-rope transmissions, especially where several parallel ropes replace a flat belt?" After announcements as to excursions, the President announced the nominating committee for officers of the Society for the ensuing year, under Article 31 of the Rules.

Such committee consisted of Mr. W. R. Warner, of Cleveland, Ohio; Mr. B. H. Warren, of Boston, Mass.; Mr. A. F. Nagle, of Chicago, Ill., Mr. Geo. R. Stetson, of New Bedford, Mass., Mr. John F. Wilcox, of Pittsburgh, Pa.

The session then adjourned.

The excursion of the afternoon is described in its proper place. In the evening a public session was held in Watkins Institute, and addresses of welcome were delivered by Governor Robert L. Taylor of the State of Tennessee; by Mayor P. C. McCarver, of the city of Nashville, and by Gen. G. P. Thruston, Chairman of the Citizens' Committee of Arrangements, who also presided at the earlier part of the sessions. At the close of these addresses, the

chair was handed over to President See of the Society, who introduced Prof. Calvin M. Woodward of the Society, who had been invited by the officers of the Vanderbilt University to deliver an address before their engineering students, and the topic selected by him was "The Coming Engineer." At the close of this address an informal reception was tendered to the Society in the rooms of the Art Association and Historical Society in the Watkins Institute.

SECOND DAY, WEDNESDAY, MAY 9.

The morning session was appointed for 9.30 in the chapel of Vanderbilt University. President See was detained from the chair by illness, and on motion ex-President Sweet was called to preside. The exercises were opened by an address by Chancellor L. C. Garland, of the University. This was followed by professional papers as follows: Prof. R. H. Thurston's paper on "Proportioning Steam Cylinders" was discussed by Messrs. Babcock and Whitham. The paper by Mr. C. C. Collins on "A New Method of Inserting and Securing Crank Pins" was discussed by Messrs. Fawcett, Hawkins, Cole, Minot, Hutton, Bond, Kent, Cole and Reese. The paper by Mr. Wm. F. Mattes on "Connecting Rods" was discussed by Messrs. Whitham and See. At the close of this discussion, the session adjourned to allow a visit to the buildings of the University, and after this inspection the members convened at the foundation begun for the building of the School of Mechanical Engineering. Bishop McTyeire, President of the Board of Trust of the University, opened the exercises by a brief address of welcome, responded to by Prof. F. R. Hutton on behalf of the Society. The contents for the chest to be deposited within the corner-stone were laid in place by Prof. Magruder of the University, and Mr. Wm. Kent, of the Society, gave an address of salutation, after which the stone was laid and sealed, and the laying was concluded by appropriate religious exercises. A lunch was served in Wesley Hall of the University by the ladies connected with its corps of officers, and the afternoon excursion was begun from this point.

In the evening, the three papers by Mr. Geo. H. Barrus were discussed as follows: "The Effect of Circulation in Steam Boilers on Quality of Steam" was discussed by Messrs. Whitham, Nagle, Coon, Gale, Minot, Suplee and Huston; that entitled

"Memoranda on the Performance of a Compound Engine" was discussed by Messrs. Coon, Minot, Parsons, Cobb, Suplee, See, Gale and Nagle; that on "An Electric Speed Recorder" was discussed by Prof. Sweet.

The Topical Queries were here again taken up for the rest of the evening. Messrs. Barrus and Minot answered the query in reference to the fuel economy of high-speed engines as compared with slower engines with releasing-gear. Mr. Suplee gave data as to the paper friction-gear used in the Northwest. Messrs. Reese, Stetson, Hutton, Minot, Lanphear, Hawkins, Rogers and Cole spoke upon the molecular structure and hardness of steel, as affected by electro-plating and flexure in reducing the temper of highly hardened steel. Mr. Kent moved that at a future meeting a symposium on steel phenomena be held as one feature of the Topical Queries of that session. This was referred informally to the Committee on Queries.

The query as to the necessary amount of metal to be removed from sheared edges of steel plate was answered by Messrs. Hawkins, Cooper, Suplee, Kent, Huston, Cobb, Coon, Nagle, See and Reese.

After announcements, the session adjourned.

Third Day, Wednesday, May 10.

The fifth session was opened by the paper of Prof. Jay M. Whitham, of Fayetteville, Ark., on "Surface Condensers." This was discussed by Messrs. Wheeler, Engel, Cole, Nagle, and Coon. The paper by Mr. J.S. Coon, of Burdette, N. Y., entitled, "Duty Trials of Pumping Engines," was discussed by Messrs. Barrus, Hawkins, Sweet, Nagle, Mattes, Kent, and Hutton. The debate was opened by proposing that a committee of five be appointed by the President to prepare a report embodying a series of rules which could be followed by members of the Society in their practice, and could be recommended as standards in such tests by outsiders. The question of the policy of the Society in the matter of adopting reports of committees was again adverted to,* but the advantages would be so great if such standard rules were prepared by competent authority, even if it were not expedient for the Society to enforce their adoption, that when the question of the appointment was

* See Volume VI. Transactions, pages 256, 314, and 877, Nos. CLXVIII., CLXVIII. A, and Appendix VI.

put it was carried unanimously, and the President subsequently appointed Messrs. Barrus, Coon, Nagle, Reynolds, and De Kinder. The paper by Mr. F. W. Dean, on the "Distribution of Steam in the Strong Locomotive," was discussed by Messrs. Barrus, Coon, Mattes, Snell, and See. That by Mr. L. S. Randolph, on "Strains in Locomotive Boilers," was discussed by Messrs. Middleton, Parsons, Kent, Hawkins, Minot, Baldwin, Reese, and Wilcox. Mr. W. L. Clements, of Bay City, Mich., presented a paper on "Steam Excavators," which received no discussion. Mr. John M. Sweeney, of Wheeling, W. Va., presented a paper on "River Practice in the West," which was discussed by Messrs. Nagle, Wilcox, Kent, Rogers, Parsons, Bond, Minot, Livermore, Woodward, and See. At the conclusion of this discussion the morning session adjourned.

The concluding session was convened in the hall of the Watkins Institute, at eight o'clock, Thursday evening. The paper by Mr. H. de B. Parsons, upon the "Displacements and Area Curves of Fish," received no discussion. The paper by Prof. James E. Denton, on "The Mechanical Significance of Viscosity Determinations of Lubricants," was discussed by Messrs. Wilcox and Hawkins. Mr. Frederick A. Scheffler, of Erie, Pa., gave a "Foundry Cupola Experience," which was discussed by Messrs. Snell, Wilcox, Gobeille, Sweet, and Mattes. The paper on "The Best Form for Nozzles and Diverging Tubes," by Mr. A. F. Nagle, of Chicago, received no discussion. Prof. Thurston's second paper, entitled, "Large and Enlarged Photographs and Blue Prints," was discussed by Messrs. Hall, Fawcett, Rogers, and Wilcox.

The paper by Prof. J. Burkitt Webb, on "A Persistent Form of Gear Tooth," received no discussion. The final paper of the session was that of Mr. Wm. Hewitt, of Trenton, N. J., entitled, "Wire Rope Fastenings," and was discussed by Messrs. Hawkins and Reese.

At the close of this paper, the following resolutions were presented by various members with short and appropriate remarks:

Whereas, The seventeenth meeting of the American Society of Mechanical Engineers has been held in the city of Nashville, and the members have received an unprecedentedly hearty welcome;

Resolved, That it is the unanimous voice of the members that they desire to place on record their appreciation of the many kindnesses and courtesies extended them during their short stay.

Resolved, That our hearty thanks are extended to the local committee of citizens of Nashville, and especially to Gen. G. P. Thruston, chairman, and Prof. Olin H.

Landreth, secretary, for their untiring efforts in planning and carrying out a most successful programme for the entertainment of the Society.

Resolved, That we heartily thank Governor Taylor, of Tennessee, and Mayor McCarver and the citizens of Nashville, who have so ably seconded the local committee in their efforts to make our stay thoroughly enjoyable and memorable.

Resolved, That our thanks are due to the President and Faculty of Fisk University for the opportunity afforded to visit their unique and noble institution, and to note their worthy and highly successful efforts in educating the colored race.

Resolved, That we extend our thanks to the Board of Trust and Faculty of Vanderbilt University for the opportunity to visit their excellent institution, and especially do we thank them for their kind invitation to participate in the exercises of laying the corner-stone of their new School of Mechanical Engineering.

Resolved, That we extend our thanks to the West Nashville Land Improvement Company, Dr. H. M. Pierce, president, and to the Nashville Iron, Steel and Charcoal Company, Gen. Willard Warner, president, for their courtesies extended in exhibiting and explaining their highly interesting and extensive plant for making iron and charcoal, and for making useful products from the waste gases of the charcoal kilns.

Resolved, That the Society extends its thanks to the President and other officials of the Nashville, Chattanooga & St. Louis Railway Company for their kindness in providing transportation to the various points of interest in the vicinity, and also for their provisions for an extended trip to Chattanooga and return.

Resolved, That we extend our thanks to Gen. W. H. Jackson, proprietor of Belle Mead farm, for his kindness in extending to us the hospitality of his beautiful and magnificent estate, and the opportunity to inspect his world-famed and unparalleled thoroughbred stock.

Resolved, That we highly appreciated and enjoyed the generous repast provided at Vanderbilt University after the exercises of laying the corner-stone, and that our thanks are hereby extended to "the ladies of the campus" of Vanderbilt University for their bounteous hospitality in providing that delightful entertainment, and for their other acts of courtesy during the day, and also to the ladies of the Reception Committee in Nashville for the attention and hospitality extended to the visiting ladies. To Mrs. Polk, also, for the privilege of visiting her historic mansion, and of enjoying her hospitality and courteous reception of her guests.

Resolved, That our hearty thanks are extended to Messrs. Whitsett & Adams, builders of the new reservoir, for their kindness in exhibiting their work while in progress, and for the generous entertainment provided during the visit.

Resolved, That the thanks of the Society are due and are hereby tendered to the members of the Cincinnati Reception Committee, Messrs. W. H. Doane and G. A. Gray, Jr., for the generous entertainment received at their hands on our arrival in their city; for the delightful carriage drive, of which our enjoyment was complete, and which formed so pleasant a feature of our trip to the seventeenth meeting, and which will be long remembered by those who were privileged to enjoy it.

The President, after the unanimous passage of the above resolutions, and response to them briefly by Prof. Landreth and General G. P. Thruston, made an announcement of the autumnal annual meeting of the Society in October, at Scranton, Penn., and the seventeenth meeting adjourned.

Excursion Days.

The members of the Society who came to the meeting from New England and New York made use of special cars put at their service by the courtesy of the N. Y., L. E. & W. R. R. from Jersey City through to Nashville. On their arrival at Cincinnati, Ohio, and pending the departure of the evening train on Monday, the party was met by Messrs. Doane and Gray of that city, and were by them escorted in carriages to visit the Exposition Buildings and Music Hall, to the Art Museum and Park at Walnut Hills, and through the residence part of the city upon the heights.

Tuesday, May 8.

On Tuesday afternoon, a special train on the Nashville, Chattanooga and St. Louis R.R. conveyed the party to West Nashville stopping at Fisk University. Addresses by the President of the University, by the President of the Society, and by Prof. C. M. Woodward, were interspersed between singing of Jubilee and other songs by the assembled (colored) students. At West Nashville, the Society inspected the new furnace plant of the Standard Charcoal, Iron and Chemical Company, being welcomed by General Willard Warner and Dr. H. M. Pierce, who also acted as guides in the inspection of the charcoal kilns and bye-product apparatus. Returning, the party reached the Union Depot in good season.

Wednesday, May 9.

After the morning session for papers and the visit to the buildings of Vanderbilt University, the ceremonies of the corner-stone laying of the new building for mechanical engineering, and a most enjoyable luncheon in the refectory of Wesley Hall, served by the ladies of the Vanderbilt University campus, carriages conveyed the party to the West Side Park, where a special train bore them to Belle Mead, the celebrated stock-farm owned by General W. H. Jackson. He received his guests in the hall of his hospitable mansion with a few well-chosen words of welcome, and then escorted them to see his deer and their park, his blooded horses and other selected stock. On the return to the verandas of the house, after refreshment, and a few words of thanks and recognition by a representative of the Society, the party returned to the city.

THURSDAY, MAY 10.

The excursion of this afternoon was by special train over the Overland Dummy Road to Glendale Park, stopping at the new City Reservoir. The party were entertained most hospitably by Messrs. Whitsett and Adams, the contractors, and much enjoyed the rest and luxuriant foliage of the Park.

FRIDAY, MAY 11.

A special train left the Union Depot bearing the party and some of their hosts to South Pittsburg. After dinner at the City Inn and Marion Hotel, the party were escorted to visit the Perry Stove Works, the South Pittsburg Pipe Works, and the furnaces and mines of the Tennessee Coal, Iron and R.R. Co., exploiting the resources of the Sequachee Valley. At the Inman mines, the geological formation of the ore bodies was described by Dr. J. M. Safford, State Geologist, and several members visited the underground workings. Returning thence to South Pittsburg, and being rejoined by the ladies, the main line was resumed to Chattanooga.

On Saturday morning an escort party of the Chamber of Commerce was in waiting at a station of the Union Ry. Co., and accompanied their guests to Citico Furnace and the New Water Works, up the Mission Ridge incline, over to East Lake, over a part of the new Chattanooga and Lookout Mountain Railway and return.

After dinner, the same train conveyed the excursion party to the foot of the inclined plane up Lookout Mountain, and from its top the magnificent view of the historic battle-fields could be seen to best advantage. After an inspection of the machinery of the plane, the party returned to the hotel.

In the evening, the Chamber of Commerce held a reception for the members of the Society in their rooms on Market Street, brief speeches of welcome and congratulation being made by both hosts and guests.

The special car of the Society was in waiting at the station after the reception, and bore the larger proportion of the members away.

The weather was perfect during the entire week of the convention.

CCLXXXIV.

PROPORTIONING STEAM CYLINDERS.

BY R. H. THURSTON, ITHACA, N. Y.
(Member of the Society).

THE best proportion of the steam cylinder, in the modern steam engine, depends upon so many and such conflicting conditions that it would seem almost hopeless to attempt to work out more than a rough approximation to the best for any given case in practice. There are, however, some considerations of prime importance which may be investigated, and which may lead to some rather definite standards. These are conditions of serious influence in the modification of heat wastes, the costs of construction, and, above all, the financial conditions of continuous operation. The first set of conditions determines the best form of steam cylinder; the second determines the type and proportions of an engine for which the power, the steam pressure, and the ratio of expansion are settled; while the last set of conditions determines how large an engine, under given conditions of cost of the various items making up the total of running expenses, can be profitably employed.

Of the last, a somewhat extended study has been made by the writer in earlier papers, and by others, and it is not intended to consider them further here. It is enough to say, in review of that subject, that it is found that the best engine for a given place is not that which gives the highest duty, but that which, all items of cost and operation being considered, gives the least average running expense for the life of the machine, or for an extended period of operation. Such an engine is smaller, and has a lower ratio of expansion than the engine which would be chosen on the basis of highest duty; and this difference in size and first cost is the greater, in this regard, as the type of engine chosen is the further removed from the ideal, the perfect engine. It is, for example, found that while the ideal engine may, under certain specified conditions, have a ratio of expansion at maximum efficiency of fluid of twenty, the actual engine built as nearly as the skill of man can make it upon the same scale and type, gives its highest duty at ten

expansions, and would do still better, financially, if given enough more work to make its ratio of expansion in regular working, we will say, seven or eight. In other words, a smaller engine is desirable in this last case than would have been taken as best were the conditions, other than financial, to have control. The precise proportions to be adopted are determinable whenever the data are furnished, and the problem thus presented will be assumed to have been solved for the cases to be taken up in the present paper.

The costs of construction vary enormously with the type and general proportions of the engine. As a rule, the larger the engine, the less the cost per horse power developed ; the shorter the stroke, and hence, the shorter the engine as a whole, in similar proportion nearly, the less the cost of building; and the simpler its design, and especially of its valve motion, the lower the expense of construction. The tendency of sharp competition, such as now exists in the general market, the result of which has been to reduce the profits in the business, in many instances, far below the paying point, is to lead to the design and construction of engines of very short stroke and of very high speed of piston and of rotation, a tendency also promoted by the fact that, other things being equal, high piston speed and high speed of rotation give reduced wastes of heat in the cylinder, as well as lessened costs of building. Thus, though the proportions of the steam-engine cylinder are often determined by the judgment of the designer, or the exigencies of location, the relative length of stroke and diameter of cylinder are actually related by definite principles; these being settled, it becomes easily possible to solve the problem, and to fix the dimensions at a single operation.

Efficiency of working fluid is promoted by securing a definite proportion of stroke to diameter. Were all parts of the interior surfaces similarly affected by variations of temperature, as the engine works through its cycle, it is evident that minimum cylinder condensation would be secured by making the ratio of that surface to the cubic contents of the cylinder a minimum; that is by making the stroke twice the diameter. The volume on each side of the piston would then have a diameter equal to its length, a condition recognized as that corresponding to the conditions sought. The losses from the exterior being considered, a stroke equal to the diameter would be prescribed ; but those losses of heat are insignificant in comparison with the wastes by internal conduction, convection, and radiation, and the proportions of the cylinder are de-

termined by the first named considerations. In fact, however, the waste by this latter form of loss of heat, by cylinder condensation, occurs mainly inside the point at which the cut-off valve closes, and the proportions should be, were this absolutely the limit, such as would make the stroke equal approximately to the product of the diameter and the ratio of expansion. On the other hand, the wastes of heat internally come quite as generally from the loss of heat during the later part of the expansion and the period of exhaust, during the period of re-evaporation, as from the action noted during the time of entrance of prime steam. That is to say: the total condensation up to the point of cut-off is determined largely by the extent to which cooling is permitted, and re-evaporation produced during and after the formation of the expansion line, and during the return stroke.

But still another consideration comes in to complicate the problem: the time of exposure of the surfaces to the action of the steam and the exhaust. It is as yet unknown just what influence this has upon the amount of the condensation, and general experience has led to the fixing of the proportions of the engine at not more than stroke 2 and diameter 1, nor less than equality of stroke and diameter. The former proportion is adopted for slow engines, the latter for those of highest speed of rotation. Usual limits lie between one and one and a half diameters.

Assuming the best proportions for a given case to be settled, as, for example, making $\frac{l}{d} = c$, and $c = 1$ for very fast-running engines; $c = 1.33$, or $c = 1.5$ for moderate speeds; and $c = 1.5$ to $c = 2$ for the slower engines; and taking the speed of piston, as assumed by Watt, as varying as the cube root of stroke, and if V is the speed, and N the number of revolutions per minute,

$$V = a\sqrt[3]{L} \div a\sqrt[3]{\tfrac{1}{2}l};$$

$$N = \frac{b}{L^{\frac{2}{3}}} = \frac{b'}{l^{\frac{2}{3}}} = \frac{b'}{c^{\frac{2}{3}}d^{\frac{2}{3}}};$$

in which $a = 500$ as a fair maximum; $b = 250$, and $b' = 1300$; we may at once obtain a value of the diameter, d, of the cylinder thus:

The net power of the engine to be designed is the quantity first

to be fixed upon, and the gross or indicated power is to be thence obtained by dividing the net or dynamometric power by the efficiency:

$$D.\ H.\ P. \div E_c = I.\ H.\ P. = 2p_m LAN \div 33,000,$$

in terms of the mean pressure p_m, the length of stroke L in feet, the area of the piston A in square inches and the number of revolutions per minute, N. Then if the proposed ratio of length of stroke to diameter of piston is c, $l = 12\ L$, and

$$I.\ H.\ P. = \frac{\frac{1}{2}p_m L\pi d^2 N}{33,000} = \frac{1}{24}\,\frac{p_m c d^3 N}{33,000},$$

and taking the value of N as a function of length of stroke, as above,

$$U = I.\ H.\ P. = \tfrac{1}{24}p_m b' c^{\frac{1}{2}} d^{\frac{5}{2}} \div 33,000$$

$$= m p_m d^{\frac{5}{2}},$$

$$d = \left(\frac{U}{m p_m}\right)^{\frac{2}{5}} = n\left(\frac{U}{p_m}\right)^{\frac{2}{5}},$$

when m and n are the collected constants.

Taking $a = 500$; $b = 250$; $b' = 1300$, as above, and assigning values to c, as in the table, we obtain the accompanying values of m, n, and $\frac{1}{m}$:

TABLE OF CONSTANTS.

c	b	m	$\frac{1}{m}$	n
2.00	500	.013094	761.35	6.411
2.00	375	.009823	101.80	7.252
2.00	250	.00655	152.00	8.625
2.00	175	.004584	213.14	10.053
2.00	125	.003274	305.40	11.613
1.75	500	.012527	79.827	6.534
1.75	375	.009396	106.43	7.392
1.75	250	.006250	159.70	8.758
1.75	175	.004385	228.07	10.247
1.75	125	.003131	319.30	11.837
1.50	500	.011900	84.03	6.680

TABLE OF CONSTANTS.—*Continued.*

c	b	m	$\frac{1}{m}$	n
1.50	375	.008925	112.05	7.556
1.50	250	.005950	168.10	8.988
1.50	175	.004165	240.10	10.575
1.50	125	.002975	336.14	12.100
1.33	500	.011430	87.47	6.796
1.33	375	.008574	116.60	7.688
1.33	250	.005720	174.80	9.203
1.33	175	.004001	249.92	10.752
1.33	125	.002858	349.89	12.419
1.25	500	.011198	89.30	6.856
1.25	375	.008399	119.07	7.756
1.25	250	.005600	178.80	9.231
1.25	175	.003919	255.14	10.752
1.25	125	.002799	357.20	12.419
1.00	500	.010396	96.20	7.079
1.00	375	.007797	128.26	8.007
1.00	250	.005200	192.40	9.340
1.00	175	.003638	274.84	11.100
1.00	125	.002599	384.78	12.822

Similar tables are readily calculated for other speeds of engine. In any case, the speeds and the relative diameters and strokes of piston being settled upon, for any type or style of engine, the designer can very easily thus deduce a series of dimensions of cylinders to suit his needs or his views.

For example: Let it be proposed to design an engine of 250 H. P., net. Then the indicated power would be, allowing an efficiency of machine $E_c = 0.90$,

$$I.\ H.\ P. = \frac{250}{0.9} = 280,$$

and if the mean pressure is taken at

$$p_m = p^1 \frac{1 + \log_e r}{r} = 40 \text{ lbs.},$$

$$d = \left(\frac{174.8 \times 280}{40}\right)^{\frac{3}{7}} = 21.2 \text{ inches},$$

and calling $d = 21$ inches, $L = 1.33 \times d = 28$ inches or $2\frac{1}{2}$ feet, nearly.

An engine of 21 inches diameter of cylinder, and say $2\frac{1}{2}$ feet stroke of piston, making $N = \frac{250}{2.5^{\frac{2}{3}}} = 136$ revolutions per minute,

nearly, with a mean pressure of 40 pounds, should thus give about 280 indicated horse-power:

$$I.\ H.\ P. = \frac{2p_m LAN}{33,000} = \frac{40 \times 2.5 \times 346.36 \times 2 \times 136}{33,000}$$

$$= 284.55,$$

and the right size of engine is found.

Referring again to the values of n given above, it may be remarked that those corresponding to the value $b' = 650$, or to $V = 250\sqrt[3]{S}$ and $N = 125\sqrt[3]{S}$, correspond to low speeds of ordinary engines, or to high speeds of pumping engines; for $b' = 1300$, or for $V = 300\sqrt[3]{S}$ and for $N = 125\sqrt[3]{S}$, to fairly high speed of engine; while those for $b' = 2600$, or for $V = 1000\sqrt[3]{S}$ and $N = 500\sqrt[3]{S}$, correspond to the highest speeds of engines for electric lighting, for locomotives, and for torpedo boats.

DISCUSSION.

Mr. George H. Babcock:—The elaborate mathematical formulæ worked out in this paper are interesting, and doubtless correct, though I find some difficulty in tracing the relations of some of the equations, as for instance those at the foot of the third page of the paper, and at the head of the fourth page, as well as those in the last paragraph, which latter difficulty I flatter myself is due to typographical errors rather than to my ignorance. It is questionable, however, if all this figuring is needed in practical work, where so much is admittedly left to judgment based on experience.

As the author remarks, "general experience has led to the fixing of the proportions of the engine at not more than stroke 2 and diameter 1, nor less than equality of stroke and diameter," and he adopts this proportion as the basis of his figures. It will be seen also that he has to assume other data in carrying out the calculations, such as the net horse-power, the efficiency, the mean pressure, and approximately the speed, as "slow," "high," or "medium." Now, having these points given or assumed, it would seem unnecessary to resort to logarithms or extract the seventh root of the cube, to ascertain the dimensions wanted.

For instance, I recently had a similar problem presented, to proportion an engine for 250 net horse-power, the conditions being

high steam pressure and medium speed. I therefore made the following assumptions, based on data and experience:

1. Stroke = $1\frac{1}{2}$ diameters.
2. Average pressure = 48 lbs.
3. Efficiency = 80 per cent. to allow ample "leeway."
4. Speed = 660 feet per minute, approximate,

then
$$\text{I.H.P.} = \frac{250}{0.8} = 300,$$
and
$$\frac{33000 \times 300}{48 \times 660} = 313 = \text{area of piston},$$
which is practically 20 inches diameter.
$$\text{Stroke} = 1\tfrac{1}{2} \times 20 = 30 = 2\tfrac{1}{2} \text{ feet.}$$
$$\text{Rev.} = \frac{660}{2.5 \times 2} = 132.$$

It will be seen that I arrived at practically the same sizes and proportions as the author—allowing for difference in data and assumptions—with only a fraction of the figures.

Prof. Jay M. Whitham:—The paper presented by Prof. Thurston is of great interest to the designer. Even if the formulæ deduced do not displace the well-known methods of design, they can profitably be employed to *check* the results. I notice that the values of c (the ratio between the length of stroke of piston and diameter of the cylinder), as given in the paper, though covering all other types, do not apply to marine engines. The following table * will illustrate this point:

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Marine Engine of	No. of Cylinders.	Diameter of Cylinders, ins.	I.H.P.	I.H.P. per cu. ft. of space in the large cylinder.	Stroke, feet.	Revolutions per minute.	Piston speed in feet per min.	$\frac{1}{a}=c$	a	b	b′	Gauge Pressure of Steam, lbs.	Mean Effective Pressure if all work is done in large cylinder.	Reference.
Horizontal, Simple, Condensing Engines.														
H.M.S. Active	2	88	4130	14.00	3.5	74	518	0.4770	341			30	21.62	R. U. S. Ins. Pro., v. 26.
H.M.S. Devastation	4	80	6637	14.60	3.25	76.8	499	0.4875	337			30	21.83	R. U. S. Ins. Pro., v. 26.
H.M.S. Inconstant	2	104.25	7364	15.53	4	74.5	596	0.4604	376			30	23.88	R. U. S. Ins. Pro., v. 26.
H.M.S. Invincible	4	72	5180	15.26	3	81.6	489.6	0.5000	340			30	20.43	R. U. S. Ins. Pro., v. 26.
H.M.S. Orion	4	65	4020	17.50	2.5	100	500	0.4616	368			30	20.45	King's "War Ships."
H.M.S. Superb	2	116	7430	15.79	4	67	536	0.4138	338			30	21.63	King's "War Ships."
H.M.S. Voltage	2	86.125	4330	15.00	3.75	79	592	0.5225	381			30	20.71	R. U. S. Ins. Pro., v. 25.
Averages				15.37			577	0.4747	351	177	920	30		

* This table is from a paper by the speaker published in No. 30 of Pro. of U. S. Naval Institute. Columns 9, 10, 11, 12 and 14 have been added.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Marine Engine of	No. of Cylinders.	Diameter of Cylinders, ins.	I.H.P.	I.H.P. per cu. ft. of space in the large cylinder	Stroke, feet.	Revolutions per minute.	Piston speed in feet per min.	$\frac{1}{a}=c$	a	b	b′	Gauge Press. of Steam, lbs.	Mean Effective Pressure if all work is done in large cylinder.	Reference.
Vertical, Simple, Condensing Engines.														
S.S. Hudson	2	48	1450	9.62	6	75	828	1.5000	456			69		Edwards' "Marine Eng."
S.S. Knickerbocker..	2	44	860	6.79	6	66	840	1.5000	462			70		Edwards' "Marine Eng."
Averages ...				8.20			834	1.5000	459	230	1193			
Horizontal, Compound, Condensing Engines.														
H.M.S. Carysfort ...	3	{ 1-46 / 2-64 }	2300	18.70	2.75	107	588	0.3646	420			60	20.06	R. U. S. Ins. Pro., v. 26.
H.M.S. Cleopatra	4	{ 2-36 / 2-64 }	2611	23.40	2.50	107.9	540	0.4689	398			60	21.11	R.U.S. Ins. Pro., v. 26.
H.M.S. Comus	3	{ 1-46 / 2-64.1 }	2406	19.56	2.75	96.4	530	0.3640	378			60	23.21	King's "War Ships."
H.M.S. Iris.	8	{ 4-41 / 4-75 }	7714	20.91	3	97	582	0.4800	404			65	24.75	King's "War Ships."
H.M.S. Miranda.....	2	{ 1-38 / 1-62½ }	1020	24.29	2	124.5	498	0.3855	395			60	22.20	King's "War Ships."
H.M.S. Satellite......	2	{ 1-36 / 1-62 }	1115	21.44	2.50	98.5	492	0.4839	381			90	19.67	R.U.S. Ins. Pro., v. 26.
U.S.S. Miantomoh..	4	{ 2-32 / 2-48 }	1600	23.53	3.50	65	455	0.8750	300			80	32.06	(Inclined engine.)
Averages...				21.55			526	0.4888	381	191	990			
Vertical, Compound, Condensing Engines.														
H.M.S. Conqueror....	3	{ 1-54 / 2-70 }	4500	28.12	3	80	480	0.3636	333				40.19	King's "War Ships."
H.M.S. Curacoa..	4	{ 2-36 / 2-64 }	2541	22.79	2.5	104.9	524.5	0.4687	387				24.84	King's "War Ships."
H.M.S. Nortaampton.	6	6-54	6010	19.38	3.25	84	546	0.5106	387				39.66	R.U.S. Ins. Pro., v. 26.
French—Ad. Duperré ..	6	{ 2-61 / 4-78½ }	7397	16.74	3.03	77	505.6	0.3549	340				25.03	King's "War Ships."
Russian—Asia......	2	{ 1-34 / 1-60 }	1050	17.56	3.5	66	462	0.7000	304				26.52	King's "War Ships."
Russian—Zabiaca...	2	{ 1-31 / 1-59 }	1470	31.82	3	92	554	0.6102	384				32.03	King's "War Ships."
S.S. Aberdeen......	3	{ 1-30 / 1-42 / 1-70 }	2086	17.38	4.5	65.5	589.5	0.7714	357			[From 60 to 90.]	29.65	*Engineering* v. 33.
S.S. Moor...	2	{ 1-51 / 1-90 }	4250	19.23	5	69.75	697.5	0.6667	408				31.60	*Engineering* v. 33.
S.S. Parisian	3	{ 1-60 / 2-85 }	6020	15.30	5	85	850	0.5000	497				20.59	*Engineering* v. 32.
S.S. Sterling Castle.....	3	{ 1-63 / 2-90 }	8509	17.50	5.5	66.3	731.5	0.5180	414				30.19	*Engineering* v. 33.
Yacht—Wanderer.	2	{ 1-25 / 1-50 }	717	20.49	2.5	99	495	0.6000	365				30.63	*Engineering* v. 31.
Averages...				20.57			585	0.5513	389	195	1011			

This table is divided into four groups representing, respectively, horizontal and vertical, simple and compound marine engines. It is noticed in column 9 that the value of c ranges but slightly either way from 0.5. This small value is due to the restrictions as to space allowed for the machinery of a steamer. Even with

the most recent *merchant* vessels having vertical, inverted compound engines, c is usually found to be much less than unity. I would recommend a value of

$$\frac{l}{d} = c = 0.5$$

for marine engines, as conforming to existing practice. The value of a, the constant in the formula

$$V = a\sqrt[3]{L} = a\sqrt[3]{\tfrac{l}{12}}$$

is from column 10 of the table, from 300 to 497, so that the maximum value of 500, as assumed by Dr. Thurston, is applicable to marine practice. Also, the value of b and b' will apply.

We have, from column 5 of the table, 15.37 I.H.P. developed in horizontal, simple marine engines for each cubic foot of piston displacement in the cylinder; 8.20 for simple, vertical engines; 21.55 for each cubic foot of space in the large cylinder of horizontal, compound engines, and 20.57 for vertical, compound marine engines.

Let d, l, c and I.H.P. denote the same quantities as before, then

$$\frac{\pi}{4}\left(\frac{d}{12}\right)^2 \times \left(\frac{2}{12}\right) = \text{volume of cylinder in cu. ft.} = \frac{\text{I.H.P.}}{K} = \frac{\pi}{4} \times \left(\frac{d}{12}\right)^3 \times c$$

Where K = 15.37 for simple, horizontal marine engines,
= 8.20 " " vertical " "
= 21.55 " compound, horizontal marine engines.
= 20.57 " " vertical " "

Whence, reducing

$d = 6.71\sqrt[3]{\text{I.H.P.}}$, for simple, horizontal marine engines.

$d = 5.64\sqrt[3]{\text{I.H.P.}}$, for simple, vertical marine engines.

$d = 5.93\sqrt[3]{\text{I.H.P.}}$, for compound, horizontal marine engines.

$d = 5.79\sqrt[3]{\text{I.H.P.}}$, for compound, vertical marine engines.

$l = 0.5\,d$, for marine engines.

These are offered only as *check* formulæ, as representing existing marine practice.

CCLXXXV.

THE MECHANICAL SIGNIFICANCE OF VISCOSITY DETERMINATIONS OF LUBRICANTS.

BY JAMES E. DENTON, HOBOKEN, N. J.
(Member of the Society.)

DURING the last ten years leading oil manufacturers and dealers have introduced the practice of determining a property of oils known as "*viscosity*," as a measure of quality, not included in the determinations of "flashing point," specific gravity, and "cold test," which had previously been the only means of indentification adopted.

Instruments for determining this property have been termed "viscosimeters," and in the majority of instances have consisted of a simple reservoir out of which a certain volume of oil is permitted to flow through a small orifice, the time of such flow in seconds being the measure of viscosity.

Fig. 103 represents such an instrument as arranged by Mr. G. M. Saybolt, who has systematized its use for the products of the Standard Oil Company so completely that determinations of viscosity made by him never vary a whole second with the same sample of oil.

It consists of a bath of water, in which is held the vessel A, which is capable of containing four ounces of oil. The lower end of this vessel has an outlet O about $\frac{1}{16}$ inch in diameter, so adjusted as to be as completely surrounded by the water bath as possible. A small piece of glass, B, is set into the lower part of A and the water bath is of glass, so that as the level of oil falls, it finally comes into view at B, and the time of flow of the oil can be made to terminate at the instant when the oil reaches a line drawn across B. The vessel A is filled so that it just overflows at aa, and the temperature of the water bath adjusted by condensing steam therein. Simultaneously a stop watch is started, the stopper at O is withdrawn and the time noted which allows the oil to drop to the mark as described. The interval in seconds, or vis-

cosity, of a number of oils on this instrument is given in column 2 of the following table.

A second instrument on the same general principle as that of Mr. Saybolt is that of Mr. Davidson, chemist to the Chicago and Northwestern R.R. Fig. 104 shows it to consist of an inner vessel *A* to contain the oil for test, and a surrounding chamber *BB*, connecting with the auxiliary annular vessel *CC*. The inner vessel *A* is filled with oil to a certain level indicated in the gauge glass *D*, and a surplus of oil placed in the reservoir *E*, at the top of the instrument. The annular chambers *BB* and *CC*

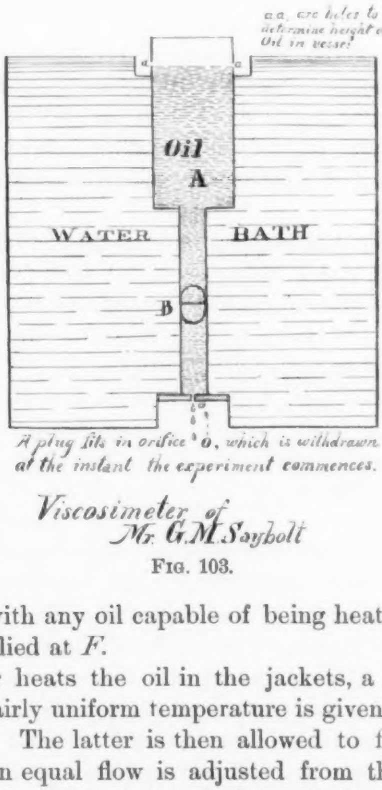


FIG. 103.

are then filled with any oil capable of being heated to 500° Fahr. and a lamp applied at *F*.

As the latter heats the oil in the jackets, a circulation commences and a fairly uniform temperature is given to the oil in the inner chamber. The latter is then allowed to flow through the orifice *O*, and an equal flow is adjusted from the surplus in *E* into the vessel *A*, so that the level of the oil therein is unchanged, the inflowing oil being heated to the same temperature as that in *A* by the waste heat of the lamp. When steady action is obtained, the time for a given quantity to flow is determined with a stop watch. With this instrument Mr. Davidson has determined

the viscosity of a range of lubricants intended for steam cylinders up to 350° Fahr., being the first to make determinations above 212°.

Column 3 of Table I. shows the results of the use of this instrument with the same oils to which Mr. Saybolt's apparatus was applied.

A third viscosimeter, shown in Fig. 105, is an entirely different

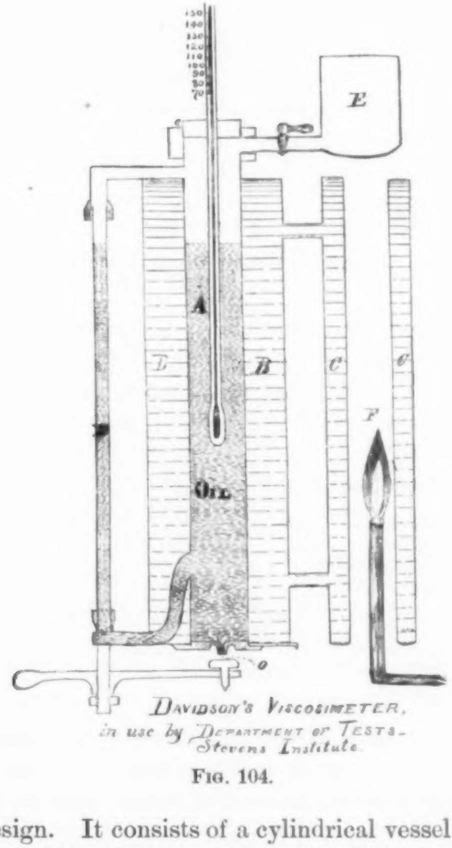


Fig. 104.

and novel design. It consists of a cylindrical vessel A in a proper heating bath and fitted with a piston B. This vessel being filled with oil, the piston is drawn up until the eye c in its rod is above the level of the cross-wire. The bath being at the proper temperature, the piston is released and begins to sink.

When the eye c passes the upper wire, time is noted with a stop watch, and the latter is stopped as the eye passes the lower wire.

This interval of time is taken as the measure of viscosity. Both the cylinder and the piston are of glass.

The piston is about $\frac{2}{1000}$ inch less in diameter than the cylinder. The cylinder is made with all the accuracy of bore that the highest optical talent can afford. This apparatus is so sensitive in its action that the viscosity of illuminating oils can be distinguished by its use.

Its designer, Mr. G. H. Perkins, Supt. of the Atlantic Refinery, Philadelphia, is a profoundly acute student of the physics of oils.

He has successfully applied the instrument to the determination of the viscosity of all varieties of oils, and the writer is indebted to him for painstaking coöperation in the examination of the oils under notice.

Column 4 of Table I. shows the results obtained with the Perkins Viscosimeter.

TABLE I.

SHOWING VISCOSITY OF OILS ON THREE DIFFERENT VISCOSIMETERS OPERATED WITH OILS AT 167° FAHR.

DESCRIPTION OF OILS.	VISCOSITY IN SECONDS.			RATIO OF VISCOSITIES.		
	Col. 2.	Col. 3.	Col. 4.	For Col. 2.	For Col. 3.	For Col. 4.
1 Heavy cylinder oil, pure petro.	346	138	523	1.000	1.000	1.00
2 Mixed oil {90% " " / 10% Neatsfoot.....	237	73	372	0.68	0.53	0.71
3 Castor oil	210	71	356	0.60	0.51	0.67
4 " " " {70% Petroleum.... / 30% Animal.......	215	62	261	0.62	0.45	0.50
5 Mixed oil {33% Light paraffine / 67% Heavy petro..	165	55	....	0.47	0.40	....
6 Lard	79	42	114	0.23	0.30	0.22
7 Tallow	....	40	....	....	0.29	....
8 Sperm	63	35	81	0.18	0.26	0.15
9 Spindle oil paraffine..........	53	31	53	0.15	0.23	0.10

The relative values of the oils, though not identical for the different instruments, show a fairly satisfactory agreement and the discrepancies shown are not of importance for the following inquiry.

The oils of Table I. being applied to lubricate the slide of an upright condensing engine, 44 ins. diameter, 36 ins. stroke, making about 80 revolutions with 70 pounds boiler pressure, with 90 square inches of area in the crosshead shoes on the working side of the slide, gave results as per column 2 of Table II., the slides being at a temperature of 167° Fahrenheit.

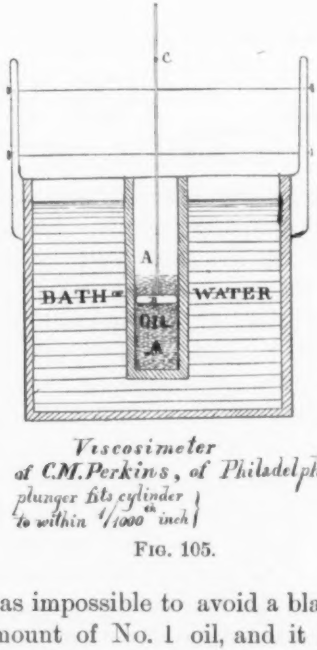
TABLE II.

COMPARING RELATIVE AMOUNTS OF OIL USED PER MINUTE ON ENGINE SLIDES UNDER HEAVY PRESSURE WITH RELATIVE AVERAGE VISCOSITIES OF TABLE I.

DESCRIPTION OF OILS.	Relative amounts of oil necessary to be supplied per minute to prevent wear of brass crosshead shoe from causing black streaks to appear on slide.	Reciprocals of relative viscosities from average of columns 5, 6, and 7 in Table I.
[1] Heavy cylinder oil, pure petroleum	1.0	1.0
[2] Mixed oil { 90% " " ; 10% Neatsfoot }	1.6	1.61
[3] Castor oil	1.8	1.66
[4] Mixed oil { 70% Petroleum ; 30% Animal }	1.6	1.90
[5] " " { ¼ Paraffine ; ¾ Heavy cylinder oil }	1.9	2.50
[6] Lard oil	4.5	4.00
[7] Tallow	4.6	3.40
[8] Sperm	4 6	5.00
[9] Light paraffine, spindie oil	60.0	6.00

A study of Table II. shows no uniform relation between the relative quantities of oil and the relative viscosity reciprocal, but it is very evident, nevertheless, that there is a sufficiently direct connection between these quantities to make the oils of greatest viscosity the most economical as regards the amount necessary to be supplied in order to maintain a minimum amount of wear; for it is believed that the circumstances of the experiment were such that the quantities of oil supplied to the slide corresponded to a constant thickness of layer of oil or a constant distance between the slide and the shoe. Reasons for this belief are as follows: The slides were at each side of the engine and of the rib form, *i. e.*, the shoes of the crosshead on each side received the slide between them. The surfaces of the slides as they were uncovered by the shoes could therefore be closely observed and the thickness of oil tested by wiping the surface with the finger sufficiently well to detect considerable variations of such thickness. Now, as the supply of oil No. 1, Tables I. and II., was reduced below the normal quantity in daily use, a portion of the slide on one side of the engine very soon became heavily streaked with black lather, while on the slide on the other side of the engine the oil remained clear. But on the latter side the fact of the presence of a reduced thickness of the layer of oil was very apparent by wiping with the fingers. The slide which blackened was therefore

examined with a microscope and found to contain a collection of brass specks where the black lather first showed itself, and the roughening which had occurred when this brass had imbedded itself was still slightly present, so that the distance between the shoe and slide at this spot was apparently less than at any spot in the slide which showed no black lather. It was therefore concluded that if the supply of oil to the brassy slide was adjusted with different oils so that the black lather was just commencing to appear, the distance apart of the shoe and slide would be the same with each oil. The figures in Table II. are therefore to be compared on this basis, with the exception of the spindle oil.



Viscosimeter
of C.M. Perkins, of Philadelphia.
plunger fits cylinder to within 1/1000th inch

FIG. 105.

With this oil it was impossible to avoid a black lather with even sixty times the amount of No. 1 oil, and it is therefore thought that for this oil the pressure upon the slides was too great to permit the entrance of the fluid under the slide as fast as was necessary, and that the majority of the oil used flowed over the advancing edge of the crosshead shoe. The pressure per square inch of surface of the crosshead shoe was upward of one hundred and forty pounds per square inch, which is from two to three times that of the most severe modern engine practice, such as the locomotive and high-speed stationary type. The engine was per-

forming very severe service, being coupled direct to a steel rail mill turning out the heaviest line of rails. The load had been increased beyond that for which the engine was designed, and hence the abnormal pressure upon the crosshead shoes. Oil was supplied to the slide from a vessel placed over its upper extremity and delivering the oil into a cavity in the top of the slide, whence it trickled down the three faces of the latter rubbed by the crosshead. The oil was heated to the temperature of the crosshead, 167° Fahr., before leaving the vessel by conduction from the steam cylinder. The rate of feeding was determined by catching a minute's supply in a bottle held under the stream of oil, and then weighing the contents of the bottle.

GENERAL CONCLUSION.

From the experiments with the engine slide described, it appears that the possible saving in the oil consumption necessary for minimum wearing away of the rubbing surfaces, due to the difference in viscosity, proves that the most economical lubricant is the oil of the greatest viscosity which will permit the oil to be fed, wherever the loss of power in friction is an element of inferior importance, as is the case in all heavy machinery.

DISCUSSION.

Mr. John F. Wilcox.—The last instrument described in the paper, that of Mr. Perkins, can be made an exceedingly useful little instrument in the tool room for any person who is in the habit of buying oil in any quantity, but in the shape in which Prof. Denton has it, it is necessary to observe accurately how long it takes for the piston to descend, requiring a stop-watch and close observation. But every man has, I think, the means of making a determination with regard to the relative viscosity of the oil. We have done it for some little while in somewhat the same manner that the Professor has. The instrument used by Mr. Perkins can readily be adapted by attaching a pencil to the upper end of the piston rod. Take the cylinder off an engine indicator, and attach it to the spindle of the minute hand of a Waterbury clock, and have the descent of the piston graphically described upon profile paper instead of having it tabulated. If there is such a thing as maximum viscosity it would be described in a horizontal line; the minimum or zero would be a vertical line. The passage of the

oil past the piston is approximately uniform, and consequently the line is straight and its angle with the horizontal would be a relative index of its viscosity, which, by means of a protractor, could be expressed in degrees and minutes if so desired.

A graphic demonstration carries an impression to most minds, and can be more easily retained by the memory than the series of tables which the Professor has obtained. When the oil agent comes in with his samples, he can be sent to the tool-room, and the keeper can, in a very few minutes, determine its angle of viscosity without a stop-watch, gauge or measure.

Mr. John. T. Hawkins.—I regret that the author of this paper is not present, so that we could ask him some questions on this subject. I think, myself, that the quality described as viscosity, and in a lubricant particularly, as measured by its flowing through an orifice, does not necessarily establish a value as a lubricant, and that its quality as a lubricant could be tested by other means more completely. What I mean by that is, that its ability to flow at a given temperature through an orifice does not necessarily measure its value as a lubricant. There are other properties of oils which could be measured in other ways which this means of investigation by its flow through an orifice would not describe or determine in any way.

Prof. Jas. E. Denton.—In reply * to Mr. Hawkins' criticism, I would say: The general conclusion drawn at the close of my paper I believe to be thoroughly consistent with all knowledge thus far derived from any and all definite methods of testing the lubricating values of lubricants. I should be glad to learn from Mr. Hawkins by what methods of testing the qualities of lubricants, such as are considered in the paper, he could invalidate its general conclusion, assuming the market prices of oils not to vary far from those now current.

* Author's closure under the rules.

CCLXXXVI.

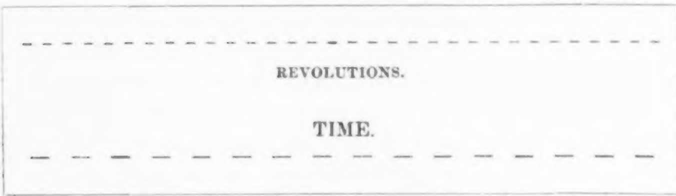
AN ELECTRIC SPEED RECORDER.

BY GEO. H. BARRUS, BOSTON, MASS.
(Member of the Society.)

In a paper read by Mr. W. R. Eckart, at the Philadelphia Meeting,* (April, 1882,) a description was given of an instrument for recording speed which was constructed on the principle of the chronograph. Recent experiments of the writer on the regulating qualities of certain steam-engine governors required the use of an instrument of similar character, and an apparatus was arranged for the purpose on the following plan:

The apparatus consists essentially of an instrument similar to a Morse telegraph register, in which there are two recording pens, each worked independently by an electro-magnet. The record is made upon a paper ribbon which is moved by clockwork at a speed of about two and one-half feet per minute. The magnets are operated by the alternate making and breaking of the electric circuits in which they are placed. In one case the circuit is made and broken by the oscillating movement of the escapement wheel of a clock, and this occurs 120 times per minute, or once in a half-second. In the other it is done by the revolution of the engine shaft, which is arranged so that during half of the revolution the two ends of the circuit are in contact, while during the remaining half the circuit is broken. The records consist of two lines of short indentations or dashes, made in the paper ribbon, and these lines are parallel to each other and five-eighths of an inch apart. The time record is made up of dashes about one-eighth of an inch long and one-eighth of an inch apart, that is, about one-quarter of an inch from center to center. The dashes making up the speed record are about one-sixteenth of an inch long, and, in the case under consideration, about half as far apart as those of the time record. A sample of one of the records is reproduced in the following copy:

* Trans., A. S. M. E., Vol. III., p. 184, No. LXVI.



The number of revolutions which take place during any desired period of time is determined by drawing perpendicular lines through the centers of those dashes which mark the beginning and end of the interval, and counting the number of speed dashes and fractions which lie between these lines. The record appears to be reliable to as small a fraction of revolution as one-tenth.

This instrument operated satisfactorily at a speed of 300 revolutions per minute. To show the utility of such an instrument, the record of one test is appended, which was made to determine the rapidity with which the governor operated when the engine was first attaining its normal speed, starting from a state of rest, the throttle valve being wide open and the engine carrying simply a friction load. The first part of the record shows the indications made when the engine was working at normal speed. The throttle valve was then closed for a time to allow the speed to become reduced; then it was opened wide and the governor allowed to take control. The record shows the number of revolutions made for intervals of two seconds during all this time:

Time.	No. of revolutions indicated by recorder.	Difference for 2 seconds.	Remarks.
4.18.55	802.4		
19.05	845.9	Av. 8.7	
15	889.4	Av. 8.7	Throttle shut at 4.19.15.
25	926.2	Av. 7.36	
27	932.2	6.	
29	937.7	5.5	
31	942.7	5.	At 4.19.30 open throttle [wide.
33	948.8	6.1	
35	956.2	7.4	
37	965.	8.8	
39	973.7	8.7	
41	982.5	8.8	
43	991.3	8.8	
45	1000.	8.7	
47	1008.6	8.6	
49	1017.3	8.7	
51	1026.1	8.8	
53	1034.6	8.5	
55	1043.4	8.8	

DISCUSSION.

Prof. John E. Sweet.—I hope Mr. Barrus will be induced to apply his speed recorder to the various high-speed engines, so that we may know something more about the actual results than we get from the reports of the builders. We all claim that our governors govern with perfect accuracy, and some of us work very close. Mr. Barrus' recorder will allow a variation of three turns in a minute, which would not sound very exact, but I fancy that with even that limit he would be able to detect the variation of any of the engines now built. The trouble is that the ordinary method of counting does not give any record at all worthy of consideration. It may give the same number of turns per minute when the engine is doing a good deal of work, as when it is doing but little work, while the speed may vary very largely during small intervals of time. With this instrument Mr. Barrus may be able to detect those changes. Prof. Anthony made a chronograph by which he could detect very much smaller errors than can be done by Mr. Barrus' recording instrument, but he never applied it to but one engine, so far as I know.

Mr. Barrus.—I would say that if this instrument is in operation a full minute, the error would be only one-tenth of a revolution a full minute.

Prof. Sweet.—I understood it one-tenth of a revolution in a half second.

Mr. Barrus.—One-tenth of a revolution in a full minute, or any length of time the instrument was in operation.

Prof. Sweet.—Suppose you wanted to determine the variation in a half second. That is really what we want to determine.

Mr. Barrus.—It would not be accurate with so small a variation as that.

CCLXXXVII.

A SHORT WAY TO KEEP TIME AND COST.

BY HENRY LEON BINSSE, NEWARK, N. J.
(Member of the Society.)

At several meetings, the attention of the Society has been called to some excellent systems for recording the costs of manufacture; but the writer has been unable to use any one of these systems for his own work: while they are admirably adapted to large workshops and for duplication, they take too much clerical labor for works engaged in the construction of a variety of single machines. As an example of this, the card system was very attractive; but a brief trial quickly uncovered the fact that good mechanics are often poor clerks, and the cards were turned in ill spelt, badly written and incomplete. To let the foreman fill out the cards would take too much of his time. The writer was satisfied that any records made by the men themselves were unreliable, because the hands did not like the task of thinking and writing; and they gave but little attention to accuracy, in their hurry to get the cards filled out. Men will describe to a time-keeper what they will not take the trouble to write down; and an expert time-keeper must often cross-question to extract the full and exact facts.

The usual way to keep time is to enter the work of each man at the close of the day in a sort of day book, and this is rewritten, charging the work to the various contracts. This involves a great deal of copying, as each item is dealt with separately. The question of keeping cost being of the first importance, the writer hopes that the system which he has adopted may turn out to be as useful in the work of other engineers as he has found it to be for his own. This system was devised by Mr. Arthur J. Frith, C. E., now with the writer.

In its essence, it consists in an analysis of the time, made by the time-keeper at the moment when he takes it from the men; so that the time book becomes a complete record of every man's work, of the cost of the contracts, of the total number of men at work on each contract, and of the wages paid. In new work which is not

to be duplicated, the exact cost of each item is seldom required. You need the total cost of the work and that of its principal subdivisions. Referring now to the plate, you will notice that for every man's name on the left side of the sheet there are six horizontal lines, one line for each working day of the week. The kind of work, whether slotting, vise, drilling, or whatever it may be, is noted in the first square to the right of these names. Then come the contracts, Nos. 49, 47, 53, and so on, with their leading subheads, the various "specials" being still farther to the right. The wages paid are shown at the extreme right.

The time-keeper, on his rounds, decides once for all to which subdivision the work belongs, enters the time in its square, and there is no copying to be done. At the end of the week, the hours each man has spent on *each subhead* are *summed up* and their value in money entered on a separate sheet, where the cost and weight of the raw materials are kept.

Now, of course, this way admits of endless variation. By making the Remarks column wider, the time-keeper can note down exactly what part of the subdivision the man was working upon. Columns for raw materials could be added; and by ruling seven horizontal lines to each man, you could sum up the weekly cost. It is obvious that by a few changes the record may be made as complete as the circumstances require. If it be inconvenient to have the paper ruled, profile paper can be made to answer.

WEEK COMMENCING JANUARY 8TH, 1888.		M.	T.	W.	T.	F.	S.												
		Smith						Jones						Sands					
		S.						V.	dr.	sl.		✓		M.	U.	out.	out.	dr.	V.
49	Head.					2	5							5					
	Bed.																		
	Attachment.	2																	
47	Head.					3	5							5					
	Tailstik.																		
	Bed.																		
	Carriage.	6	10	10	4														
	Apron.																	7	10
53	Head.																		
	Bed.																		
	Table.																		
	Carriage.																		
	Feed Motion.				3	5													
	Attachment.				3														
46	Frame.																		
	Tables.																		
	Ram.																		
	Driv. Motion.																		
50	Stanchions.																		
	Cross Heads.																		
	Table.								3	10	10		10						
	Bed.																		
	Feed and Driv. Mot.																		
52	Special.							9	7						10	10	10	3	
54	Special.																		
57	Special.																		
	Expense Shop.							1											
	Tools, M. T. F.																		
	REMARKS.																		
		8	10	10	10	10	10	10	10	10	10		10	10	10	10	10	10	10
							58						50						60
							30						30						25
							$17.40						15.00						15.00

M.	Brown....................	Sh.																	10														10			
T.		✓																																		
W.		✓																																		
T.																			1									9				10 lbs. iron.	10			
F.																				2								8					10			
S.					5												5																10	40	25	10.00
	S..........................		10																														10			
			9										1																				10			
			2														4	4															10			
				8		2																											10			
											2							8															10			
																3									1		3		3				10	60	22½	13.50
	K..........................		5			5																											10			
			5			5																											10			
			5			5																											10			
			5			5																											10			
			5			5																											10			
			5			5																											10	60	22½	13.50
	E..........................			4										3														3			Boiler repairs.	10			
														3								7											10			
																						10											10			
				3																		7											10			
											5											4											9			
											10																						10	59	20	$11.80

Amount Forward $96.20

WEEK COMMENCING JANUARY 8TH, 1888.

		F. M.	T.	W.	T.	F.	S.	W. M.	T.	W.	T.	F.	S.	D. M.	T.	W.	T.	F.	S.	REMARKS.
	Tools, M. T. F.																			
	Expense Shop.															5	10			1 7-16 reamer. 5 lbs. b. c. steel.
57	Special.																			
54	Special.														2	5				
52	Special.													10	8					
50	Feed and Driv. Mot.																			
	Bed.		7	10	10	5	10													
	Table.																			
	Cross Heads.																			
	Stanchions.																			
46	Driv. Motion.																			
	Ram.																			
	Tables.																			
	Frame.																			
53	Attachment.								1											
	Feed Motion.								6	10	8									
	Carriage.																			
	Table.							8	3		2	10	10							
	Bed.																			
	Head.																			
47	Apron.																			
	Carriage.																			
	Bed.																			
	Tailstik.																	5	4	
	Head.	10	3																	
49	Attachment.																			
	Bed.							2										5	6	
	Head.																			
		10	10	10	10	5	10	10	10	10	10	10	10	10	10	10	10	10	10	

	F.	W.	D.
Hours	55	60	60
Rate	20	16⅔	15
Amount	11.00	10.00	9.00

Brought Forward, $96.20

Amount Forward $126.20

NAME, *Smith.*

BEGIN YOUR FIRST MARK AT THE TIME YOU COMMENCE THE DAY.

Date.	No. or Description of Job.		7 — 7.30	8 — 8.30	9 — 9.30	10 — 10.30	11 — 11.30	12 — 12.30	1 — 1.30	2 — 2.30	3 — 3.30	4 — 4.30	5 — 5.30	6 Total Hrs. Day.	Overtime.
		TIME.						*Omit marking across the dinner time in this column.*							
Feb. 15,	8,435 *Catalogue, Jones & Co.,*	Alterations.												2¼	
		TIME.													
"	8,438 *Catalogue, Wesel & Co.,*	Alterations.												5¼	
		TIME.													
"	8,603 *Time Table, Erie R'y.*	Alterations.												2	
		TIME.												10	
		Alterations.													
		TIME.													
		Alterations.													
		TIME.													
		Alterations.													
		TIME.													
		Alterations.													
		TIME.													
		Alterations.													

[COPYRIGHTED, 1885.]

DISCUSSION.

Prof. F. R. Hutton:—On the previous page is a form of time blank, which has been worked out and copyrighted by Mr. R. Schalkenback and has come to my notice recently. While intended primarily for printers' use, yet it is also applicable to many manufacturing types of shop. It explains itself so completely that no further presentation of it seems necessary.

Mr. L. S. Randolph.—The author of this paper does not describe very fully the character of this work upon which his time system is used; but judging from the samples shown, I cannot help but think that his time system is better for getting the total time on each job than the card system.

I am using at the present time a card system, which was substituted for a system very much like the one described, and find it very satisfactory, as it enables me to obtain accurate detailed statements of the labor account.

Below is the form of our time ticket, and appended hereto is

M. P. Form 11-20,000-4 88.

TIME TICKET—C. & P. R. R. CO.

Mt. Savage, Md.,..................................188

Name,.. *Rate*,..............

Order No.	PIECE.	WORK.	No. of Hours.	No. of Pieces.
Correct,..........................		*Foreman*.		

a table of detailed cost of each operation. The lower figures in each square are hours, and the upper figures give cost.

We have very little trouble with defective tickets, not over one or two per cent. giving us any trouble. All tickets go through the foreman's hands, and are signed with his initials. This takes about fifteen minutes of his time, never more than twenty, and gives him an opportunity of noting carefully just how long each man takes for each operation. The men take from ten to fifteen minutes per day for making out tickets.

DETAILS S. O. NO. 1,105. BOILER FOR C. C. C.

	Laying off.	Flanging.	Punching.	Rolling.	Rounding.	Fitting.	Marking.	Riveting.	Chipping.	Cutting.	Caulking.	Drilling.	Tapping.	Forging.	Bending.	Threading, Turning.	Pattern Work.	Testing.	Loading.	Painting.
Boiler Shell	*6.50* 20		*6.67* 34	*6.88* 35	*2.90* 20	*.72* 5		*20.08* 170	*3.60* 25		*4.41* 30	*5.75* 39	*.52* 4					*3.12* 20	*2.07* 13	*1.12* 5
Flue Head..............	*1.62* 5	*11.66* 54	*.33* 2			*.34* 2	*1.02* 6		*1.61* 10			*5.89* 42	*.12* 1							
Dome..................			*.76* 6			*1.30* 8		*7.56* 52	*.90* 5		*1.12* 7	*2.64* 18	*.48* 3							
" Ring..............						*.32* 2						*.28* 2		*5.68* 24		*2.31* 11				
" Liner.............		*3.17* 17										*1.88* 8								
" Cover												*.63* 4½				*1.16* 5½	*2.00* 10			
Check Covers...........		*.65* 4						*1.48* 12				*1.75* 12								
Flues..................						*13.54* 69					*5.43* 28									
Fire Doors.............						*7.88* 45						*.28* 2					*2.00* 10			
" Irons						*2.93* 18								*1.90* 12	*1.05* 5					

DETAILS S. O. 1,105. BOILER FOR C. C. C.

	Laying off.	Flanging.	Punching.	Rolling.	Rounding.	Fitting.	Marking.	Riveting.	Chipping.	Cutting.	Caulking.	Drilling.	Tapping.	Forging.	Bending.	Threading, Turning.	Pattern Work.	Testing.	Loading.	Painting.
Angle Pieces		*1.40* 9	*.65* 4					*1.95* 12												
Knees								*.97* 6				*2.67* 10					*2.00* 10			
Breeching			*.70* 6			*6.39* 40		*1.75* 9	*.97* 6			*1.52* 10			*.98* 6					
" Pipe	*8.13* 25	*3.72* 22	*3.80* 26	*.65* 4		*4.60* 22	*.60* 4	*6.40* 38		*1.83* 11		*.82* 5			*3.58* 23					
" Irons			*.63* 5			*2.86* 19		*.61* 5				*1.30* 8		*2.33* 15		*9* 1				
Manhole Clamps												*.21* 1½		*1.81* 11						
Braces						*1.50* 9		*.37* 2				*2.04* 12		*.95* 6		*.12* 2½				
Steam Pipes and Fittings						*3.05* 16½														
Steam Pipe Flanges						*3.90* 14						*.59* 4	*.60* 4				*4.10* 20½			
Bolts and Studs						*.96* 6								*.78* 6		*2.50* 31				

Mr. H. L. Binsse.—In answer to Mr. Randolph,* I would say that the system has been used in a shop building a variety of machines. It would seem that Mr. Randolph's system is used for duplicate work. It is evident that his cost sheet has taken considerable labor, but in a shop engaged constantly in new work this clerical labor would be multiplied, and reach an amount not justified by its value. Most work-shops need the total cost of each machine and its principal sub-division. The system used by me gives this with so little labor, that the cost of each job can be summed up at the end of the week in an hour's time without copying a single figure. For duplicate work the system would need change to adapt it to each shop; and, as the system is very elastic, it can be made to serve for that as well as for the other. The great advantage which it possesses over all other methods familiar to the writer is that it gives a complete view of the day's work, the number of men engaged on each job, what they were doing, as well as their rate of wages; and all this without copying a word. It is no exaggeration to say that, in my own case, I have obtained a more comprehensive view of my work than I ever had before, with one-tenth the labor.

* Author's closure under the rules.

CCLXXXVIII.

ESTIMATING THE COST OF FOUNDRY WORK.

BY GEO. L. FOWLER, NEW YORK CITY.
(Member of the Society.)

To make a correct estimate of the actual cost of castings, as they leave the foundry, is, perhaps, one of the most difficult of the duties which devolve upon the engineer in charge of any manufacturing establishment. There are so many little insinuating expenses, which can only be determined with the utmost difficulty, and which vary so constantly, that many managers give up the task as a hopeless one, make a rough guess, and establish an error at the very basis of all their calculations. It is an easy matter to open an account with the foundry, and after six months or a year to foot up the expenses for wages, iron, fuel, rent, moulding material, and all of the thousand and one little incidentals which will come up, divide the total by the output in castings in pounds, and say that the latter have cost two, or two and a half, or three cents per pound, as the case may be. But this is of very little account. Suppose that we attempt to sell all castings at a constant price, and make a fair profit above the average cost. The inevitable result will be that we are overcharging one class of customers and losing money with others. One piece of work may be worth twice or even ten times as much per ton as its neighbor, and yet no system of general averaging will point it out. The only way, then, is to make a detailed estimate of each heat, and ascertain what has been the cost of each individual casting produced.

It will be attempted in this paper to show, briefly, the outline of a plan which the writer has used most successfully in his own practice.

The first point to determine, before developing any system of estimating, is the basis to be employed about which all of the expenses shall cluster. If we take the weight, we are reduced to the old method of averaging all expenses at a certain rate per pound. It is evident, therefore, that the expense of moulding is the basis upon which all estimates and calculations should be founded.

coupled with a careful consideration of weights, and the cost of iron and fuel.

For the sake of convenience, we may divide our expenditures under the heads of the cost of iron, wages to moulders, and moulding materials, as sand, blacking, coal dust, etc., the loss of iron consequent on melting, the expense of superintendence and repairs, cost of cleaning, helpers, office and other current expenses, and pattern-making. The first step is to arrange the outlay under these various heads, and determine in what relationship they stand one to another. It is evident that the price of a piece of work which costs $2 per ton in moulders' wages exerts a very different effect on the average cost of the output from one costing $10, so that great care should be exercised in determining just what the actual expense of moulding each individual piece may be; for, as we have already said, this is the principal factor in all of our calculations. These figures should be obtainable from the books of any well-regulated establishment; but they are not yet arranged so that they can be used for the direct purposes of an estimate, since there should be a rearrangement and modification of those items which can in any way exert an influence upon the cost of any special articles which may be under consideration; whether they are dependent upon its weight or are in direct connection with the expense of moulding it. A heavy casting may require little time or expense in moulding it, and a light one a great deal; therefore those items which are particularly connected with the weight must be separated from those which relate to wages. We then formulate from the table which has been already prepared an auxiliary account, comprising: the cost of melting; a certain percentage which rises and falls with the expense of moulding, and is independent of weight; a business factor, including maintenance of buildings, salaries, interest, etc. This latter is practically constant, though its ratio does rise and fall inversely with the increase and decrease of business. All of the expenses of the establishment are gathered around these two heads, namely: weight and moulders' wages. We see now that the expenses of melting, including the cost of the iron and fuel, the service of the cupola, the clearing up of the drop, and the cleaning of the castings, are items directly connected with the weight, and that they are apt to vary, per pound, inversely with the total weight of castings produced, because the labor of preparing and serving the cupola and the fuel used in making the bed is constant, leaving only the total of the remaining tolerably constant

ratio of the several charges to bear the whole expense. Under the domain of the moulders' wages we bring all of the other expenses of the establishment. It may be true that there are some expenses which do not stand in a direct, actual constant ratio to the wages paid to the moulders, but the variation from this ratio is so slight that no error will be introduced into our reckoning if we assume this constancy to be perfect. Thus we group about this item the wages paid the helpers, salaries to clerks, expenses for superintendence, moulding material, etc. This latter may appear to be misplaced at first sight, but careful observation has convinced the writer that the ratio is nearly constant. For those heavy and cheaply-made moulds, where the weight runs up into the thousands, take no more dressing, in the way of blackening, facings, etc., than the smaller and more elaborate moulds which require many more times the amount of labor per hundred pounds than the heavier ones.

This is the general plan, and we will now give a brief elaboration of the details, which will consist of obtaining the cost of the iron and the ratios existing between the moulders' wages and the following items of expense, namely: moulding materials, expense of superintendence, business expenses, depreciation of property, wages of helpers, and pattern-making.

We have now grouped all of the expenses of the establishment about the two chief heads, moulders' wages and the weight, to which they bear a certain ratio, and proceed to determine the amounts and proportions of the various minor items.

First taking the expense of melting, we tabulate a schedule which will show the amount of wood used in starting the fire and heating the ladles, the coke and coal required in making the bed, the fire-brick and clay, expense for banking, rounding up, and charging. These expenses are, or ought to be, nearly constant for each heat, as all of the items mentioned are required in the same amount, whether the heats be heavy or light. Add then the cost of the iron and fuel used, and the expense of melting for a given heat is obtained. It will be readily understood that, given the expense of preparation and the amount of iron melted, with the ratio of the fuel used in the charging, the whole melting expense per ton of castings is ascertained in a few moments.

To determine the ratio of the moulding materials to the wages of the moulders requires some time—months in fact, at first; but when once obtained can easily be kept accurately by occasional reference to the books. It is best, when starting out to use this for-

mula, to refer to the books and take the ratio existing between the amounts paid for facings, riddles, etc., and the moulders' wages for two, three or more years, and adjust it from time to time by reference to current expenditures. This is, in fact, the only way practicable with any degree of convenience, for it would be a work of too great detail to weigh out all the articles actually consumed, and estimate the wear and depreciation on the tools for each heat. The ratio of the waste of iron to the total amount melted can only be determined by experience, and no absolute rule can be given for it, as the percentage varies with every cupola, and even with the kind of fuel and iron used; but when once determined, it will be found to remain tolerably constant, and may be verified by dividing the weight of castings, scrap, sprues and shot for each heat by the total weight of iron melted, and subtracting the quotient from one.

We have now to deal with the most obscure of all of our items of expense. It is that incurred by interest on borrowed capital, salaries of superintendent and clerks, office expenses, taxes, insurance, and, with the rest, that bugbear of all estimates—depreciation of property. The total of all of the items but the last, is very easily obtained, while the last must be carefully estimated with due consideration for all of the surroundings. When this is done, it is best to make a studied review of the business for several years, learn what has been done, what was the average of the pay-roll for brisk times, dull times, and when only a fair or medium business was being done. Take these three items and compare them with the present status, and the probable total pay-roll for the coming year: this will give a ratio or percentage to be added to each and every dollar paid to the moulders, and should be of course incorporated in the expense of casting. To be sure to establish this ratio and make it exact is a work which should be thoroughly done, but when once accomplished, it should be in such shape that a simple reference to our pay-roll, which will show whether we are doing a brisk, medium or a dull business, will determine which coefficient should be used for the month. Thus far the assumption has been that the office expenses for the foundry are distinct; but where, as is almost universally the case, the foundry is connected with a blacksmith and machine shop, with possibly other departments care should be taken to assign to the foundry only that portion of, the general expenses which belongs to it.

Finally we have to assign to its proper place an expense which the foundry must pay for, and which is usually treated as a separate

department. I refer to the expenses of the pattern shop. Where a shop is doing special work, this expense may consist of the repairs and construction of patterns for the various machines that are built, and the labor and material therefor may be directly charged without burdening the foundry with them. But in most cases, especially in small works, where general work prevails, where patterns are altered and made to serve a variety of purposes, this with the expense of flask repairs must be put upon the foundry. Again, perhaps at the same time with this general work, there may be a class of foundry work where the moulding is done by machine and no expense is required for pattern repairs, and none should be charged to it. The ratio of pattern repairs to moulders' wages is then really easily determined, but it should be remembered that the foundry is to pay for no new patterns, but simply the depreciation on the same.

Having now tabulated our formula and established our ratios, it only remains to make the practical application thereof. To do this, the time-keeper should each day take the time of each moulder, and the work upon which he has been engaged. This gives the basis to start upon. Add together the various ratios already determined, multiply by the wages paid, to this add the wages and the cost of the iron in the casting, and the cost of that casting is known. For example, suppose that forty cents is paid for moulding a grate bar weighing 100 lbs.; that the sum of the several ratios is 1.2, and that the cost of iron and fuel has been one cent per pound of castings; then:

Sum of all ratios multiplied by the moulders' wages	.48
Moulders' wages	.40
Cost of iron	$1.00
Total cost of the grate bar	$1.88

It must be remembered by those who are disposed to criticise the method here enunciated, that there has been no space within the limits of this paper for any elaboration of detail. It is due both to myself and the originator of the plan, Herr A. Messerschmidt, of Essen, Germany, to say that he has used it in his own practice for seventeen years, and that, though my own experience with it has been for a shorter time, I can say that it has more than come up to my expectations for accuracy and the ease with which it can be used. By adhering to its principles, several unsuspected leaks have been discovered and stopped, and it has been possible to adjust

prices more in accordance with the real value of the article sold than was possible before the plan was employed. To those who may object to its elaborateness, I can say that an hour to an hour and a half suffices to work out the details of a heat of from five to seven tons, and determine the cost to the manufacturer of each individual casting produced, and this when the output is composed of miscellaneous job castings; and that, when the heat is entirely on line work, the estimate can be made in from fifteen to twenty minutes.

Discussion.

Mr. W. W. Dingee.—As a contribution to the subject of cost of foundry work, I would say that for several years I was employed as superintendent of a manufacturing establishment where the casting accounts were kept under headings, as per the accompanying table, which represents four days' work, and showed that the cost of casting per pound varied from day to day in this establishment. The work consisted of bench and floor moulding, the heaviest piece of the latter weighing 500 pounds.

At the end of each year the foundry was charged with its proportion of power, superintendence, office expenses, interest on investment, depreciation of plant, etc.

The cost included cleaning and grindstone work on castings, and putting in bins ready for the machine shop:

Iron Netted.	Casting made.	Return Scrap.	Shrinkage.	Coke used.	Cost of Iron.	Cost of Coke.	Cost of Labor.	Cost Sand Fencing, etc.	Total.	Cost per lb. Casting.
Lbs.	Lbs.	Lbs.	Lbs.	Lbs.	$	$	$	$	$	$
14425	10682	3120	623	2346	127.47	10.39	147.55	18.80	304.21	.02,846
14850	11150	3693	7	2312	128.07	10.72	146.50	19.62	304.91	.02,734
14950	11053	3327	570	2278	130.71	10.54	145.35	19.45	306.05	.02,769
15000	11170	3648	182	2312	126.24	10.72	145.47	19.65	302.08	.02,704

Mr. Thos. D. West.—It affords me much gratification to note the presentation of such a paper before the Society. Foundrymen, as a rule, are certainly working much in the dark as to what it costs per pound to manufacture castings. The trade is about as badly off for intelligence in the office as on the floor. There must be intelligent figuring as well as moulding to make the business profitable. I could step into almost any machinery foundry and point out work which is costing from one cent to ten cents per pound more to make it than is being allowed or paid for it. Machine

shops do not work this way, and why should a foundry? They figure to make a little, or at least come out even on every job. The present plan with foundrymen of bidding by bulk or without classification is ruining the business. Mr. Fowler's able paper could well be put in the hands of most of the foundrymen, if for no other reason than to get them to suspect the advisability of being informed how to bid on work intelligently.

Mr. J. T. Hawkins.—I find on the second page that Mr. Fowler winds up the items among which the expenditure should be divided with "pattern making." I do not think it would be advisable to include that in the cost of the casting. We make many castings but once from a pattern, while other patterns we use eight or ten years in succession, making castings, perhaps, every day from them. It would therefore be very hard to determine what rate the value of the pattern should bear to any casting made from it.

Prof. J. E. Sweet.—Mr. Fowler ends up by saying that he only counts keeping the patterns in repair, which is a part of the foundry work.

Mr. Hawkins.—The paper names "pattern making." That is what I object to.

The President.—In the establishment of Cramp & Sons they have no foundry, but make all their patterns, employing something like sixty pattern makers.

*Mr. Fowler.**—The system of foundry accounts presented by Mr. Dingee, while it is undoubtedly better than none, is precisely that to which I object. It gives the average cost of the total output, without specifying the cost of any individual piece of work. This may be all very well for shops doing line work, for which one standard price must be charged, but is useless for the job shop whose castings include every conceivable thing that can be made out of iron. The only way in which accurate and just prices can be given is to know the cost of each article produced, and only that system is a perfect one which will give this information.

The pattern making, which I include as one of the items of foundry expense, will be found by examination of the paper to include only pattern repairs, and it is distinctly stated on Page 5 that "the foundry is to pay for no new patterns." The patterns are usually inventoried with the other property, and should not be added to the cost of the castings.

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* Author's closure under the rules.

In my own experience the operation of the system was about like this: When we were estimating on a piece of work which would cost us four or five cents per pound to make, our price was necessarily such that we almost invariably lost the job; it went to our competitors, who took it at a figure at which we would have lost money. Then, when we found our price dropping down to one and a half or two cents per pound, we made a price yielding us a good profit, yet so far below our competitors as to make them think that we were wild, and we secured the paying contracts. Our reputation for unaccountable jumps in prices was a little disagreeable, but we knew what we were doing; the foundry was a paying institution and we did not find ourselves saddled with losing contracts of which the machine shop would have to stand the brunt. In short, the system worked, and we made money by its use.

CCLXXXIX.

A PERSISTENT FORM OF GEAR TOOTH.

BY J. BURKITT WEBB, HOBOKEN, N. J.
(Member of the Society.)

In discussing the subject of Friction in Toothed Gearing at the last meeting of this Society,* I proposed the following problem: "Required to find a form of tooth which will preserve its shape in spite of wear, *i.e.*, to find a *persistent form of tooth*, if such there be, the number of pairs of teeth in action at once being the same for the whole of the arcs of contact."

The discussion of a problem so general in its form as this is requires a correspondingly general treatment, and therefore the method by which the wear was calculated for involute teeth (see the discussion, Fig. 90, and accompanying analysis) is not adapted to the purpose of such an inquiry, being applicable to involute teeth only. The object of this paper will therefore be to develop a formula for the wear of teeth, applicable to all possible forms of the same, and the *equations of condition for a persistent form of tooth.* The limits assigned will, however, preclude a discussion of the equations, and, therefore, a complete solution of the problem; we shall, however, show that the equations produced will apply to both involute and cycloidal teeth.

In Fig. 106 let A be the radius of the driver and B that of the driven wheel; these radii should properly be supposed measured from C to O' and C to O'' respectively, and not from the centres toward the circumferences, and we have chosen an internal gear for the driver, so that both radii may be plus. Let C be the point of contact of the pitch circles and c that of the tooth surfaces of which ca is a differential element of the driving tooth and cb an element of the driven tooth. Let C', C'' and C''' be corresponding points on A, B and their tangent through C such that when the tooth elements ca and cb have slidden over each other so as to bring a and b together, these points will arrive at C and become coincident.

* Trans. A. S. M. E., Vol. IX., p. 206, No. CCLXXIII.

The necessary and sufficient condition for two elements of tooth surfaces that shall work together and maintain the constant velocity ratio $A \div B$ is that the common normal to the surfaces at their point of contact, c, shall pass through the contact point of the pitch circles, C. This normal, $\lambda = cC$, consists of two coincident normals, one on the driver and one on the driven wheel; the consecutive normal to the former is aC' and to the latter bC'', and these will become coincident when a and b come in contact.

To obtain expressions for the lengths ac and bc of the elements corresponding to the element dt of the tangent, we will suppose the

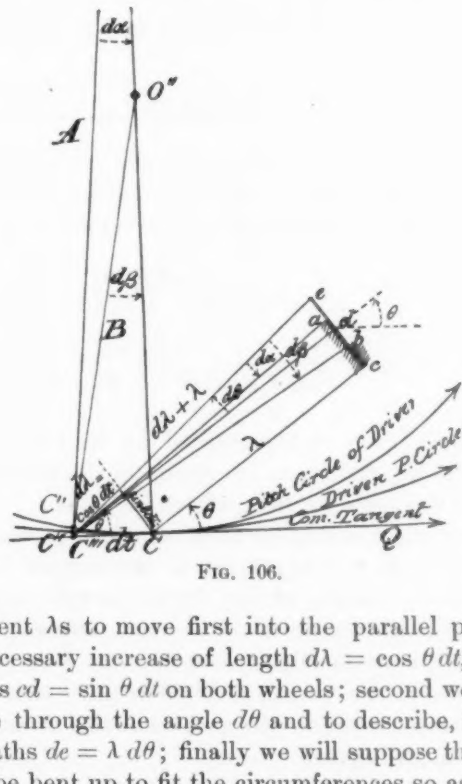

FIG. 106.

two coincident λs to move first into the parallel position dC''', with the necessary increase of length $d\lambda = \cos\theta\, dt$, thus describing the paths $cd = \sin\theta\, dt$ on both wheels; second we will suppose λ to revolve through the angle $d\theta$ and to describe, therefore, the duplicate paths $de = \lambda\, d\theta$; finally we will suppose the tangent element dt to be bent up to fit the circumferences so as to bring the λ's into their final positions and thus complete the generation of the two tooth-elements. When the tangent element is bent to fit the driver, one λ will be carried from the position eC''' to its final position aC', and when the element is bent to fit the driven wheel that λ will be carried from eC''' to bC'', the respective angles

through which the λs will be revolved by the bending being $d\alpha = dt \div A$, and $d\beta = dt \div B$.

We have then for the lengths of the tooth-elements,

$$ca = cd + de - ea = \sin\theta\, dt + \lambda\, d\theta - \lambda\, d\alpha,$$
$$cb = cd + de - eb = \sin\theta\, dt + \lambda\, d\theta - \lambda\, d\beta,$$

and for the sliding between the same

$$ca - cb = \lambda(d\beta - d\alpha)$$

If Q is the pressure along the tangent which would exist between two tooth surfaces at C at right angles to the tangent, then the pressure P at c will be

$$P = Q \div \cos\theta.$$

The normal depth of wear being proportional to the sliding multiplied by the pressure and inversely proportional to the surface over which the wear is distributed, we shall have

$$dn' = m' \frac{Q}{\cos\theta} \frac{\lambda(d\beta - d\alpha)}{\sin\theta\, dt + \lambda\, d\theta\lambda - d\alpha}$$

$$dn'' = m'' \frac{Q}{\cos\theta} \frac{\lambda(d\beta - d\alpha)}{\sin\theta\, dt + \lambda\, d\theta - \lambda\, d\beta}$$

where the normal wear is represented by dn and where m' and m'' are constants depending on the ability of the material to resist wear; if this is the same for both wheels $m'' = m'$.

If the form of the tooth is not to be changed by the wear, then the wear at any point, measured not in the normal direction but perpendicular to the radius of the wheel through the point, must be proportional to the length of the radius out to the point. Such w r will result simply in slightly rotating the face of the tooth about the center of the wheel, and will require that the normal wear shall be proportional to the perpendicular from the center upon the normal; this gives

$$dn' : A\cos\theta \text{ and}$$

$$dn'' : B\cos\theta, \text{ or}$$

$$\frac{dn'}{\cos\theta} = \text{constant}$$

$$\frac{dn''}{\cos\theta} = \text{constant}.$$

We may now write the complete equations expressing the relations necessary for the wear which will not change the form of the teeth :

$$\text{Constant} = \frac{dn'}{\cos\theta} = m' \frac{Q}{\cos^2\theta} \frac{\lambda(d\beta - d\alpha)}{\sin\theta dt + \lambda\, d\theta - \lambda\, d\alpha}$$

$$\text{Constant} = \frac{dn''}{\cos\theta} = m'' \frac{Q}{^2\cos\theta} \frac{\lambda(d\beta - d\alpha)}{\sin\theta\, dt + \lambda\, d\theta - \lambda\, d\beta}$$

To put these expressions in a simpler form, we eliminate $d\alpha$, $d\beta$ and dt by means of the values already given for $d\alpha$, $d\beta$ and $d\lambda$, at the same time putting for brevity $y = \sin\theta$ and including all the constant quantities, Q, m', etc., under the two quantities, M' and M''. We thus get

$$\frac{1}{1-y^2} \cdot \frac{1}{\frac{y}{\lambda} + \frac{dy}{d\lambda} - \frac{1}{A}} = \frac{1}{M'}$$

$$\frac{1}{1-y^2} \cdot \frac{1}{\frac{y}{\lambda} + \frac{dy}{d\lambda} - \frac{1}{B}} = \frac{1}{M''}$$

Clearing of fractions these become

$$\frac{dy}{d\lambda} + \frac{y}{\lambda} - \frac{1}{A} - \frac{M'}{1-y^2} = 0,$$

$$\frac{dy}{d\lambda} + \frac{y}{\lambda} - \frac{1}{B} - \frac{M''}{1-y^2} = 0,$$

which are respectively the *equations of condition for a persistent form of tooth* on the wheels A and B. If such a relation between λ and θ is practicable, then that form of tooth will be a persistent form; but, having reached the limit proposed for the present paper, we shall conclude it by showing, as a check on these equations, how they cover both epicycloidal and involute forms, and reserve further discussion of them for the future.

For epicycloidal teeth on A,

$$\lambda = 2Ay \text{ and}$$

$$d\lambda = 2A\, dy,$$

so that $y \div \lambda$ and $dy \div d\lambda$ are both constants and the equation of condition for A reduces to

$$\text{Constant} = \frac{M'}{1 - y^2},$$

which is impossible because y is a variable. For large wheels, however, $1 - y^2$ will vary very little, so that this form of tooth nearly satisfies the equation.

For involute teeth on A the angle θ and, consequently, y are constant and $dy = 0$, which reduces the equation of condition to

$$\frac{\text{Constant}}{\lambda} = \text{constant},$$

which is impossible because λ varies, and as λ varies considerably, this form of tooth is far from satisfying the equation of condition as shown in the discussion at the last meeting.

CCXC.

A PLEA FOR THE PRINTING PRESS IN MECHANICAL-ENGINEERING SCHOOLS.

BY JOHN T. HAWKINS, TAUNTON, MASS.

(Member of the Society.)

COEVAL and contemporary with the steam engine, the printing press made its advent and has been developed; and the world is to-day indebted to the latter, for the progress made during this century, if not in an equal degree with, at least second only to, the former. The steam engine and the printing press have long been held up as the two great civilizers, as the two fields of human effort, to the cultivation of which the extraordinary progress made in human affairs in the nineteenth century is principally due. With these, the plow, the loom, the cotton gin, etc., have been each, in a lesser degree, compared; and in later years electricity, in its manifold applications, has come to exceed in importance any of the results of man's ingenuity, except, possibly, the first two. It will probably be nothing to the disparagement of electricity to place it third in the catalogue of the most important factors in human progress, with the possible potency and promise that in the near future it may attain to the front rank.

In the mechanical-engineering courses of our technical schools, the steam engine, while to some extent a specialty, is made a somewhat broad and general field of mechanical study, and is very properly given great prominence; and involving, as it does, the application of the important study of thermodynamics, it is doubtful if electricity and its application can ever equal it in importance. In later years, with the extraordinary advancement made in the latter science, it has received more marked attention and a more prominent place in the curriculum of the mechanical engineer, as its importance deserves, as well as coming to the higher level of constituting a general course of study and furnishing a separate degree for the mechanical engineer. Of course, among the vast variety of special fields of mechanical study, it is not to be expected that, in the college course, any particular attention can be given to more

than a very few of the most prominent subjects; and we very properly find among them the steam engine and other motors, electricity and its appliances, and such as hoisting and pumping machinery, textile machinery, machinery of transmission, etc., a special study of which constitutes more or less a part of the curriculum. But in no single instance can the writer find any indication, in printed courses of study or by any other means, that the printing machine, with all its prominence in the mechanical world and its importance as a principal factor in the progress of civilization, has ever received any greater attention in our schools than is involved in the study of fundamental and general mechanical and engineering subjects applicable to it; and perhaps most astonishing, he fails to find the printing press once mentioned in the registers, or catalogues, of the four of our best technical schools, in connection with the study of mechanical engineering, which he has had the opportunity to consult.

Taking the registers, or catalogues, of these four schools, the writer finds the following special studies named and conditions affecting this subject:

First—Stevens Institute of Technology.

Among text-books of special subjects, we find: "Barr on Boilers," "Heminway on the Indicator."

In the course of instruction: third year, first term—construction of valve gears and link motions; second term—mechanism of engine, boilers, steam indicators, foundations, boilers; third term—machine design, boilers, hydraulics; fourth year, second term—steam engines, hydraulic motors, including the turbine.

Among books of reference are: "Proportions of Steam Engines," "Work-shop Appliances," "Steam Boilers," "Steam Engines," "Lowell Hydraulic Experiments," "Manual of Marine Engineering," "Manual for Railroad Engineers," "The Windmill," etc.

The inspection tour of Class of '87 includes the following: Iron and zinc works, machine-tool works, locomotive works, shipyards, automatic screw machinery, manufacture of repeating rifles, heating and ventilation, turbine wheels, paper mills, inspirator manufacture, pumping engines, sewing machines, city water supplies, cotton mills, etc. And, as though printing-press works contained some deadly miasma, which could not be safely approached, the latest attempt at setting type by machinery was ventured upon as being as near as was advisable to get to it; but with this exception, printing or printing machinery is not even suggested.

Out of two hundred and forty-two alumni, the subjects of whose theses and present occupation are in nearly all cases given, not a single thesis has the remotest connection with printing machinery, while but one is now engaged therewith, and this gentleman's graduating thesis was upon the subject of "Petroleum Illumination."

Second—MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

Special studies mentioned in the courses: second year, second term—mechanism of shop machinery; third year, first term—slide valve, link motion; third year, second term—steam engineering; fourth year, first term—hydraulics, engine-lathe work; options—marine engineering, locomotive construction, mill engineering; fourth year, second term—hydraulic engineering; options—marine engineering, locomotive construction, mill engineering.

Under "Methods and Apparatus of Instruction," we find, during the last school year, a series of lectures on the following subjects: The Indicator, The Locomotive, Shop Management, Naval Architecture, and Cylinder Condensation.

In the laboratory of mechanical engineering, we find three steam engines, five boilers, a steam pump, a turbine wheel; cotton machinery, consisting of a card, drawing frame, speeder, fly frame, ring frame, mule, and a number of looms; apparatus for testing injectors, etc.

Among graduates from this institute from 1868 to 1887 inclusive, there are chief and assistant engineers of railroads, chief engineers of water works, builders and superintendents and chief draughtsmen of steam-pump works, superintendents of iron and steel works, cotton mills, tool works, sugar refineries, wire works, slate works, railroad-car works, ice-machine manufactories, paper mills, petroleum refineries, scale works, steam-gauge and valve companies, vacuum-brake works, marble works, thread and twine companies, wall-paper manufactories, nail works, wood-working machinery, etc.; while not a single one in any way connected with the "art preservative of arts" or the construction of machinery for it.

Under the titles of fifty-eight theses of successful candidates for degree of bachelor of science, for 1887, not a single one has the remotest connection with the printing machine.

Third—CORNELL UNIVERSITY:

Page 78: "The closing work of the course consists of the study by text-book and lecture, of the theory of the steam engine, and other motors."

Of graduate courses (page 81), we find: "Electrical Engineer-

ing, Marine Engineering, Mining Engineering, Steam Engineering." And among nine strictly mechanical-engineering theses of distinguished excellence, for 1887, not a single one has the slightest hint of the printing press.

Page 106: "Steam engines and other motors, thermodynamics and the theory of the steam and other heating engines, structure and operation of engines, steam generators, etc."

Page 118: "Senior year: Steam engine and other motors, for both fall and winter terms."

Fourth—LEHIGH UNIVERSITY:

Page 58: "During the course there are frequent visits of inspection to engineering works, both in and out of town, with special reference to such subjects as machine elements, prime movers, machinery for lifting, handling, and transporting, and machinery for changing the size and form of materials."

Page 61: First term—boilers—strength, construction, and wear and tear of boilers.

Page 62: Second term—machinery of transmission, senior class, first term—link and valve motions, quick return motions, parallel motions, hoisting machinery, accumulators, cranes and locomotives; second term—pumps, pumping engines, blowing engines, compressors and fans, machine design, calculating and working drawings for the following machines: drilling, shaping, milling, shearing and punching machines, hoisters, pumps, and stone-breakers.

Among seventeen theses by the graduating class of '87, of a strictly mechanical-engineering character, but one bears the least relationship to printing machinery, and that exceedingly remote, viz., design of machine for binding books.

Among the list of graduates as mechanical engineers from 1869 to 1887, ranging among special professions similar to those mentioned for the Massachusetts Institute of Technology, there is not a single mention of one in any way connected with printing machinery.

The printing machine of to-day certainly deserves a higher place in our schools, as a specialty in applied mechanics, than is indicated by the foregoing; and it may reasonably be assumed that such a total absence of anything like attention to this branch of the mechanic arts in the school courses accounts for the rather astonishing fact that, in the long list of graduates from the four schools mentioned, but one appears as having entered this field, even in the remotest connection.

The writer has recently been in receipt of a very commendable circular from one of the colleges named above, inviting machine manufacturers to find places for young graduates, as a means of starting them in their particular line; and he will say that if any of the eighty-eight graduates of this school were as well fitted to make a beginning at the manufacturing of printing machinery as the records and printed courses of the schools show them to be in other special branches—some of them of vastly less importance—it would be much easier for him to comply than it now is.

Now, while the writer would not counsel the abandonment of any of the above-quoted specialties of mechanical engineering to give place to the printing machine, it would seem, in view of the acknowledged comparative importance of the latter, that it should have a place among them, even at the expense of a portion of the time devoted to some of them, and possibly (bearing in mind that, in the time allowed, the total quantity of this kind of study must necessarily be limited) to the exclusion of some of the least important.

The writer feels—and his experience sustains him in it—that the printing press has received too little special consideration or study at the hands of our professors and instructors in mechanical engineering; and that, with greater attention given, and more importance attached to it, they would be likely to open up an extensive field which the graduate may profitably enter, and from which he is now in a great degree, excluded, for the reason that the printing press, as one of the most prominent specialties in mechanical engineering, is given little, if any, more attention at school than is involved in the study of the more abstract subjects which he may subseqently apply to it, if opportunity offers.

A graduate, entering an establishment in which steam, electric, hydraulic and textile machinery, or even such as hoisting, crushing, and grinding machines and machine tools are produced, has the advantage of already having some knowledge of what may be called the technics of the particular branch he has adopted; but the printing-press manufacturer finds none so well prepared to commence in his field, because of the lack of consideration given it as a special study, while probably a more important branch and involving the refinements of mechanical study more than any abovementioned, with the exception of the steam engine and electricity. The writer believes that the manufacturer of printing machinery, in common with the young mechanical-engineer graduate, suffers

to-day because of the above facts, and that the printing press is losing much of the talent fostered and acquirements gained in our best mechanical schools, because of the comparatively cold shoulder it receives as a desirable branch of special study by them.

The writer hopes that his friends, the college professors, in discussing this paper, will not imagine that it is written in any fault-finding spirit. In the discussion of a recent paper on a cognate subject, it was remarked that it had become quite fashionable of late to pick the colleges to pieces; and, as it may be thought that the writer is actuated by a desire to be in the fashion in this respect, it may be well to disclaim, once for all, any such motive, and to say that, having been denied in his youthful days the privilege of pursuing any such admirable and invaluable course of preparation as is now the good fortune of our sons to be able to obtain, he entertains perhaps a greater admiration for the technical schools and their present work than would have been likely if he had been a recipient of the favors any one of them now affords; and it is due to these considerations that he feels it a duty to offer anything which appears to him to be in the least likely to improve them or be of advantage to the young men, in whose behalf they have been established.

DISCUSSION.

Mr. Wm. Kent.—I have listened to this paper with a great deal of interest, and think it is a very good one. If I understand the paper rightly it is a complaint, but as far as I can see Mr. Hawkins is the man chiefly at fault. He has known more about the printing press than any other man in this Society. He has not yet presented a paper on the subject. He has not written a book on the printing press so far as I know. There is not a college in this country, so far as I have seen, which has a text book on the subject, because there is none in existence. I know of no paper presented to engineering societies relating to the printing press. I do not remember to have seen anything in *Engineering* of London on that subject for several years. People who have known about the printing press have kept the information very strictly to themselves. If Mr. Hawkins will write a text-book in which the general principles of the printing press are laid down, I am sure the professors will take hold of it as a book of reference and give their students some instructions in the matter.

But the same complaint made about the printing press should be extended on other things, for instance, watches, nail-making machinery, tack machinery, pin machinery and things of that kind. I think there is an opportunity for colleges to take up the study of practical mechanism, in explaining machines such as the printing press, watches, nail, tack and pin machinery and all such things.

I want to impress it strongly on Mr. Hawkins that he is the man to blame, for he has not written a book from which we can learn something about the printing press.

Mr. E. S. Cobb.—I think Mr. Kent has struck upon the point exactly. I am interested in instructing in machine designing, and the trouble is not to find subjects for instruction, but rather to select which to discard from among a large number of subjects which present themselves. Of course, as has been intimated by the last speaker, you might go into nail-making machinery, and tack machinery, and all that class, but I think no class of work presents itself of which so little is generally known as the printing press. Such manufacturers as R. Hoe & Co., and others I might mention, are very exclusive. I know the case of a number of students who have gone to work for such companies, and who have had to learn the entire business after going there. Their general education in practical machine designing was of little use to them. But the trouble, I think, is partially with such men as Mr. Hawkins, who, knowing well how to, do not publish the information which we desire on that score.

Mr. F. R. Hutton.—There is another side to the question which Mr. Kent has raised, and that is that in the technical schools we have only four years in which to do a great deal, and in selecting subjects available for instruction we have to select those which will yield, for a given amount of attention, the greatest amount of training. It is possible to undertake the calculations of the steam engine and certain other calculations of that class and carry them through to the end; but there is no man who can start with the printing press and carry it through to the end, because, as Mr. Kent has well said, the professors do not know enough, and the second reason is that the student has not enough previous information to go on. He has a certain amount of previous information in the matter of the design of steam engines, but he has probably never had the opportunity for the same degree of preliminary study of the printing press which he has had of the

steam engine, and I think that is one reason why the printing press has been neglected.

Mr. Cobb.—One more point in connection with that I think might be worth mentioning, and that is the field offered to young men after graduating from these institutions. They see the steam engine and kindred motors in use everywhere; but printing press manufacture is confined to several cities, and while the product in machines may be large, the product of each machine itself is very large afterwards, and it does not seem to be such a universal product as the others, and does not, except in a purely kinematic sense, present such a good subject for theoretical study.

Mr. W. T. Magruder.—I think another reason why the printing press has not been introduced into our mechanical engineering and other similar technical schools is this: The printing press establishment has been a sealed book and closed door to all visits of engineering students. I was connected for a number of years, with the printing press manufacturing establishment of which Mr. Hawkins is the honored president, and about three years ago I suggested to him and to some of the officers of the company that it would be a good thing if some of the technical schools could be invited—I was thinking at that time of the Stevens Institute—to come there and visit our establishment; but they were not allowed to come. I have been told also that it is almost impossible to get into R. Hoe & Co.'s establishment; and the only way for a teacher of an engineering class to bring his class into intimate contact with printing presses is in the printing offices themselves. I would say to those who are interested in teaching in engineering and manual training schools that there is no place where they can teach mechanical movements and the geometry of mechanism and things of that kind to better advantage than in the printing office. All sorts of illustrations are there—cams and gears, links and levers, belts and springs, and the best facilities will be found there for teaching students from practical examples.

Mr. J. L. Gobeille.—The Ohio Wesleyan University is about to start a department of manual training, and at the meeting of the committee, of which I happened to be a member, it was decided that the first thing to be put in the department would be a printing press, to teach young men and women two things—first, the mechanism and usefulness of the printing press as applied to what they would, perhaps, be apt to do in after life; and another

thing was to let them do something which would have a commercial aspect to it. They have to pay for the printing which they have done at the University, and if they could do their own printing, the chief end of labor would be attained; they would get money for it and get it honestly. They talk a great deal about the dignity of labor in the training school. The fact is, there is not any, and the sooner they learn that the dignity of labor is to make money by it, the better it will be for the young men sent to those schools.

Mr. Jacob Reese.—I would just add a word—that I think it is very important that the members of this Society should take this question to heart. As we represent technical experts in the different departments of industry, the world at large looks to us for direction and explanation of these different machines and processes. It seems to me it would be a desirable thing for the schools to be able to have a printing press to take apart, and to have all its parts numbered and their connection explained. I know little about the printing press—very little about anything outside of my own line of experience,—but I had a letter the other day, which I will mention, to show the importance of our taking this matter to heart, and preparing something for the schools. This is a good way—bring it in here and have it criticised. I had a letter the other day from an assistant of Prof. R. H. Thurston, our honored member, who requested that I should give him a detailed explanation of the basic process, so that he could present it to his class. I presume there is not a school in the country which has anything reliable on this important question in such a shape that they could present it to their classes. We have had papers on the Bessemer, basic and open-hearth process of such a technical nature as not to be presentable to a class. I will take occasion at my earliest opportunity to make out for him such a description as he wants. I say this to show you that the schools are anxious for something which will give them a concise, definite description of the machines and the processes which they are teaching.

Prof. Sweet.—At Cornell University, in the early days, they were favored with the presentation of a large press by R. Hoe & Co. to start their printing office. They had a number of presses, and we had to keep those presses in repair. But after I left, the department, from some cause or other, ran down, as well as the printing office. The printing office has been closed entirely and

the presses thrown aside. The large press given by Hoe & Co. is, I believe, in the scrap heap, and the others sold. The printing department was at one time in a flourishing condition. A stereotype foundry was carried on, so that the students had an opportunity of getting a good knowledge of printing presses and stereotyping. But that has all been allowed to die out—in my opinion a great mistake.

The President.—The School of Industrial Art in Philadelphia teaches the students not only how to weave textile fabrics on looms of different styles, but also the designing of the pattern, making the cards, in fact everything necessary for a correct understanding of the art. I think it would be a good idea for Mr. Hawkins, or some of the press companies, to present a technical school with a printing press, so that they could obtain a correct understanding of the machine, which they could take apart, and then write a thesis on the subject.

Mr. Hawkins.—I think that the objectors to this paper have lost sight of the principal feature in it; and that is, that there are certain industries in this country which have the most importance, and others have a certain grade of importance, all the way down the scale; that many special branches of machinery manufacture have been included among the subjects studied and taught in the technical schools, and that the printing press should occupy a more prominent position than has been given to many of the less important special studies. It is the main object, in writing this paper, to bring this state of things to notice, in order that it may be improved upon.

I feel very much complimented by the proposition of Mr Kent and others that I write a text-book on the printing press. I hope I would be able to do it in a satisfactory manner, if I undertook it; but I am afraid not. I would say, in that connection, however, that there is not by any means the poverty of literature on the printing machine which Mr. Kent would indicate, although there is probably none in quite proper form to serve as a text-book for school use. There is, however, a great deal of matter in existence on the printing press, all accessible, which would be valuable in colleges and mechanical engineering schools. There are none of the things mentioned by Mr. Kent, except possibly pin machinery, but have been visited by students of the different universities; but in no case can I find where they have visited printing press manufactories.

I am not a little astonished by what Prof. Sweet stated; that they had already had printing machinery in Cornell University, and threw it out. I find, so far as I have looked, that they have not, in any of the institutions, anything connected with printing machinery, of any consequence. They ought to have something of this nature, even if they should have to buy it. I am even more astonished that the suggestion of the President was not about the first thing that was offered in criticism of this paper; that is, that printing press manufacturers be invited to send printing presses, without charge. Of course it would not be well for us to be invidious; we can hardly be expected to supply every school with a printing press; but I guarantee that we will supply a printing press to a technical school, free of charge, if others in the same line will do as much—that is, if proper arrangements can be made by them so as to get over the difficulty of being invidious between one and the others of the schools, for it could hardly be expected that all the schools could be supplied in this way, even though every printing press manufacturer donated one machine; and the experience of the Cornell University does not hold out any very great incentive to printing press manufacturers to attempt such a thing. Moreover, for one printing press manufacturer to donate one single machine out of perhaps fifty different varieties made by them, would, it seems to me, go a very small way toward bringing about some special study in this direction among the schools generally.

Mr. Magruder has referred to the fact that there was a suggestion made at one time that engineering students visit our establishment; but he did not explain that we are not manufacturers ourselves. We have our printing presses built by the Mason Machine Works, on contract. We have nothing to say about whether any one shall come into the shops or not. I have no doubt it could be accomplished, if any direct effort were made in that direction. I will say that if any of the engineering schools would like to bring their students to visit our establishment, if they will communicate with me, I will endeavor to arrange with the Mason Machine Works for their coming. So far as we are concerned, we would be very glad to have them. Besides, printing offices themselves, where such machines are to be seen in operation, are always open to the visits of students from schools, or generally so, and they could obtain much valuable information in that way. But we never hear of their making such visits.

I only wish to impress on the convention that my principal object in writing this paper was to show that the printing press is much more deserving of prominence than many of the machines to which the schools give attention. If they will take the matter in hand, so far as I am concerned I will do all I can to assist them, and I believe that other printing press manufacturers would gladly do the same.

Prof. F. R. Hutton.—It is only fair to state in rebuttal of one or two remarks in the debate, that visits to printing press establishments are not so difficult or impossible, as above suggested, provided that proper arrangements are made in advance for such visits, and the party is properly officered and guided. The firm of R. Hoe & Co. has been mentioned as one in whose case the difficulty of admittance has been experienced. I have been most cordially met when I have applied there for permission to accompany a squad of Columbia's students, and that firm in particular is well known as taking a most enthusiastic interest in technical education particularly for its own apprentices. They have classes of their own in mechanical drawing and mathematics as well as in the English branches.

*Mr. J. T. Hawkins.**—The proper course to pursue, I think, on this subject, on the part of the schools, would be for each of them to procure one or more full machines of the standard types, such as would be fitted for instruction purposes, from the various makers, and perhaps some separate parts representing the fundamental kinematics of the printing press; and I have no doubt that every press builder in the country would join the company I represent in placing such in the schools at reduced prices, which is certainly all that could be asked. Some varieties of printing machines are entirely too costly and occupy too great a space, to permit of a thought of having a sample of them in a school. This is equally true, however, in the steam engine and of pretty much every other machine in which special instruction is now given in the schools. As an exercise for the mind in machine designing, I do not hesitate to say that the printing machine is not excelled by any other piece of mechanism produced to-day. It involves kinematics of a very high order, and of almost endless variety, including, in some form, almost every kind of motion known to the mechanical world; and such a machine, or system of machines, if studied and taught as a specialty in the schools, would be of

* Author's closure under the rules.

incalculably greater value to the student than many of which they now have to give their attention. Mr. Cobb says he has known of a case of a number of students who have gone to work for printing press companies and who have had to learn the entire business after going there, and that general education in practical machine designing was of little use to them. This is precisely the state of things which I desire to see corrected. Give the boys some special instruction in this branch, just as now done on the steam engine, and thence down to wind-mills; and they will be able to present themselves better equipped for the fight.

To Prof. Hutton's remarks, I would reply, that if for the lack of previous information no man can start with a printing press and carry it through to the end as with the steam engine, the proper thing to do is to give the student a little of this "previous information," just as is done with the steam engine and many other things; and if the four years are not long enough within which to accomplish this, leave out some of the less important to which attention is now given.

To Mr. Cobb's second remarks, I would say that perhaps he would be surprised to know the comparative number of steam engines and printing machines in use in the world, and be equally surprised, perhaps, at the number of printing press manufacturers in the country. I hardly think he justly expresses it by saying that their manufacture is confined to several cities. Without using anything more than my general knowledge of the extent of this industry, I should say that I was well within the truth if I counted not less than 4,000 to 4,500 printing machines made in this country per annum, of all kinds, excluding the mere toy machines for amateur use; and more than half of these will be machines that range from two to five tons each in weight. It is, moreover, precisely in a kinematic sense that this class of machines does present the best possible subject for theoretical study, while it is in this sense, as indicated in a previous paper ("Education of Intuition"), that our technical graduates are most lacking to-day.

I would suggest, in view of Prof. Hutton's objections, that if four years do not give time enough to properly consider such subjects, that at the commencement of the junior year, students be given the privilege, or be required to choose among a considerable number of studies, which two or three they would prefer to receive instruction in, to the exclusion of all others, with the

view of the young man's adopting either one of the chosen ones upon graduation; giving the printing machine its proper place among them. So much is known as a whole nowadays on any subject, that no man can expect to acquire it all; and no better time can appear at which a young man may determine for himself some limit within which his future sphere of action should be confined than at about the middle of his college course. If he tries to prepare himself fundamentally to enter any one whatever among the whole, he is pretty sure to be well fitted for none. He will be much like the traditional jack at all trades—master of none.

CCXCI.

SURFACE CONDENSERS.

BY JAY M. WHITHAM, FAYETTEVILLE, ARK.,
(Member of the Society).

1. ORIGIN AND NATURE OF THE SURFACE CONDENSER.

JAMES WATT was granted a patent for inventing the surface condenser, Jan. 5, 1769. He said: "In these engines, that are to be worked wholly or partly by condensation of steam, the steam is to be condensed in vessels distinct from the cylinders, though occasionally communicating with them. These vessels I call *Condensers*, and whilst the engines are working they ought to be kept as cool as the air in the neighborhood by the application of water or other solid bodies." * * * "Whatever air or other elastic vapor is not condensed by the cold of the condenser is to be drawn out of the steam vessels, or condensers, by means of pumps wrought by the engines themselves or otherwise."

Watt's first experimental condenser consisted of two small pipes, made of tin plates, about a foot long, placed vertically, and connected at the top and bottom respectively to the steam cylinder and a pump. The pump and tubes were submerged in a cold-water bath.

The surface condenser was replaced by a jet condenser by Watt, "in order to secure a surface sufficiently extensive to condense the steam of a large engine, * * * as the pipe condenser would require to be very voluminous, and because the water with which engines are frequently supplied would crust over the thin plates and prevent their conveying heat sufficiently quickly."

The surface condenser appears to have been used only as a model until S. Hall applied it, in 1838, to the steamship *Wilberforce*. This condenser had 2,374 copper tubes, 8 feet long and one-half inch in diameter, placed vertically in a box. The surface was about 2,486 square feet, and the horse-power of the engines 285. There were 8.72 square feet of condensing surface per horse-power. This condenser was not considered a success, as the tubes became

coated with mud, so that they were removed and the surface was changed to a jet condenser.

In 1859 the P. & O. Steamship Co. applied surface condensation to the steamship *Moulton*. The condenser had 1,178 tubes, 5 ft. 10 in. long, $\frac{5}{8}$ inch in diameter, 0.05 inch thick, or a surface of 4,200 square feet. The indicated horse-power was 1,731. There were 2.42 square feet of condensing surface per I. H. P. The tubes were packed with linen tape and screwed glands. The circulating water was controlled by a 36-inch disk centrifugal pump making 200 revolutions per minute. This is probably the first independent circulating pump ever used. The tubes were vertical, and the refrigerating water ascended them on the outside.

The year 1859 marks the date at which surface condensers began to come into general use.

2. REASONS FOR USING OR NOT USING A SURFACE CONDENSER.

J. F. Spencer, in a paper read before the Institute of Engineers (Scotland), Feb. 5, 1862, gave the following

Advantages of a Surface Condenser.

1. Freedom from injurious deposits in boiler. Small amount of scale.
2. Since the boilers will be stronger, steam of a higher pressure may be used than is possible with the jet condenser.
3. The foulest water may be used as the refrigerating agent.
4. The supply of feed water to the boilers is more regular than is possible when using the jet condenser.
5. The load on the air pump is uniform.
6. Gain in the use of the fuel, as the loss from blowing off is from 15 to 20 per cent. less when surface condensers are employed.
7. Boilers need not be cleaned so frequently: less wear, tear and expense.
8. Can use increased expansion of steam.
9. Heating surface of the boiler is more efficient, as there is less incrustation.

Disadvantages of a Surface Condenser.

1. Additional pumps and machinery.
2. Additional space occupied by the machinery.
3. The use of the same water over and over again is held by some to corrode the boiler.

4. Complication of tubes, etc.
5. Liability to leakage.
6. Increased first cost of from 10 to 20 per cent., and increased cost of repairs.
7. More refrigerating water is needed than for a jet condenser.

3. JOULE'S EXPERIMENTS WITH THE SURFACE CONDENSER.*

Joule's apparatus consisted of two concentric copper tubes, through the inner one of which passed the steam to be condensed, while the circulating water traversed the annular volume between them. When a spiral wire was used to give a twist to the current of circulating water, it was placed in this annular volume.

He deduced: 1. The temperature of the steam side of the tube is uniform throughout its length.

2. The resistance to conductivity is due to the film of water on each surface, and is independent of the kind or thickness of the metal used.
3. The conductivity of the tube used increases with an increase in the velocity of the circulating water: and that for the same head, the conductivity is greatest when a spiral wire gives a twist to the current of circulating water.
4. Air is a very poor refrigerating agent.

The conductivity of copper tubes is thus stated by Rankine: †

Cooling fluid.	Initial Temperature, Fahr.	Material.	Steam Condensed per sq. ft. per hour. Lbs.	Authority.
Water.	68 to 77°	Copper.	21.5	Peclet.
"	?	"	100.0	Joule.

Each pound of steam condensed gave up about 1,000 British thermal units.

4. ISHERWOOD'S EXPERIMENTS ON CONDUCTIVITY OF METALS. ‡

The apparatus consisted of several metal pots, each 10 ins. internal diameter, 21.25 ins. inside height, which were immersed in a

* From an article "On Surface Condensation of Steam," by J. P. Joule, *Jour. Frank. Inst.*, 1862, p. 136.

† Rankine's *Steam Engine*, § 222.

‡ See p. 58, Shock's *Steam Boilers*.

common vessel supplied with steam of a certain pressure and temperature, from the boiler. The pots were kept constantly filled with water at 212° F., and the quantity evaporated measured the conductivity of the metal. The heat for vaporization was given by the steam-bath. Pots of copper, brass, cast and wrought iron were used simultaneously. The conditions were identical in each series of experiments. The experiments covered many days. The temperature of the steam-bath, and the thickness of the pots varied between wide limits.

The following laws were deduced:

1. The number of heat units transmitted per hour through a square foot of surface is in direct ratio of the difference in temperature of the sides of the intervening metal.
2. Within limits, the rate of transmission of heat through a metal wall is independent of its thickness. (Isherwood used thicknesses of $\frac{1}{8}$, $\frac{1}{4}$ and $\frac{3}{8}$ inch.)
3. The thermal conductivity is as given in the following table:

Metal.	Thermal conductivity in terms of heat units transmitted per hour through one square foot of material for a difference of temperature of 1° Fahr.	Relative thermal conductivity.
Copper, (refined).........	642.543	1.000 000
Brass, (60 cu., 40 zn.).....	556.832	0.866 607
Wrought Iron, (best rolled).	373.625	0.581 478
Cast Iron, (several times remelted).............	315.741	0.491 393

5. NICHOL'S EXPERIMENTS WITH CONDENSER TUBES.*

The apparatus consisted of an ordinary brass condenser tube, inside a wrought-iron pipe. The radiation of heat from the outer pipe was determined from separate experiments, and its effects were eliminated. Steam filled the annular space between the tubes. Its temperature and pressure were noted, and also the amount condensed. Circulating water traversed the inner tube. Its velocity varied. The quantity flowing through in a given time, and the temperature on entering and leaving the tube, were noted. Experiments were made with the tube in a horizontal, vertical, and in an inclined position.

* London *Engineering*, 20, 449.

The results were:

1. The temperature of the water side of the tube is the arithmetical mean of the initial and final temperatures of the refrigerating agent, provided the rise in temperature is not greater than is found in ordinary surface condensers.
2. The efficiency of the condensing surface is increased as the quantity of circulating water is increased.
3. The surface is most efficient when the tube is horizontal.
4. The number of heat units transmitted through a unit surface in a unit time is greatest when the difference in temperature between the sides is greatest.

6. METHODS USED IN DESIGNING THE CONDENSING SURFACE.

RIGG gives the following formula:—

$$\text{Condensing Surface in Sq. Ft.} = \frac{\text{Pounds of Steam Condensed per Hour}}{8.93 \text{ to } 7.81}.$$

This formula* permits about 8,000 British heat units to be transmitted through a square foot of condensing surface per hour, and assumes that about 1,000 units are given up by each pound of steam condensed.

SEATON† gives the following proportions, when the circulating water enters the condenser at a temperature of 60° F.

Absolute Terminal Pressure of Steam in the Condensing Cylinder.	Square feet of condensing surface per I. H. P.
6	1.50
8	1.60
10	1.80
12.5	2.00
15	2.25
20	2.50
30	3.00

When the vessel is to cruise in the tropics, the values given in the table must be increased 20 per cent.; when she occasionally visits the tropics, 10 per cent.; while a decrease of 10 per cent. will suffice in Arctic waters.‡

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* *The Steam Engine*, by Arthur Rigg.

† *Manual of Marine Engineering*, by A. E. Seaton, p. 198. See, also, Burgh's *Condensation of Steam*, p. 241.

‡ P. A. Engineer John A. Tobin, U. S. Navy, in his report on the *Improvements in Naval Engineering in Great Britain* (*Ex. Doc.* 48, 47*th Congress*), gives

Prof. Marks* gives the formula:—

$$\text{Condensing surface in square feet} = \frac{W(H - T)}{0.1 \text{ to } 0.2\, C\, (T' - T)}$$

Where W = pounds of steam sent to the condenser per hour.
H = total heat units in one pound of steam at the boiler.
T = mean temperature of the circulating water.
T' = temperature of the vacuum.
C = 556.832 for brass, and 642.543 for copper tubes, as found by Isherwood, and shown in the table of § 4.

7. PROPOSED FORMULA FOR THE CONDENSING SURFACE.

The area of the condensing surface depends upon the quantity, quality and temperature of the exhaust steam, the initial and final temperatures of the circulating water, the character of the exposed surfaces, and the metal used. It is evident that the methods given by Rigg and Seaton do not cover all the requirements. Prof. Marks' rule is not exact (1) because the heat given up by the steam to the circulating water is not nearly so great as $W(H - T)$, and (2) because the wide range in the value given to the thermal conductivity of the metal, *i. e.*, from 0.1 to $0.2C$, is misleading.

In 1883 the writer deduced † a formula for the condensing surface. It involved all the data necessary for a complete solution of the problem, but the value of the coefficient of efficiency of the condensing surface was not well determined. This formula is here reproduced and modified so that it may be used in designing.

In studying the action of the surface condenser we will make the following assumptions, as warranted by the experiments given in §§ 3, 4 and 5, viz:—

1. The temperature of the steam side of the tube is uniform throughout its length (Joule), and the steam is saturated at a tem-

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"The proportion of condensing surface to the horse-power in fifteen of the most recent (1883) types of high-speed merchant steamers by the best Scotch builders, averages 1.95 to 1. This proportion compared with the data of seven steamers, taken from a paper read before the *Institution of Mechanical Engineers*, by Sir F. J. Bramwell in 1872, having a ratio of 3.18 to 1, shows the saving effected in this direction during the past ten years."

* § 85, Marks' *The Steam Engine*, 1887.

† *Proc. U. S. Naval Inst.*, No. 24.

perature corresponding to the reading of the vacuum gauge. This latter assumption,* though arbitrary, is probably sufficiently exact, since (*a*) the fluctuations of the reading of the gauge are inappreciable; (*b*) the exhaust port is opened and closed gradually, steam is exhausted throughout all, or nearly all of the stroke of the piston, and the steam is condensed as soon as it arrives in the condenser; (*c*) the steam in the cylinder, at the end of its expansion, is almost certain to be wet, even with steam-jackets, and this wet steam, on account of free expansion during the exhaustion, is saturated when it reaches the condensing surface. This is still further probable because the condenser pressure is always several pounds below the terminal pressure in the condensing cylinder.

2. The temperature of the water side of the tube has a value equal to the arithmetic mean between the initial and final temperatures of the circulating water (Nichol).

3. The conductivity of the surface is increased as the quantity of circulating water used is increased (Nichol, Joule). This quantity of water will vary inversely as its rise in temperature.

4. The number of heat units transmitted per hour through a unit surface depends directly upon the difference between the temperature of the sides (Isherwood, Nichol); varies with the material used, as shown by the table in § 4; and is independent of the thickness of metal used for the tubes, as found in ordinary practice (Isherwood, Joule).

Let S = the condensing surface in square feet.

T_1 = the temperature of the steam in the condenser, or that of saturated steam corresponding to a pressure indicated by the vacuum gauge, in degrees Fahr.

T_2 = the temperature of the condensed steam as it leaves the condenser, *i.e.*, the temperature of the hot-well.

t = mean temperature of the circulating water, or the arithmetical mean of the initial and final temperatures.

L = the latent heat of saturated steam at a temperature T_1.

k = perfect conductivity of one square foot of the metal used for the condensing surface for a range of 1° F., or 556.832 British thermal units for brass (§ 4, table).

c = fraction denoting the efficiency of the condensing surface.

q = rate of conductivity corresponding to a variable range of

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* This assumption is indorsed by C. Andenet in *Étude sur les Condenseurs à Surface*, and by E. Cousté in *Annales du Genie Civil*. See *Van Nostrand's Engr. Mag.*, Vol. I., Nos. 7, 9 and 10.

temperature $T - t$, and an elementary surface of rate ds. T has a value between T_2 and T_1.

W = total number of pounds of steam sent to the condenser per hour.

The heat given up by the steam to the circulating water is

$$\int q\,ds = W(L + T_1 - T_2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

As a unit mass of the steam at a temperature T_1 impinges upon the refrigerating surface, which is at a constant temperature t, the units L are given up, and the steam becomes water at T_1. The range of temperature during this performance is

$$T_1 - t,$$

and for the units L, we have the constant

$$q = c\,k\,(T_1 - t).$$

The condensed steam at T_1 now gives up heat to the circulating water, so that the range is at first

$$T_1 - t,$$

and finally

$$T_2 - t;$$

and at any instant while it is on an elementary area of rate ds, the range is

$$T - t.$$

Hence for the water

$$q = c\,k\,(T - t), \text{ a variable quantity.}$$

Transforming equation (1) and integrating:

$$S = \frac{W}{ck}\left\{\frac{L}{T_1 - t} + \int_{T_2}^{T_1}\frac{dT}{T - t}\right\}, \quad . \quad . \quad . \quad . \quad (2.)$$

or

$$S = \frac{W}{ck}\left\{\frac{L}{T_1 - t} + \log_e\left(\frac{T_1 - t}{T_2 - t}\right)\right\}, \quad . \quad . \quad . \quad . \quad (3.)$$

The quantity $\frac{L}{T_1 - t}$ is always large, while $\log_e\left(\frac{T_1 - t}{T_2 - t}\right)$ is never greater than about 0.1. Hence equation (3.) will be practically correct, and much simplified, by dropping the last term, so that

$$S = \frac{WL}{ck(T_1 - t)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

The fractional coefficient, c, denoting the efficiency of the condensing surface, remains to be determined. It must be applicable to a condensing surface in ordinary use, *i.e.*, coated with saline and greasy deposits. The best data, available to the writer, for determining the value of c are furnished by the U. S. R. M. S. *Dallas*, from the experiments of Messrs. Loring and Emery,* as shown in the following table:

W	7261.54
S†	857.7
Barometer	30.195
Vacuum gauge	24.79
Vacuum pressure in pounds per square inch	2.597
Corresponding temperature for saturated steam, T_1	136.17
L	1019.22
Initial temperature of the circulating water	67.67
Final temperature of the circulating water	108.67
Mean temperature of the circulating water, t	88.17
Temperature of the hot-well, T_2	134
$T_1 - t$	48
$T_2 - t$	42.83
k from Table of §4	556.832
ck	180.14
c	0.323

The value of ck being 180, = Equation (4) becomes

$$S = \frac{WL}{180(T_1 - t)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

This applies to an engine having an *independent* circulating pump. When the pump is worked by the main engine, the value of S should be increased about ten per cent. *Formula* (5.) *is recommended in designing a surface condenser.*

The writer regrets that the official data relative to the exact quantity of steam used in the cylinders are so scant that he can use no other performances in deducing the value of ck. The formula will be found, however, to give safe results.

The value of W, the pounds of steam sent to the condenser per

* *Report of Secretary of the Navy*, 1874; *Engineering*, XXI., 121.

† The condensing surface, though published as 900 sq. feet, has been found to be but 857.7.

hour, will vary with the type of engine used, initial pressure of steam, ratio of expansion, and whether the cylinders are steam-jacketed or not. No more reliable data are accessible on this point than the results of Messrs. Loring and Emery, already referred to, and here summarized, viz.:

Type of Condensing Engine.	With or Without a Steam-Jacket.	Absolute Steam Pressure in Boilers. Pounds per sq. in.	Pounds of Steam used per I. H. P. per Hour.	$\frac{\text{Condensing Surface}}{\text{I. H. P.}}$
2 cyl. Compound, 90°.....	With.	55	22	2.08
2 cyl. Compound, 90°.....	With.	85	18.4	1.74
Non-Compound..........	With.	27.5	33 to 37	3.12 to 3.5
Non-Compound..........	With.	55	22 to 26.5	2.08 to 2.53
Non-Compound..........	With.	85	20.5 to 25	1.94 to 2.36
Non-Compound....	Without.	27.5	40 to 44	3.78 to 4.15
Non-Compound..........	Without.	50	26.7 to 31	2.54 to 2.93
Non-Compound..........	Without.	85	21.7 to 25	2.05 to 2.36

In designing, it is well never to anticipate a vacuum exceeding 25 inches of mercury when the engines are developing full power. This corresponds to about 2.5 lbs. pressure. So that $T = 135$ and $L = 1020$; and equation (5.) may be reduced to

$$S = \frac{1020\,W}{180(135 - t)} = \frac{17\,W}{3(135 - t)} \quad . \quad . \quad . \quad . \quad (6.)$$

The value of t will vary with the quantity of circulating water used and the season of the year. It, being the arithmetical mean of the initial and final temperatures of the circulating water, is about 60 in the winter and 75 in the summer. Since the larger value of t gives the greater value of S, we will substitute $t = 75$, and equation (6.) becomes

$$S = \frac{17\,W}{3(135 - 75)} = \frac{17\,W}{180} \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

W is the total number of pounds of steam condensed per hour, or

$W =$ I. H. P. × pounds of steam used per I. H. P. per hour.

By combining this with the data given in the preceding table, the last column is deduced.

The pounds of circulating water required per hour is

$$= \frac{W(L + T_1 - T_2)}{R}, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8.)$$

where R = the rise in temperature of the circulating water.

DISCUSSION.

Mr. Frederick M. Wheeler.—I have been very much interested in the paper on Surface Condensers by Prof. Whitham, as I believe it is the first paper on this subject that has been presented to this Society. He refers to the experiments of Hall, who arranged his tubes in a vertical position. Now Hall would certainly have been more successful had the tubes been placed horizontally, as I have found by experience (as also have other builders of surface condensers) that the horizontal system is the better one. Nichols' experiments certainly prove this, for he condensed from 10% to 20% more steam with the tubes placed horizontally than when placed vertically. The reason for this is very apparent; the condensed steam continually drops away from the entire length of the tube, while in the vertical form the water of condensation flows down the outside of the tube, thus covering the tube with a film of water for its entire length. Joule's experiments showed that about 100,000 heat units could be transmitted per square foot of surface per hour; now this result has been exceeded by Kirkaldy, of England (according to a report published in the *London Engineer*, in 1885) with a condenser of his make where 130,000 heat units were transmitted per hour per square foot of surface; Kirkaldy used fluted serpentine coils, which, in my opinion, are objectionable, owing to its being almost impossible to clean properly the inner and outer surfaces of the condensing surface—a necessity if you wish to keep up the efficiency of a condenser. The best surface condenser is one so arranged that the tubes can be readily removed for cleaning or repairs; this means the use of a straight tube and with fastenings which will not be destroyed by the removal of the tube. My experience, dating back nearly twenty years with many types of surface condensers, has shown that most every condenser in the market is open to the objection of having the tube packings arranged with wood, paper, or other fibrous packed ferrules or glands; these are all likely

sooner or later to leak and give trouble, and where a vessel is laid up for any length of time, are liable to dry out and cause leakage; then again, whether the tubes are packed with ferrules, stuffing-boxes or glands, there is always a liability of the tubes being held rigidly, thereby preventing proper expansion and contraction.

The superintendent of the Ward Line of steamers had an experience of this kind with condensers on his steamers; in cleaning the condensers after a few years service, the tubes were indented and otherwise roughened at the ends, in taking them out; when replaced, the tube ferrules gripped and held firmly a sufficient number of these tubes so as to cause the rupture of the tube heads; the result was that all the tube heads had to be replaced at considerable expense. I have prevented trouble of this kind by arranging the tubes so that but one end was held fast—the other end being free to expand and contract without limit. To accomplish this I used one tube inside a large one, the water first passing through the smaller tube and returning through the annular space between the two tubes; this space being over 50% greater area than the area of the smaller tube. With an active circulation of water through tubes of this arrangement and a water chamber of special design I secured remarkable results, particulars of which I shall publish when verified by an Official Board from the Bureau of Steam Engineering.

Now the formula proposed by Prof. Whitham seems to me to be very consistent and will no doubt prove of practical value in the designing and proportioning of the surface condensers. The only thing I might criticise is the second line of the formula where the term T_2 might trouble in the result of his calculations; for the reason that the temperature of the hot well is generally less than the temperature of the condensed steam as it passes out of the discharge nozzle of the condenser; in other words, as the condensed steam leaves the condenser and passes through the air pump and pipes, it is continually losing heat until it reaches the hot well. I have known the difference of temperature of condensed steam to be as high as 10 to 20 degrees.

Prof. Seaton's tables are generally followed in English practice, and are now being used in this country more than formerly. It is very amusing, however, to find how little is known by the builders of surface condensers in this country as regards the proper adaptation of the condenser to the engine; and I have no

doubt that the paper of Prof. Whitham (with the wide circulation which the Society will give it) will do much to educate and assist manufacturers in more intelligent practice.

Mr. Louis G. Engel.—I believe the common practice is to use condenser tubes of brass, which brass is perhaps similar to that mentioned in the table given by Mr. Whitham, as being composed of 60% of copper and 40% of zinc. It is this zinc which has obliged us to throw out brass pipe for any purpose where it is exposed to sugar liquors, or even the vapors from the same, in a vacuum pan and to substitute copper therefor. The specimen submitted of what was once a brass tube is from the surface condenser situated between a vacuum pan and the jet condenser, exposed to the vapor from the pan at a high velocity, subject to a range of temperature from probably 50° to 212° F., the tubes being about 12 feet long in a vertical position and free to move at the lower end. Time of service about six years with Ridgewood (Brooklyn) water inside the tubes for cooling.

It will be observed that the form of the tube remains, that the zinc has largely disappeared, that the residue is probably an oxide of copper so weak as to resemble red brick in its facility for breaking short.

We have replaced those broken or split with copper tubes which expand about as well as brass, are as free from leaks where expanded, and last very much longer. Of course where the tubes are only exposed to the action of condensed steam from an engine, brass may last indefinitely, especially if good oil is used for lubrication; but the specimen submitted speaks for itself as to its endurance under the conditions cited. The principal advantage claimed for brass tubes seems to be their rigidity, especially when horizontal, yet we have two-inch copper tubes No. 14 Birmingham Gauge over nine feet long in use about four years, horizontally, without any perceptible sag. I am convinced that this is due to the fact that one end was free to move; for when this condition does not obtain, a sag is the inevitable result not only of the weight but of a gradual growth of the tube in length, due to innumerable expansions by heat, greater individually by a minute quantity than the subsequent contractions, but in the aggregate actually greater by a very appreciable amount.

Mr. Nagle.—I know as a matter of fact that these vapors permeate a sugar refinery throughout, and are destructive to all brasses within the premises, from the basement to the attic. Of

course they are more active on some floors than on others; but even in the basement these vapors affect all brasses and cover them with a slight oxide. One remedy is, to coat them with paraffine, where that is possible.

Mr. J. S. Coon.—I have known a case where the brass tubes of a feed water heater were acted on in the same way as those exhibited. The gases in the coal ate the zinc out, and left the copper in a similar brittle condition, so that the heater had to be taken out for this reason.

CCXCII.

AUTOMATIC REGULATOR FOR HEATING APPARATUS.

BY J. T. HAWKINS, TAUNTON, MASS.

(Member of the Society.)

For the heating of buildings, the hot-water system has many advantages over steam radiators, and it is now coming more largely into use than heretofore. In Europe and Canada the hot-water system has been much more largely used than in the United States. Lately, however, its superior advantages have become recognized in this country, and it is now rapidly supplanting the steam system, particularly for the heating of residences.

The principal advantage of the hot-water over the steam method is that the temperature of the radiators may be varied in the former to suit the exigencies of the season, while in the latter, if sufficient radiating surface be supplied to warm a given room in severe weather, it is altogether too liberal in its heat-dispensing power in mild weather; and too high a temperature must be submitted to in such cases in the apartments heated, or the radiator shut off and the other extreme endured; or, even if by constant watchfulness the steam radiators be from time to time turned off and on—which operation constitutes a very grave nuisance—the apartments sought to be warmed must at best fluctuate between considerable maxima and minima of temperature.

In the open hot-water circulating system, with the proper regulation of the combustion in the water heater, the radiators may be kept at any temperature desired below 212° Fahrenheit; while with steam coils or radiators, if they be heated at all, it must be at least at 212°, and, where a pressure of four or five pounds to the square inch, necessary for complete circulation of the steam, be carried in the boiler, it will be something more. The nuisance of being obliged to expel the air from the cooled steam radiator also adds to the discomfort attending it, and also renders it less efficient in maintaining an equable temperature in the apartment. Automatic air valves mollify this difficulty somewhat, but even with the best of these, when so nicely adjusted as to be entirely closed when

the radiator is heated, the time required to discharge the cold air from a radiator of any considerable size through the exceedingly small opening offered when the radiator is cold is so great as to add still further to the fluctuations of temperature of a room heated by them in moderate weather, if they be of sufficient capacity to warm the room properly in the coldest weather.

The hot-water system has not, however, heretofore, so far as noticed by the writer, been provided with any efficient means of automatically regulating the combustion, while the steam system, by means of varying pressure of steam in the boiler, offers a very facile and simple means of opening and closing the dampers automatically, and thus regulating very perfectly the combustion to conform to the demands made upon the radiators.

There are in use automatic electric regulators for hot-water and hot-air furnaces, governed by the temperature of the apartment through a delicate thermostat making and breaking electric contacts which trip into operation some extraneous power for opening and closing the dampers; but they are very delicate in construction, and under ordinary use quite liable to lose their nice adjustment and become inoperative. When kept in perfect adjustment, moreover, they are extreme in their operation, requiring a certain range of temperature to be submitted to in an apartment before a change will occur; and when such an appliance operates upon the dampers, by the apartments having reached either the maximum or minimum temperature at which it will act, the dampers are either wholly closed or wholly opened by the extraneous power referred to; and the result is a continual succession of maximum and minimum temperatures in the apartment, instead of an equably maintained one. Thus, with the dampers entirely open, the combustion goes on at a rapid rate until the temperature in the apartment reaches the maximum point at which the thermostat makes the necessary connection by which the trip is operated, and the extraneous power closes them tight, the reverse obtaining until the minimum temperature in the apartment is reached; and these operations are periodically and continually repeated, and, so far as the writer knows, these fluctuations are quite beyond the limits of comfort.

There are also in use damper regulators for hot-air and hot-water apparatus in which the dampers are operated through the expansion of a straight rod, the motion of whose free end is multiplied into the dampers by means of a succession of unequally armed

levers; but, in such, the multiplication of motion is required to be so great as to involve the use of a class of mechanism badly adapted to such situations as are usually occupied by furnaces; and, while suitable for instruments of precision, are objectionable for such purposes as above, as liable to fail because of the accuracy of operation required, which is not likely to be long maintained in such situations.

The above considerations induced the writer to devise the appa-

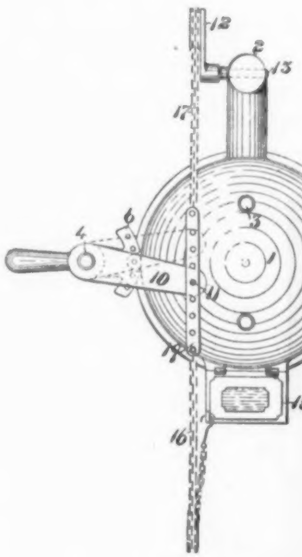


FIG. 107.

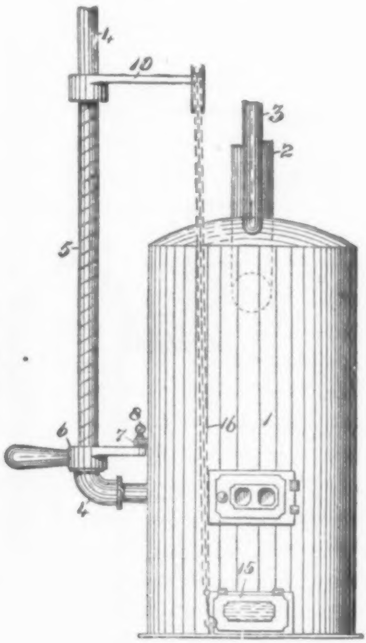


FIG. 108.

ratus outlined in the figures—which, of course, may be varied in construction in many ways. Figures 107, 108 and 109 show respectively a plan view and a front and side elevation of the apparatus as attached to a hot-water system. 1 is the heater; 2, the flue leading to chimney; 3, a pipe leading from the top of the heater to a radiator; 4, a return pipe from the radiator, delivering the cooled water into the lower part of the heater. Surrounding pipe 4 is a helical-coil thermostat, constituted of two strips of dissimilar metals, having widely different coefficients of expansion, as brass and iron, riveted, soldered, or brazed together. This helical ther-

mostat fits over the pipe 4 easily, with the more expansible metal on the inside, and the less expansible on the outside, exposed to the temperature of the surrounding air. The lower end of the helix is secured to a lever 6, having in one arm an arc of pin-holes, the outer arm constituting the handles. A lug 7 is attached to the heater, having the single hole meeting the arc of holes in the lever 5. A pin 8 may be placed in either of the holes in the arc, and thus the lever 6 be rigidly held in either position determined by the given hole used.

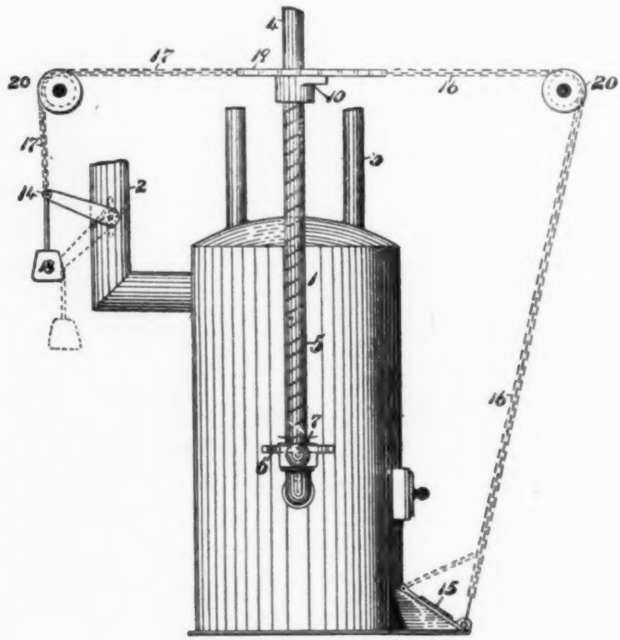


FIG. 1(9.

To the upper end of the helix is secured a lever 10, having at its free end a pin 11. 13 is a damper of the flue pipe; and 12 a lever secured to its axis. 15 is the ash-pit or inlet damper. 19 is a strip of metal containing a series of holes fitting the pin 11. 16 is a chain passing over one of the leading pulleys 20, with one end connected to the strip 19 and the other to the inlet damper 15. 17 is a similar chain passing over another leading pulley 20, and connecting the other end of the strip 19 with the free end of the damper lever 12. 18 is a weight suspended from the lever 12, to

counterbalance the weight of the damper 15 and the surplus vertical chain leading from it.

In this construction it will be obvious that the expansible helix, having the more expansible metal exposed to the fluctuations of the temperature of the pipe within it, while the less expansible one is in contact with the surrounding air, and therefore only partially heated by conduction from the inner one, will undergo a considerably greater straightening or unwinding for a given elevation of temperature of the pipe within it than if it were merely immersed wholly in the medium whose variation of temperature was to operate upon it, as with the ordinary thermostat; and that, the lower end of the helix being fixed, and the helix being of considerable length, such a straightening or unwinding by elevation, or coiling up by reduction of temperature of the pipe, will cause the upper lever 10 to move through a considerable arc for a small variation in temperature of the pipe within it, and that the force exerted to move the lever 10 will be a very positive one. The pin holes in the plate 19 serve merely to regulate the relative position of the dampers 13 and 15, and the lever arc 6. The same, however, may be accomplished by hooking the chains directly to the lever 10, and taking them up or letting them out, as may be required. The adjustment of the position of the lever 6 will determine the temperature of the pipe 4, at which the dampers 13 and 15 will become closed or remain in any desired position of partial opening; and, in any given weather, it will be only necessary to put the pin 8 in such hole of the arc of lever 6 as will keep the water returned down the pipe 4, and consequently that circulating through the radiator, at the required temperature. This adjustment decides what temperature the apartment will be maintained at, the apparatus thereafter automatically maintaining that temperature. Of course, the adjustment of the lever 6 may, by any well-known means, be made to be operated from the room where the radiator is situated, in order to avoid going into the cellar to make the adjustments; and in many cases this may be desirable, as it is often required, without any marked change in the weather, to maintain a lower temperature throughout the house—for instance, to have it at night cooler than during the day, in order to reduce the combustion during the night to a minimum.

The helical thermostat is shown as surrounding a return pipe, but it will be equally efficient, and is generally most desirable, if placed upon an ascending or delivery pipe; the only difference being that

in one case the regulation is made to conform to the fluctuations of the temperature of the water passing from the heater to the radiator, and in the other case to those of the water returned from the radiator to the heater.

As in the gravity, or open hot-water system, there is generally required to be a separate return and delivery pipe connecting each radiator with the heater, it would be desirable to attach this apparatus to either the delivery or return pipe of the principal radiator in the house, so that the temperature of the entire house will be governed by that of its principal apartment.

This apparatus is quite as applicable to the government of hot-air furnaces as to hot-water, by providing a special small branch hot-air flue, to be surrounded by the helical thermostat, and the necessary connections made for the dampers.

DISCUSSION.

Mr. Geo. H. Babcock.—The arrangement proposed by Mr. Hawkins for regulating the damper of a hot-water apparatus is a very ingenious adaptation of Dr. Arnot's "thermometer stove" of fifty years ago, and as a means of preventing the overheating of the water, so as to generate steam, will doubtless prove very efficient, when the thermostat is fixed upon the hot-water pipe. But as a means of regulating the temperature of a room or building, it seems to me to be of little value for several reasons. First, the thermostat working by the difference of temperature between the fire room and the pipe will cause the temperature of the water in the pipe to vary with that of the fire room, and presumably of the external air, so that the colder the weather, the less will be the temperature of the water circulated through the pipes,—directly the reverse of the requirements of the case.

Again, the demands for heat vary with the hours of the day. Frequently a cool morning will be succeeded by a very warm afternoon, and unless the thermostat be often adjusted, the heating will be quite too regular for comfort. But there is a greater objection to the arrangement. It is the nature of a hot-water apparatus not to permit sudden changes in supply, whatever the demand. The chief recommendation of this method of heating heretofore has been that if the fire be left to go out, the effect will not be felt for an hour or two; and, per contra, it takes an equally long time to get up a comfortable temperature in a cold room. Now the thermostat arranged as described may keep up an even

temperature in the pipes while the fire holds out to burn, but it cannot do more than keep up a practically constant heating power in the room, which is, frequently, just what is not wanted. Moreover, the adjustment of the thermostat will not make itself felt for some time after it is made.

Mr. Hawkins speaks slightingly of the electrical regulation of steam heat, and feelingly remarks upon the constant recurrence of maximum and minimum temperatures, which he says are "quite beyond the limits of comfort." Now it happens that I have had one of these—Johnson's—in my house for three seasons, and I have not found it a very delicate instrument to manage, or a severe trial to the nerves. It is true it will not keep the house cooler than the external air, nor when the radiation from the closed registers and heated floor above the boiler is sufficient to carry the temperature above the standard does it prevent the house from becoming too warm, but it would be perfectly easy to so arrange it that at such times it would admit a supply of cold air, and thus overcome this objection. Many of them are put up, one in each room, connected with the steam valves of the radiators, in which case the temperature in the room has been known not to vary *one-half a degree* all day long, which variation, I submit, is not "beyond the limits of comfort." If Mr. Hawkins can keep a room heated by hot water within the same limits, he will deserve the thanks of all those who use that method of heating.

Objection might also be taken to the statement that the hot-water system "is now rapidly supplanting the steam system, particularly for residences." Such is, I think, not the fact except possibly in certain localities in New England, where considerable effort has been recently made to introduce certain kinds of hot-water heaters. These things go very much by fashions, like styles of architecture. Some thirty-four years ago, after Hood had spent ten years of earnest endeavor to introduce hot-water heating into England, Mr. Charles Tomlinson in his *Cyclopedia of Useful Arts*, wrote: "The method of heating by steam pipes has long given way to hot-water apparatus." At that time it was fashionable in this country also. But hot water again gave way to steam, and was relegated mainly to the heating of green-houses; even these, however, are now being heated extensively by steam in preference. So far is hot water from displacing steam at the present time, that several times as many new buildings, and even dwellings, are being fitted with steam as with hot water. Neither steam nor hot-water

heating is very new. In a former discussion on this subject,* it was pointed out that steam heating was used in Pompeii; how much older it may be we do not know. So far as I am aware, the first use of a circulation of hot water in pipes was by M. Bonnemain, in France, who used it for hatching chickens. Some interesting discussions of the relative value of the systems for heating from central stations will be found among the papers of the Washington meeting, June, 1887.

The worst thing which can be said against steam in a dwelling is that which has been pointed out in the paper—the difficulty in tempering its heat to very moderate weather. But even this is perfectly overcome by an arrangement now quite generally used, of enclosing the boiler and a sufficient number of steam coils in a brick chamber, from which heated air is taken to the living rooms and main hall. In moderate weather the boiler itself, without making steam, gives sufficient warmth to the air for comfort—in that case becoming for the occasion a hot-water apparatus—while its full power is available in cold weather to supply steam for every room. With this arrangement for detached buildings, or the supply of steam from street mains where it can be obtained, and the electrical controlling apparatus, properly applied, it would seem that nothing further could be desired in the way of domestic heating.

Mr. E. L. Dent.—I would like to ask what change of temperature is required to make this apparatus work.

Mr. Hawkins.—I will say in that connection that I expected to be able to report on a series of experiments on an operating instrument by this time, but I have not succeeded in getting the work done in time; but I can say I have made some crude experiments in which I have found that with a smaller helix of about an inch in length, the material being about $\frac{1}{32}$ of an inch thick, half brass and half steel, and the width of the strip about $\frac{3}{16}$ of an inch, coiled in about a $\frac{3}{4}$-inch diameter coil—with that arrangement could be gotten motion through an angle of a little over 180 degrees with about 100 degrees variation in temperature. I saw a metallic thermometer made in about that proportion, and the index motion multiplied the rod motion eight times, so that the pointer passed through 180 degrees of arc with a 100 degrees variation in temperature, the helix being wholly immersed in water passing through the pipe.

* Transactions A. S. M. E., Vol. VI., p. 861.

Mr. Wm. Kent.—I would like to ask why the hot-water system has come into use so slightly in competition with steam heating.

Mr. Hawkins.—I can only say in reply to that, that I have been as entirely ignorant on that question as Mr. Kent, and that my having anything to do with this at all has simply arisen from my deciding to take a hot-air apparatus out of my own residence and look up what was best to replace it with; intending to use either steam or hot water, and in my inquiries I found that in the northern and colder climates, hot water was used very much more than it had been, and was going into use still more.

Mr. W. R. Warner.—In answer to Mr. Kent's question, I would say that this subject has been brought to my notice before. One of the objections to the use of hot water is that you must always have the radiators full of water from the top of the house to the bottom, and in case there is any leak, you are liable to be flooded. Another is, that in case you wish to shut off the water from any of the upper rooms, you are liable to freeze; then you will be flooded. I have learned of no method of obviating those two objections. They are not generally spoken of by the agents of the hot-water system. In the use of steam, no such accidents can happen.

Mr. Hawkins.—I would not like to have it understood that I have prepared this paper merely to advocate the hot-water system. I merely said what I did in reference to its coming into more universal use than heretofore because that was what I found in my investigation, particularly in Canada and the Northern States, where the weather is very cold in the winter season. I am not particularly an advocate of it, although I believe, everything considered, that it is the best method of warming residences to-day. While it may be true that an engineer, such as we know Mr. Babcock to be, could have his electrical apparatus in his house and receive the best kind of satisfaction from it, it is nevertheless true that in the large majority of houses where there are no such engineers, the reverse would be the case, and that the electrical apparatus would be very likely to get out of order when there would be nobody sufficiently skilled to attend to it. Very few could be as much interested in making a steam plant successful as Mr. Babcock.

Then I think he makes a mistake to some extent in stating that the length of time required for a change of temperature in the radiator is so great. If what the hot-water people represent be

true—I cannot state from personal observation—and the apparatus be properly piped, the circulation due to change of temperature is very rapid, and the temperature of a pipe 100 feet from the furnace will change in a very few minutes from a small change in the condition of the fire. That is what they represent, and, so far as I have gone in investigating it, I think it is true.

Mr. Jacob Reese.—I wish to say here that I differ from Mr. Hawkins in one statement that he has made. He states that he thinks that is the very best method of heating. In Pittsburgh we heat our houses with natural gas. I think that is a great deal in advance of the gentleman's system.

Mr. J. L. Gobeille.—As a matter of fact, in northern cities—Montreal and Quebec, for instance, heating by hot water is almost the rule, and that has not come into vogue only in the last three or four years, but has been in use for a number of years. Since the winter carnival has been held in Montreal, and people from southern climates have gone there and shivered around the hotels having a good time, the natives have paid more attention to heating, and it is found that nothing will do so much heating and give such an equable and comfortable heat, as hot water. I venture to say that in the city of Montreal nine-tenths of the heating, as regards the principal residences and hotels, is done with hot water.

Mr. Dent.—In confirmation, also, of what has been said, I would say that in my city—Washington—I think there are three hot-water plants put in for one steam. It has taken the place of almost every other method of heating there. In regard to the time of changing temperature, I would say that I have had apparatus in which the temperature could be raised from that of cold water and complete circulation established, the water coming back at a temperature of 125 to 130 degrees inside of 13 minutes. I do not think any system could do much better than that.

Mr. Kent.—I would like to ask Mr. Dent how he gets over the difficulty about the leaks, raised by Mr. Warner.

Mr. Dent.—The difficulty is simply to be overcome by good workmanship.

Mr. Kent.—What as to freezing?

Mr. Dent.—I never had a case of it. There is one objection to steam heating for dwelling houses which has not been brought out, I think, and that is, no matter how well the apparatus is constructed, a very slight degree of carelessness in the management

of it will cause water-hammer in the pipes. I think we all know how disagreeable that is. Simply the shutting of one valve in the radiator will cause that. Nine people out of ten seem disposed to manage the radiator in that manner.

Mr. H. P. Minot.—I do not know but that I had better expose a little of my ignorance here in regard to this matter. I tried a little experiment on these heating radiators at one time. As to handling the valve at the radiator, I would as soon think of going to a coal mine and telling the miner to send along less coal as to attempt to control the heat in the room by operating the damper at the boiler. In the arrangement tried, the valve casting was made similar to an angle valve—a brass casting with a slide valve in it with two openings. I made an instrument with several strips of steel and brass perhaps six inches long with the convex sides together and a rivet through. I made the thing, I think, fifteen inches high. The radiator stood near the grate or mantel and I put the instrument on the mantel and fastened it to that. I operated the thing for six days, commencing Monday morning and running it until Saturday night. The windows were opened and closed several times (it was in the winter time) to see how nearly I could hold a certain temperature. I believe the proper way to do that is to get some apparatus which will keep the room at some proper temperature—wherever you may determine it to be—by using an adjusting screw to set the valve to the temperature required. As I say, the valve had two openings, with a small V-shaped notch at the bottom of each one, and the bottom notch was for the purpose of draining the water out of the regulator. I put a glass head in the outer end so that I could watch the result. Condensed water would dam up against that opening until it got perhaps a spoonful: then it would go out with a kind of a gush and be all clear until it filled up again, and I could hold the temperature of that room within two degrees of any temperature I desired from morning until night. I do not believe there is a bit of trouble in doing that. The unequal expansion of the metal would control that just as you want it, adjusting by a screw. You would have to do that to get the right temperature. You might get the room to 100, or down to 50, but there is not a bit of trouble in doing it. You can all very readily see how sensitive you could make a thing of that kind and still have it positive. I would not expect as good a result from any helix, such as this gentleman speaks of, because I do not believe it would be positive enough. There is nothing

like getting the expansion of a straight rod or something of that kind that cannot possibly get away. If you fasten the upper end of this arrangement I speak of to something which is immovable, the other end has got to go wherever you desire it.

Mr. Reese.—Another question arises here: How is water compared with steam, in respect to cleanliness? I presume this room is heated by steam. You see the dark clouds on the wall of this room over the radiators. We used to have a great deal of dirt about Pittsburgh, but now we protest against anything that looks like dirt, because our natural gas burns in a beautiful sky-blue flame. We whitewash our fire-places and grates, and dip brickbats into lime and fill up the grate with chunks of stone or brick dipped in lime, and it is all white and beautiful, a thing of beauty and a joy forever to those that have it. A radiator creates a current of air and draws the dust from the floor and darkens the wall where the dusty current strikes it. Hot water probably does not do that as much as steam, and that would seem to be quite an advantage.

The President.—Very few places are favored like Pittsburgh.

Mr. Reese.—Yes, very few.

Mr. Minot.—I do not think the steam in the radiator has got anything to do with the black deposit on the wall any more than any other heat would have (the temperatures being the same). We see the black and light streaks on the ceiling; the plastering being open or porous, the air passes through more freely between the timbers, being rarefied and moving with considerable rapidity. The small particles of dust or dirt cannot get through as small a hole as the air. Hence the deposit. The air splits and goes each side of the timbers and takes the fine dust with it. Hence the light streaks at her timbers.

Mr. Hawkins.—Referring again to Mr. Babcock's discussion, I think he lost sight of one statement in that paper. That arrangement could be made with this apparatus by which the instrument could be controlled from the apartment which was to regulate the temperature of the remainder of the house. It would be a very simple thing, of course, to make connection from the bottom of this helix up into the room and have a little handle or index to set it by. This could be done with great facility. I do not know of any apparatus that could be put in a house with better facility for varying the temperature of the rooms with small variations of temperature of the outside air. It is necessary sometimes

to have a low rate of combustion during the night. We would only have to put the pin in the proper hole to maintain it at that temperature during the night, and set it back again in the morning.

With reference to Mr. Minot's remarks about regulating at the radiator, there are a great many methods now in existence for regulating the temperature of a room at the radiator, but I do not know of any that have been successful. They are too complicated, and require too much attention. They are open to the same objection I made to the electrical apparatus; but if Mr. Minot has a successful one, I think it is to be commended except in this regard—the maintaining of the temperature in an apartment, uniform, is not the only thing. To a great many, perhaps, it is the only consideration. But to a great many others, the question of the burning of fuel is another consideratien; and if we regulate at the radiator, our fire is not taken care of, unless we have a separate regulator at the boiler. With a hot-water apparatus it would not be taken care of if we regulated at the radiator. The fire goes on unless the dampers are manipulated, and with the apartments well regulated as to temperature we might burn the house down or explode the boiler with the state of the fire neglected.

Mr. Minot.—There should be a regulator at the boiler, of course. I noticed that in these experiments of mine the pipe farthest from the valve when it was nearly closed off would begin to cool off first, and I could hold three pipes, quite warm, next to the valve. I did not notice less than three pipes. I had supposed it would take a line diagonally, *i.e.*, heating one pipe a part of the way up, the next pipe to it a little less, etc., but instead of that it would heat the three pipes straight up and down.

*Mr. J. T. Hawkins.**—Mr. Babcock, I think, somewhat misunderstands, or at least under-rates, the value of the instrument. The thermostat does work by the difference between the temperature of the fire-room and the pipe within it; and the former will vary with the temperature of the outside air, although not so greatly; but he seems to forget that provision for this very variation is made in the changing of the position of the lower end of the helical coil, from either any change in the temperature of the outer air and with it of the fire-room or desired change in the temperature of the apartment to be warmed. The temperature

* Author's closure, under the rules.

of the fire-room will presumably change appreciably only with a change in the temperature of the outer air, and therefore an adjustment of the lower end of the thermostat coil is all that is necessary at any time to keep the apartment at the desired temperature, for a given temperature of the outside air or of the fire-room as governed by the latter; so that I do not see how he can possibly arrive at the conclusion that the result will be directly the reverse of the requirements of the case, so long as provision is made for changing the adjustment of the instrument to correspond with changes in the outside temperature. If cool mornings are often succeeded by warm afternoons, a few days' use of the instrument will enable a person who attends to the fire some two or three times a day (as must be necessary in every case), to adjust the instrument so as to provide for a proper temperature in the apartment until the next operation upon the fire becomes necessary, while the instrument will take care of both the fire and the apartment meanwhile, and this is precisely what it is designed to do, and what has so far not been done with hot-water apparatus at all effectually.

To Mr. Warner's remarks, I will say that one is quite as likely to be flooded by steam-radiators as with hot-water. It is ordinary practice to set the safety-valves of steam-heating boilers, when used exclusively for that purpose, to blow off at about ten or fifteen pounds gauge pressure, while in many situations where steam from the same boiler is used for power and other purposes, particularly where it is furnished through street pipes, it is delivered to the radiators under much higher pressure. In the former case, the pressures named will support a column of water from twenty to thirty feet; and whenever, under such circumstances, but one valve of a radiator is closed—either the steam or water—as is so often negligently done, the steam radiators will fill with water quickly, if the steam-valve only be shut, from the condensation of the steam within the radiator, and the steam pressure in the boiler forcing the water bodily up to fill the vacuum; and less quickly, if the water-valve only be shut, by the accumulation of condensed water merely. Now, under either of these conditions, we are much more likely to have flooding occur, because a slight steam leak at the stuffing boxes of a steam-radiator is not noticed so long as only steam escapes; which by some is considered more beneficial than otherwise, as serving to moisten the air of the apartment; but, whenever the radiator may,

from either of the above causes, become filled or partially filled with water, and a leaky stuffing box exists, a flooding is inevitable, particularly as such is not looked for; while with a hot-water apparatus, a slight leak is likely to be detected at once and corrected. This, of course, is characteristic, particularly of what is known, in steam-fitters' parlance, as "two-pipe work," and which, for many reasons, is much preferred to "one-pipe work," in which latter the admission of steam to, and discharge of water from, the radiator is performed through one and the same pipe. In the latter, however, a similar result obtains from a defective or incomplete closing of the single valve, permitting the radiator to fill with water, from condensation, faster than it can pass through the opening of the incompletely closed valve; in fact, filling of the radiator with water in the "two-pipe" system will also occur from an incomplete closing of one or both valves, in the same way. So that it is quite wide of the actual facts of the case to say that "in the steam system no such accident can happen," for the writer has had cause to know, to his cost, that such accidents not only can, but are very likely to happen.

CCXCIII.

A NEW METHOD OF INSERTING AND SECURING CRANK PINS.

BY C. C. COLLINS, NEWARK, N. J.

(Member of the Society.)

SOME time since, in making some experimental machinery, it was found desirable to use a crank shaft in which the crank pin could be changed, sometimes using 8″ throw and sometimes using 12″ throw. That is, in a part of the experiment, the piston, which was 14″ diameter and loaded to 250 pounds to the square inch, would have a stroke of 24″; then it would be changed to 16″, these changes being made a number of times.

It will be seen that, with this pressure, whatever method of securing the crank pin might be adopted, it must be one which, when the pin was in place, must be practically unyielding and not liable to work loose; at the same time it should be one which would allow of easy removal of the pin without injury to either crank or pin.

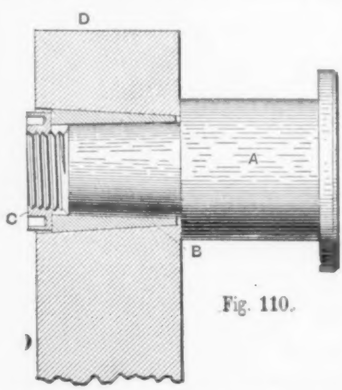


Fig. 110.

The plan which was adopted will be readily understood by reference to the accompanying sketch (Fig. 110).

The holes in the crank *D* were bored taper, largest at the back side. The bush *B* was also bored taper, and turned on the outside to fit the taper hole in the crank, so as to bring the edge to within about $\frac{3}{32}$″ of the front face of the crank. In fitting this bush, the taper hole in the crank was scraped so as to insure the absolute truth of crank pin in relation to the shaft. The pin *A* was then turned and fitted to the bush *B*, so that the shoulder should be at the same distance from the edge of the bush that the face of the crank was, viz., $\frac{3}{32}$″; this bush was then cut from end to end, so as to give it a chance to expand or contract, and it thus became a circular

wedge between the crank and the pin. The end of the crank-pin is fitted into the nut *C*, care being taken that the thread is a good fit. It is better not to make the pitch of the thread too coarse.

Now insert the pin *A*, and place the circular wedge *B* in at the back between the pin and crank, and force it well home by means of the nut, and you have a pin as rigid as if it had been put in by means of shrinking, or forced in by hydraulic pressure, and yet one easily removed without injury either to pin or crank.

This particular pin was never subject to a continuous test in running, but it showed, under a variety of trials, that it would stand up to its work without fault.

I believe that this method employed on built-up double crank would be of great utility, especially where for any cause crank-pins have to be renewed.

DISCUSSION.

Mr. E. Fawcett.—The new method of inserting and securing crank-pins, presented to this Society by Mr. Collins, deserves to be greatly appreciated, as it is certainly one of the best for securing and removal of pins without injury. In my practice I have used, on an ordinary taper pin inserted directly in crank, a nut, one end cone-shaped and fitting to a corresponding counter-sink in crank, with good results.

Mr. J. T. Hawkins.—Mr. President, I would like to say one word with reference to this matter. This method of inserting the crank-pin, as shown in the diagram, I should criticise in this respect—that it detracts very much from the amount of bearing that the pin has in the metal of the crank, and there are very many situations where it would be inadmissible to have the nut project beyond the face of the crank. As shown in the sketch, it takes away at least one-third of the bearing, and makes it extremely short compared with the remainder of the pin. I should say, in situations where the thread could be extended entirely outside of the metal of the crank and have a proper nut projecting, that it would be an excellent device.

Mr. H. P. Minot.—A good many of these gentlemen here have put in crank-pins. I have put in a few myself. I have always had pretty good success with a pin by giving the proper amount of shrink and making a straight hole. I cannot see any particular benefit in this arrangement. It certainly is more expensive.

There is no trouble in putting a pin in without anything except the pin and the hole, and getting it perfectly tight, so that it will stand all the duty. I have seen pins get loose by overtaxing the engine, and being crowded off sidewise. It seems to me that this is taking a roundabout way to get at it. I believe the shortest cut to anything to be the best way.

The Chairman.—Mr. Minot forgets that this was a peculiar pin to fit a certain place. It was a pin designed to move from one hole to another, giving the engine two different strokes. He simply presents the paper as a way of doing that particular thing, and not for crank-pins in general, although he suggests it might be a good way for crank-pins in general.

Prof. Hutton.—I should like to ask Mr. Minot what allowance he makes in practice for shrink.

Mr. Minot.—I think about $\frac{1}{50}$ of an inch to 10 inches in diameter, possibly a little more than that, would be, in my judgment, about the right thing. I think it is weakening to the material to get too much shrink. I have seen cranks broken by shrinking them on the shaft. It is an easy matter to do it if you get too much.

Prof. F. R. Hutton.—The reason I asked the question was that one of the members of the Society was in its office the other day, and was talking about a little experience which he was then undergoing. He had been asked to cast paddle-wheel centres for a river boat engine, and the specifications called for a shrunk wrought iron ring over the hub and the outside. He had finished his casting, and made what he considered a shrink-fit for the ring. He made an allowance of $1\frac{1}{4}$ inch for shrinkage on the circumference of 10 feet in diameter, and delivered the job, with the ring shrunk on, to the parties. They proceeded to take a 38-pound sledge hammer and see whether that shrunk ring was a tight fit. The allowance which he had made was just sufficient so that the strain on the wrought iron shrunk ring should not exceed the elastic limit of the material. The strain which the person to whom he was furnishing the wheel wanted was such that it would resist a sledge-hammer blow, and in order to get that amount of shrink fit, the amount of allowance would have to be so great that practically the iron band was stretched up to its elastic limit before it was put to service at all. The question of shrink, this gentleman said, was one that could very properly be ventilated in the Society, and a little knowledge extended as to the amount of strain which a shrunk ring ought to be exposed to. When the first ring

was removed, it showed that it was still $\frac{3}{4}$ of an inch less in circumference than the casting which it was to fit. The member whom I am quoting uses an allowance of $\frac{1}{1000}$ of an inch to the inch.

Mr. G. M. Bond.—Referring to shrinkage allowance for locomotive tires, I have here a circular which was issued by the Secretary of the American Railway Master Mechanics' Association, date of September 15, 1886, in which the shrinkage allowance is given, determined upon for the different sizes of driving-wheel centres adopted for standard locomotives, the smallest size being thirty-eight inches and the largest sixty-six inches diameter. The allowance given for shrinkage for steel tires, in order to insure a perfect fit—that is, tight enough to stand the required service—is, for a 38-inch wheel centre, 0.040 of an inch; for a 44-inch, 0.047 inch; for a 50-inch, 0.053 inch; for a 56-inch, 0.060 inch; for a 62-inch, 0.066 inch; and for a 66-inch, which is the largest size adopted as standard, 0.070 inch in diameter; making the shrinkage allowance about $\frac{1}{80}$ of an inch per foot of diameter of tire or wheel centre.

This is the average of the practice of a number of best locomotive builders and managers of rolling stock of this country, and was determined upon by a committee of the association referred to, including Mr. James N. Lauder, Superintendent Rolling Stock of the Old Colony Railroad, Mr. Jacob Johann, now Superintendent of Motive Power, Texas & Pacific Railway, and Mr. H. N. Sprague, Superintendent of the H. K. Porter Locomotive Works, in Pittsburgh. Mr. Lauder, in his former experience, used a little less than the allowances mentioned, and Mr. Johann a little more, but with a tire so shrunk on, I think there will be no trouble experienced in regard to its becoming loose, nor to its being unduly strained with such shrinkage.

I think it would stand a little more than that; I think where there is plenty of metal around the pin a greater allowance could be safely made; but in the case of steel tires, of course there is a greater amount of surface in contact, and the section of the tire is small in proportion to its diameter, so that it is important that the strains should be kept well within the elastic limit, to prevent possible breakage under the conditions of severe cold weather.

Mr. Lauder states that tires have never become loose in his practice, and this would be within nearly the same limit, corresponding closely with the practice in Germany.

Mr. Wm. Kent.—The shrink is $\frac{1}{80}$ of an inch to the foot?

The Chairman.—Yes.

Mr. Kent.—That is part $\frac{1}{960}$ part of the whole. Allowing steel to have a modulus of elasticity of thirty millions, that would be equal to a strain of a little more than thirty thousand pounds to the square inch—which would be under the elastic limit of most tire steel. That would be very safe.

Mr. Jacob Reese.—In determining the relation of shrink to the tensile strength of iron and steel, I have no direct experience in that line, but I presume shrinking is always done, where it is expected to be anywise accurate, at a certain given temperature. The strength of iron and steel will vary with the temperature. In determining the strength, or the limit or the modulus of elasticity, it should be done at a given temperature. There ought to be a zone of temperature for all tests, and all tests should be adjusted to that. Now, why is that? Because temperature being the measure of molecular velocity, as weight is the measure of matter, the molecular activity is greater at one temperature than at another, and the strength of the material will depend upon the molecular activity. A body of iron is not a continuous body. It may be physically divided, the end of physical division being the molecule. That may be divided by chemical action; the end of that is the atom. Neither the atoms nor the molecules exercise any energy. They are inert *per se.* Their energy is derived from the physical forces that they are endowed with. Consequently, the greater the distance these molecules or these particles are one from the other, the less the strength of the material will be. So that this question of temperature has a great deal to do with the strength of the material. It also has to do with the expansion and contraction; for every additional unit of temperature produces an increased distance between the molecules or the atoms, and consequently they will go and come with every change. We find that carbon has a great deal to do with that, and when we talk of giving a certain expansion, it depends largely upon the carbon; it depends largely upon the other constituents which are in there, and the reason why one temperature will not answer in one case and will in another, is because one metal is higher in these elements that give this differential expansion than another. I remember buying an engine some years ago and looking at its pedigree, as they say up here in Kentucky about the horses, I found that it was born the same

year I was—1825—and the crank pin was evidently shrunk in, and it is working in Pittsburgh to-day as sound as it was when it was put in. You could not better that. It was put in, evidently, with a certain shrinking to suit that class of iron. I know of another case where the most expert mechanic we had in Pittsburgh shrunk a crank pin in, and the whole thing burst and cost me about $5,000 for repair. He did not know what he was doing; the other fellow did.

Mr. Bond.—I would like here to give the figures for one size wheel centre in the German practice to show how much they consider is the tensile strain per square inch of section with the allowance for shrinkage they have adopted. For a locomotive wheel centre, the diameter of which is equal to 69.04 inches, the shrinkage allowance in inches is 0.059, which is a little less than that adopted by the Master Mechanics' Association, being about 0.01 inch per foot of diameter of tire. They have made a great many experiments to determine the amount of compression of the wheel centre under this strain. It is found to be, in the size mentioned, equal to 0.01 of an inch, decreasing the diameter of the wrought iron wheel centre by this amount, and the strain in tons per square inch of section of the tire so shrunk is given as 9.2, and for other sizes down to 35-inch wheel centres, the strain varies from 9.2 to 10.8 tons per square inch.

It thus appears that they have made careful tests relating to this subject, and that their shrinkage allowance is but a little less than that of the practice in this country.

Prof. Sweet.—In a case where it became necessary to remove a certain locomotive wheel at Syracuse, they found it impossible to remove it from an axle with the means at hand. They then cut off the tire and the wheel was removed without the aid of any mechanical means whatever, which showed that the hole inside of the wheel had been compressed on the axle.

*Mr. C. C. Collins.**—In reply to the objection of Mr. Hawkins, "that the use of a nut as shown in sketch diminishes seriously the effective bearing of pin in the crank," I would say that in practice the cases are few where the nut could not project beyond the crank, and in such cases good results can be secured by boring the crank straight for the depth required by the nut and having the nut a snug fit in this straight bore; also that a hole could be drilled through the centre of the pin and a button-headed bolt

*Author's closure under the Rules.

used, with the head bearing against the circular wedge and the nut at the outer end of crank-pin.

However it was not designed that this method of inserting pins should be substituted, in ordinary practice for the usual methods of shrinking or forcing in the crank pins; but for these cases, and they are quite numerous, where the pin has to be even occasionally replaced.

I believe that for "built-up" double cranks, no better method has yet been devised, as where there are three disks, with their pins and shafts, it requires very expensive appliances and very good workmen to secure a good job, by shrinking or forcing in the pins; but should this method be adopted it comes within the scope of any ordinary shop.

I have found in shrinking in pins, for moderate sizes, if the end of the pin can be driven into the bore of crank from half to three-fourths of an inch by easy driving with hand hammer, it will be as snug a fit as can be desired. This is something less than a hundredth of an inch allowance. I also have been accustomed to turn a slight groove next to the shoulder of the pin that comes against the face of the crank. This groove should be about $\frac{1}{8}$ inch wide and $\frac{1}{64}$ inch deep, with filleted corners. With such a groove, it takes a large increase of power to withdraw the pin over that required when there is no groove.

CCXCIV.

A SAFETY CAR HEATING SYSTEM.

BY HENRY R. TOWNE, STAMFORD, CONN.

(Member of the Society.)

In response to the urgent demand of the public, emphasized in several of the States by legislative enactments, many improvements have recently been devised in methods of heating railway cars. The terrible loss of life which has resulted from railway accidents in which the cars have been set on fire by the stoves used for heating, has led to an imperative demand that the ordinary car stove shall be abolished. Experience has already settled conclusively that this change is not only easy of accomplishment, but also that the economy resulting therefrom is so great as to make it directly to the interest of the railroad companies to effect the change as quickly as possible whenever they are satisfied that the system which they propose to adopt has been perfected to a degree which makes it probable that the work done now will prove permanently satisfactory.

When the project was first mooted of using steam from the locomotive to heat the ordinary passenger train, consisting often of ten or twelve coaches, grave doubts were expressed as to the ability of the present locomotives to respond to the extra duty which would thus be put upon them. The earlier students of this question, among them the writer, were therefore compelled carefully to consider the questions of thermodynamics which were involved, and to base their further projects upon this theoretical study. The writer's careful examination of this question led him, long ago, to the conclusion that the additional duty on the ordinary passenger locomotive, under quite severe conditions, would not exceed five per cent. of its efficiency. The correctness of this calculation has now been corroborated by the results of actual experience, and there is no longer any discussion as to the ability of a locomotive to furnish the steam needed for efficiently heating the train of cars behind it. The experience referred to has also demonstrated that the additional coal burned in the locomotive is but a small fraction

of the amount which has heretofore been required for supplying car stoves, of the best kinds in use, and that this economy in fuel is so great as to enable the expense of applying the new system to be met out of the savings resulting from it during a few years' use.

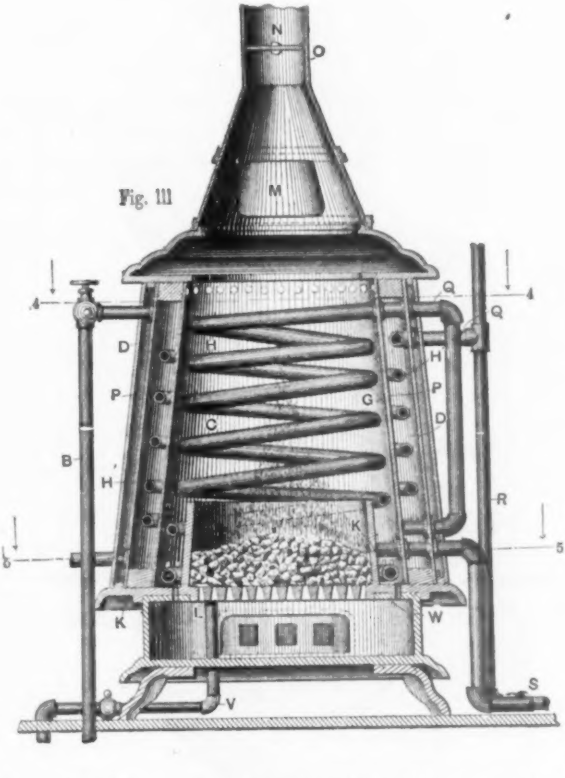
The system herein described contemplates the retention of the well-known Baker heater, or its equivalent, and the use of hot water for the local circulating medium within each car, although modifications of the system adapt it equally well to the Spear stove, or to any other apparatus having a hot-air circulation. The description herein given, however, will be confined to the system as applied to a hot-water apparatus.

Fig. 111 shows the ordinary Baker stove with the additional fittings needed to adapt it to this system. The ordinary fire grate is retained, and within the combustion chamber C above it is the usual heating coil H, containing the circulating medium, which is preferably ordinary water, salted sufficiently to prevent it from freezing. The cooled water, returning after circulation around the car, enters this coil at its lower end and passes out of its upper end, going again down through the connecting pipe Q, and entering at the bottom of the exterior heating coil H′, through which it again rises and passes out at the upper end of the latter, through the pipe Q into the standpipe R. At the upper end of the latter is provided the usual expansion reservoir, while its lower end connects with the system of circulating pipes under the car seats, the return or other end of which connects with the lower end of the heating coil H previously referred to. The secondary coil H′ is contained within an annular steam chamber, outside of which is the usual sheet-iron jacket P, with an air space intervening to retard radiation. Steam from the locomotive is carried under the train by a supply pipe, with which connection is made through the steam pipe B, at the head of which is a valve for controlling the admission of steam to the steam or "transfer" chamber D, within which the heat of the steam is transferred to the water of circulation contained within the coil H′. The water of condensation, from the steam or transfer chamber, is taken out through a suitable pipe near the bottom of the chamber, and may be returned to the locomotive or otherwise disposed of.

At the bottom of the stove, in a narrow space surrounding the fire pot, is a gas pipe W, shown in Fig. 111, and also in Figs. 112 and 113, which are transverse sections of Fig. 111, on the lines 4—4 and 5—5 respectively. Gas is admitted to the annular pipe W

through the supply pipe V, and is utilized by means of any proper form of gas-burner, so that the heat of its combustion is made available in the combustion chamber C, just as in the case of the combustion of solid fuel on the ordinary grate of the present stove. It will thus be seen that the heater or stove in the new system pos-

Fig. 111



sesses three functions, capable of independent or simultaneous operation, viz.:

1. The heating of the water of circulation by means of steam from the locomotive, in the transfer chamber D.

2. The heating of the same circulating medium by the combustion of gas fuel within the combustion chamber C.

3. The heating of the same circulating medium by the combustion of solid fuel of any kind in the combustion chamber C.

The effect of heat applied by any of these three methods to the circulating medium within the coils H, H′, is identical. The increase

in the temperature of the water within the coils changes its density, and hence its gravity, thus causing it to rise, the volume of water thus displaced being replaced by an equal volume of cooled water flowing back from the car, the action of this part of the apparatus

Fig. 112

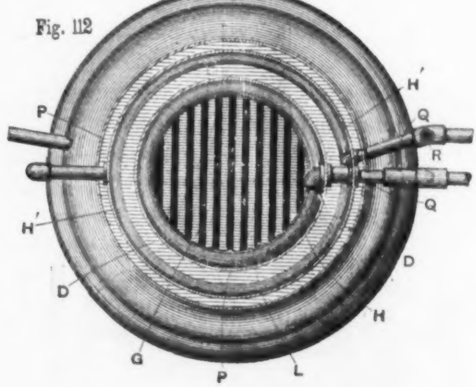
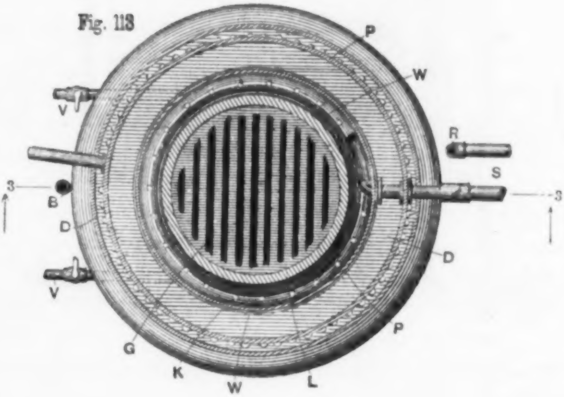


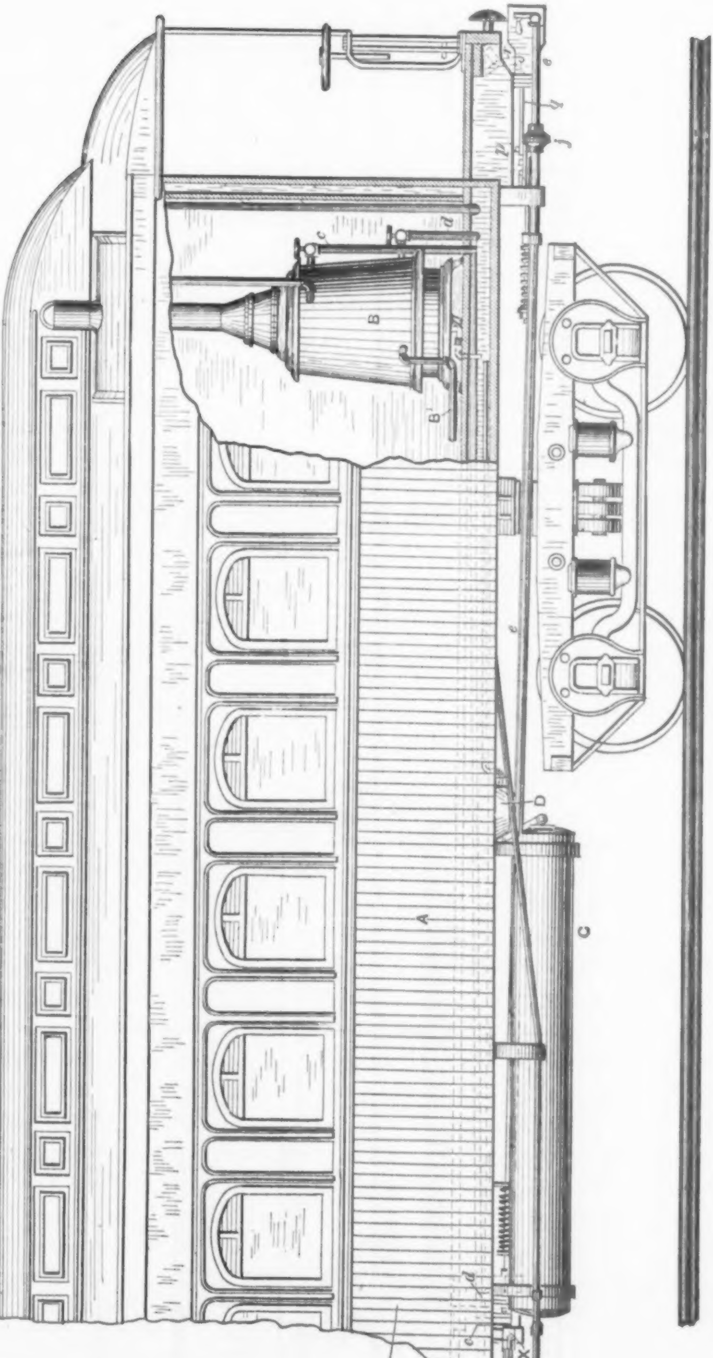
Fig. 113



being precisely similar to that of any of the well-known arrangements of hot-water apparatus.

The application of the complete apparatus to a car is shown in Figs. 114 and 115, the former being an elevation of one end of a passenger car, with the side removed so as to show the stove and its connections, and the latter being a floor plan of the car. One or more gas storage tanks C are provided under the car, within which

Fig. 114.



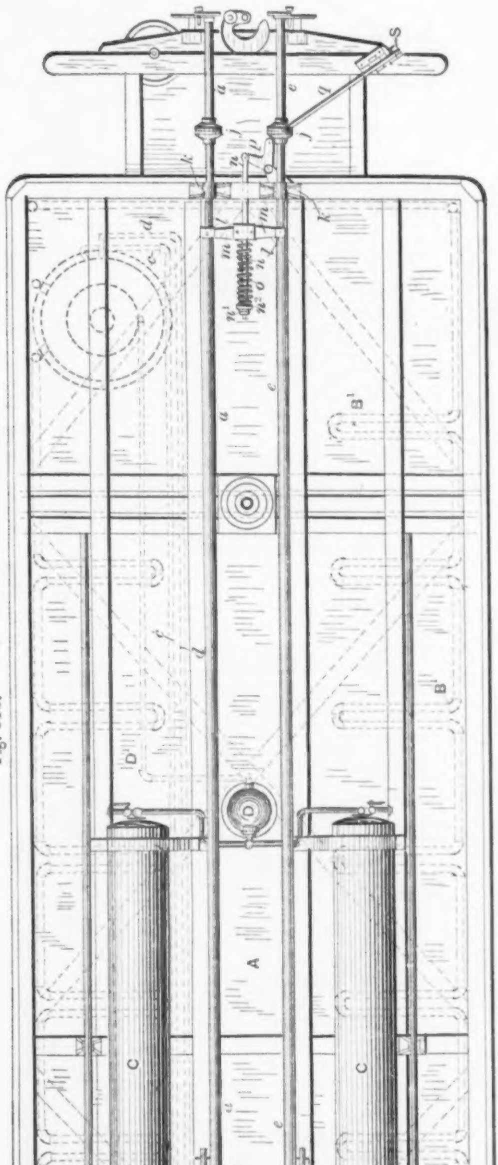


Fig. 115.

gas, compressed to ten atmospheres or more, is stored for use, both for lighting and as an emergency fuel. The pipes from these tanks pass through the governor or reducer D, by which the pressure is diminished to the desired tension, and from which the gas passes through the pipe D′ to the stove, where it can be conveniently turned on or off by a cock, in the usual manner.

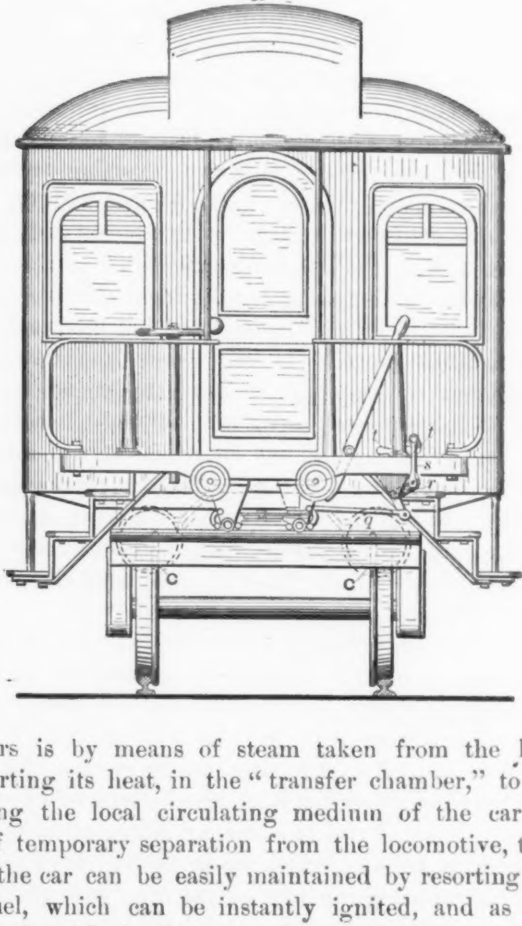
An improved method of piping the cars for conveying steam from the locomotive, and conveying the water of condensation back to it, is shown by Fig. 115. This consists of a double line of pipes, extending from the locomotive under each car, one of which is used as a steam main, and the other as a return or condense-water main. Each of these pipes is interrupted at or near the centre of the car, and there connected with a "centre fitting," having proper ports or passages in it, and having, also, two reversing valves, by means of which either line of pipe can be connected with the steam pipe leading to the transfer chamber in the stove, and the opposite line connected with the condense-water pipe returning from the stove. The details of this apparatus have all been fully worked out, and the "centre fitting" constructed, but further description of them here will be omitted. In like manner, the drawings indicate an arrangement of coupling devices between the cars, whereby each pair of longitudinal pipes can be conveniently coupled to those on the adjoining car, but these details will not be further described in this paper. The main features of the system herein described may be utilized either with a single or double system of longitudinal pipes, connected between the cars by any proper form of coupling device. An end view of a car fitted as above described is shown in Fig. 116.

Although the apparatus above described shows the several devices for imparting heat to the circulating medium as being combined within a single structure, it is obvious that the same results can be otherwise accomplished if preferred. For example, Fig. 117 indicates the floor plan of a car having the gas storage tanks and other features above described, but with *two* heaters, at opposite corners of the car. Each of these heaters or stoves contains one of the circulating coils, both of which are in the circuit of the pipes containing the circulating medium of the car. One of these heaters contains the transfer chamber, within which the heat of the steam from the locomotive is transferred to the local circulating medium of the car, and which may be located at any convenient point within or under the car. This heater may also contain the gas combustion

chamber, or, if preferred, the latter may be arranged within the other heater, which, as shown in the drawing, consists of the present Baker stove, without modification, this being retained only for use in case of emergency.

From the foregoing description the operation of the apparatus will be readily understood. The normal method of heating each

Fig. 116



of the cars is by means of steam taken from the locomotive, and imparting its heat, in the "transfer chamber," to the water constituting the local circulating medium of the car. During periods of temporary separation from the locomotive, the temperature in the car can be easily maintained by resorting to the use of gas fuel, which can be instantly ignited, and as easily and quickly extinguished whenever the car is again coupled into a train. Finally, to meet the case of an emergency in which the car is isolated from its locomotive, or the latter is without steam, and in which the supply of gas fuel has been exhausted, there

remains the ordinary fire-box and combustion chamber, in which solid fuel of any kind, either coal or wood, can be utilized.

Dependence upon steam from the locomotive alone may be accepted for suburban and local trains, but for other trains, liable to detention, there must be some supplemental heating device, and for these the use of gas fuel possesses many advantages. For many kinds of service, however, it will be necessary that the cars continue to have provision whereby, under exceptional circumstances, the temperature can be maintained without dependence either upon the locomotive or a supply of gas, and hence it is probable that the ordinary stove or its equivalent must always be retained. The system described combines all of these essentials in a simple and harmonious manner.

As the use of gas fuel for the purpose herein proposed is as yet experimental, it is proper to add that careful calculations have determined the fact that it is easily possible to provide tank capacity under an ordinary car for carrying an amount of gas which would maintain the temperature of a car in zero weather, without any assistance from the locomotive or from other fuel, for a period of *at least* ten or twelve hours, and probably for twenty-four hours. The gas supplied for this purpose can also be utilized for lighting the car, thus eliminating the oil lamp, which, with the common stove, constitutes at present, in the winter at least, the greatest danger to the traveler under our railway system as now organized.

[NOTE.—It is proper to state that the system herein described is the subject of application for patents now pending in the U. S. Patent Office, and that the introduction of the system has passed at the time of this writing into the control of a company organized in New York.]

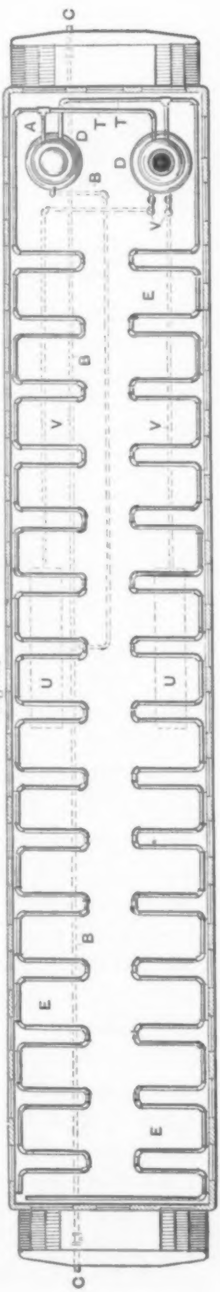


Fig. 117

DISCUSSION.

Mr. Geo. H. Babcock.—Taking high pressure steam from the locomotive for heating the cars has seemed to me but changing the character of the danger in case of a collision. With the stoves there have been many instances of setting fire to the cars, and consequent agony and death to many persons. But with live steam, the rupture of the pipe or connections releases a death-dealing agent quite as destructive as fire. In fact, with the stoves there is a chance that the car will not take fire, but with the steam pipe there are few chances to escape scalding. An evident remedy for this is to take the steam for heating the cars from the exhaust pipe of the locomotive below the nozzle. There is sufficient pressure for heating purposes, at this point, when the engine is at work. When it stops, the supply stops automatically, and no possible harm can come of it when there is a smashup. There is also an element of economy in this, though it is of less importance than safety.

But to make this available it is necessary to use larger pipe and some process of storing up heat for use when the cars are still. It would also be necessary to have some independent means of warming the car before starting out. The plan presented in this paper, as well as that in the paper of Mr. Baldwin, uses probably the best method for storage by causing the steam to heat a quantity of water sufficient to tide over any reasonable stoppage. This plan of Mr. Towne's also provides a means of heating at any time when the locomotive is detached. The car could be heated before starting from a boiler located in the station with a series of pipes and couplings, which would have many advantages over separate fires, but Mr. Towne's plan has some other advantages, particularly where the car is liable to be caught out in a blizzard.

The expensive steam chamber, with its supplemental coil, in Mr. Towne's arrangement, seems unnecessary, as a simple pipe enveloping the stand pipe will give all the surface necessary. I heat my conservatory by hot water, which water is in turn heated by a pipe from my house-heating boiler somewhat in the same way. Fig. 274 shows the heating arrangement. There is only three feet in level between the hot and cold pipes of the circuit, and these four-inch circulating pipes are connected by a vertical pipe

five inches in diameter. Through the centre of this vertical pipe runs a two-inch steam pipe connected with the boiler. This three feet of two-inch pipe furnishes all the heat necessary to supply something over one hundred feet of four-inch hot-water pipe and keep the conservatory, 16 × 20, warm. According to the paper of Mr. Baldwin, this is sufficient surface for a passenger car. In my case the steam is rarely over two or three pounds pressure at

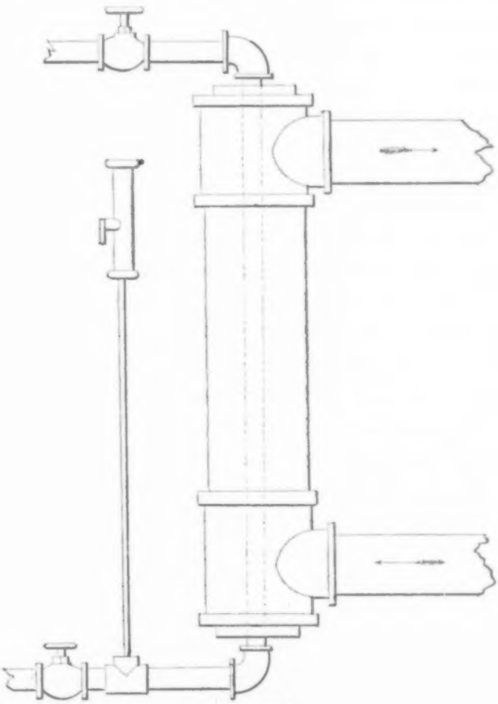


FIG. 274.

the boiler, and the conservatory is about fifty feet distant. It would not seem difficult to apply exhaust steam from a locomotive to heating cars in a somewhat similar manner.

Prof. F. R. Hutton.—I might state, in discussion of the paper, that last winter—on the 31st of December—I was asked to look into some of the details of this heating system proposed by Mr. Towne; the experiment being to take the vestibule train on the New York Central Railroad, which had been fitted by this system, and alongside of it in the yard of the Grand Central Station to put

another train fitted with the ordinary Baker car heater, and to see which of the two systems gave the greatest temperature in the cars in the same time, the conditions being the same. The experiment was not conclusive for either, because they both took about the same time—except in so far as it showed that the system proposed by Mr. Towne was as efficient as the other system in heating a car from a temperature of 28 degrees inside, which it was when we boarded the two trains, up to a temperature of 55 degrees, when the experiment had to cease on account of the necessity for cleaning the cars.

Mr. J. L. Gobeille.—I might say the great difficulty in carrying heat by steam is in the connection between the cars—to have a connection which will not sag when the cars come together and fill up with water from the steam condensation in cold weather (requiring too much power to blow it out). There has to be about six or seven inches leeway in length, I think, and about three or four inches laterally. On a road like the one which we came over last night, it would probably require a little more than that, and there has been considerable discussion as to how to get this joint. A telescopic joint would perhaps be as good as anything, but such joints are apt to wear and leak. I should like to hear some discussion as to this question, with sketches on the board, if anyone is prepared to give any information.

Mr. H. P. Minot.—I have given this matter a little thought myself. I thought I would connect it with a hose and crook it up instead of down, and hold the largest part of the crook, perhaps, with a spring which would yield a little as the car swung around.

Mr. Wm. Kent.—I think I would make that joint like the letter S laid horizontally, and hang it in the same way.

Mr. Towne.—In reply* to the foregoing discussion, I have to say that experience has already settled conclusively the most important questions relating to the continuous system of train heating by means of steam from the locomotive. The feasibility of the plan is thoroughly established, and all that now remains to be done is the perfecting of the necessary details.

Mr. Babcock's objection that the presence of live steam in a car constitutes an element of danger, would be well taken if it were proposed to carry a considerable volume or pressure of steam within the car. The system as heretofore used eliminates this danger by locating the steam drum or transfer chamber underneath

* Author's closure, under the Rules.

and outside of the car, all of the steam connections being similarly located, and a loop of the water circulating pipes being carried through the floor of the car into the transfer chamber. Even where the drum is located within the car, the volume of steam it contains is small, and of low tension.

As compared with the danger of a stove, containing a large mass of solid fuel in active combustion, the steam system under discussion is infinitely safer. Even the small element of risk which it now contains will doubtless be largely, if not entirely, eliminated as the system is further developed by experience.

CCXCV.

CONNECTING RODS.

BY W. F. MATTES, SCRANTON, PA.

(Member of the Society.)

THE object of this paper is to call the attention of engineers to a form of connecting rod which has been adopted by the writer in his practice at the Lackawanna Works.

It differs from the ordinary "marine" rod, first, in having jaws of sufficient length to hold both boxes; and, second, in having a cap which hooks over projections on the jaws. These projections are turned concentrically with the rod, and the cap hooks are counterbored to match. The cap is thus held in position by the combination of hooks, box-flanges and bolts; but the long jaws and the counterbore of the cap relieve the bolts of any other duty than merely clamping the parts together, and no particular accuracy need be observed either in turning the bolts or drilling the holes for them. In large rods, it is sometimes a convenience that the cap has less weight than that of the marine type. As compared with rods having the outer box secured by a transverse bolt passing through eyes in the jaws, several advantages are claimed. The flat parallel sides of the head facilitate the operations of forging and finishing; the tensile stresses are taken by bolts, in which the required strength can be easily and certainly secured, instead of by eyes, of uncertain condition, cut out of a large forging; an unsightly swelling at the end of the rod is avoided, and the weight sensibly reduced. Rods with the transverse bolt are frequently seen with flat-sided heads, but the result is achieved by a disregard of nicety in proportioning which is allowable only in small sizes. For the main rods of locomotives having two pairs of drivers, the transverse bolt is most convenient, but this is a special case, and the precedent should not be blindly followed. As compared with the old strap-rod, we find the new form cheaper to fit up and better in use. As compared with the solid-head rod, the open end is a great convenience, except on crosshead ends where the pins are removable; the jaws hold the boxes with equal rigidity, permit

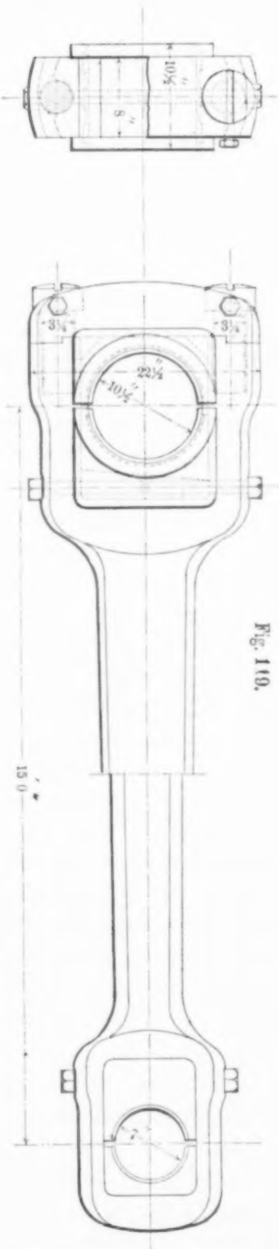


Fig. 119.

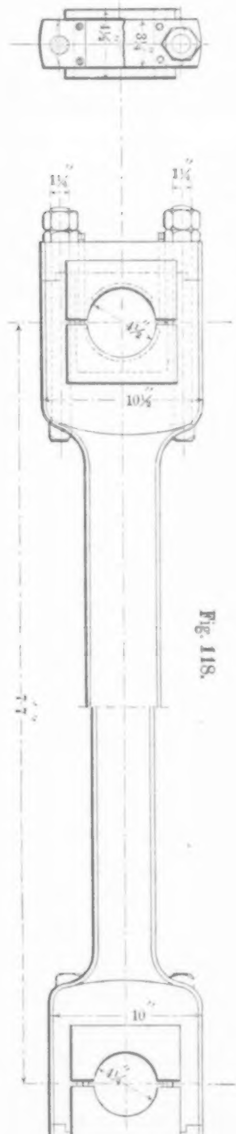


Fig. 118.

the use of full flanges on both boxes, and do away with loose collars on the crank pins.

Fig. 118 shows a plain form of the new rod. This was designed for use in pairs, connecting the ends of a crosshead with a pair of fly wheels. In such a case it is usually safer to take up the wear by liners, rather than by wedges or cotters. The opposing screws pass loosely through the caps, and are threaded in the boxes. We have discovered no advantage in the use of these screws for low speeds, and have had no opportunity to test them otherwise. For small rods we prefer the plain, straight caps shown in this figure. They are convenient for fitting, and the distribution of metal is better than appears at first sight. The bolts have round heads slightly countersunk, and are kept from turning by a spline.

Fig. 119 is the rod used in a pair of horizontal, coupled blowing engines, with $52'' \times 60''$ steam cylinders. Here a wedge was almost a necessity, and to clear the adjusting-screws, tap bolts, instead of through bolts, were adopted to secure the caps. As before, the holes are loosely drilled through the caps, the holes in the rod being threaded by special taps. The bolts are steel, $3\frac{1}{2}$ inches in diameter, and are chased 5 threads to the inch. All parts, excepting the boxes, are hammered steel. The wedges are full width, and threaded for the adjusting-bolts, which are in tension. The boxes are "Eureka" steel cast, fully faced with babbitt.

Fig. 120 is a rod designed for a double crank, where a longer journal, and consequent wider head, admitted four through-bolts, spaced to clear the wedge-adjusting screws. The crosshead end has the wedge under the cap—an arrangement that tends to preserve the correct length between centres of journals. Practically the same end may be secured in a rod like Fig. 119, by the occasional introduction of a liner between the cap and the box.

In Fig. 121 is shown the crank end of one of a pair dropped from the overhead beam of a compound engine. The cap is formed with an eye, bushed with hardened steel, to which is secured the forked end of a smaller rod for driving the air-pump. At each mid-stroke the thrust of the air-pump is delivered at an angle of about 45° to the cap; thus imposing a somewhat unusual test, but no loosening has been developed. The method of lubricating this design is also illustrated. The flow of oil to the lower journal is controlled by the milled head at the upper end of the long tube.

In Fig. 122 we have one of a pair of short links connecting the journals of a beam with a crosshead below. The construction is

Fig. 120.

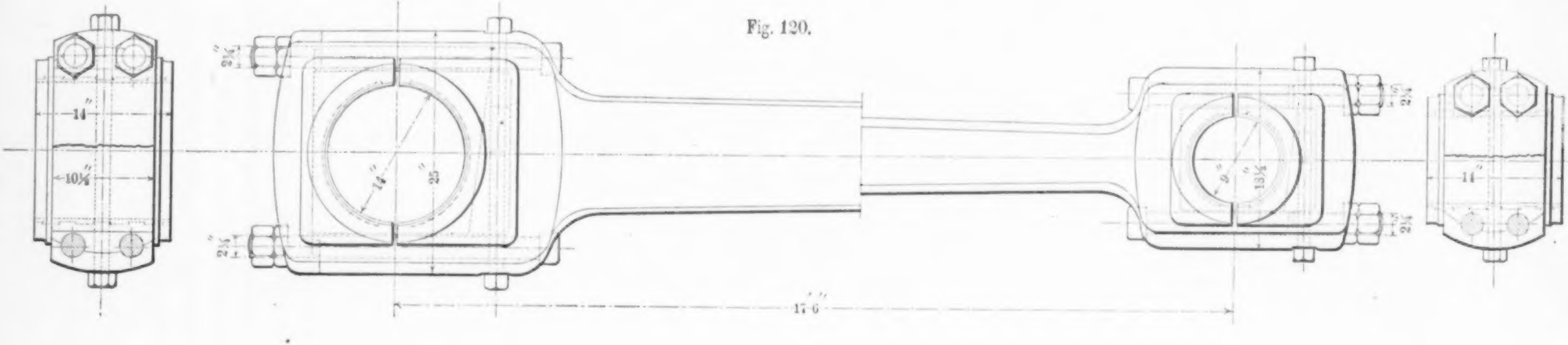
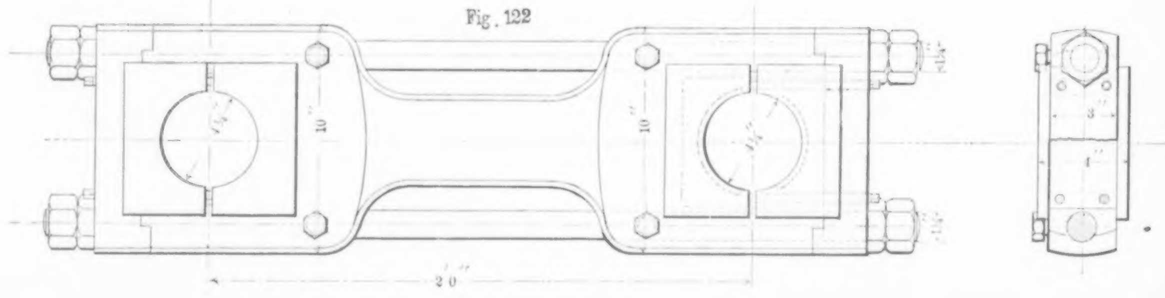


Fig. 122



similar to Fig. 118, except that the bolts pass through both ends, and are kept from slipping during erection, or at any time when the caps have to be loosened, by the small set-screws shown on the sides of the heads.

In the construction of these rods, the head is first forged solid. Holes are then drilled for the interior angles, and narrow slots run

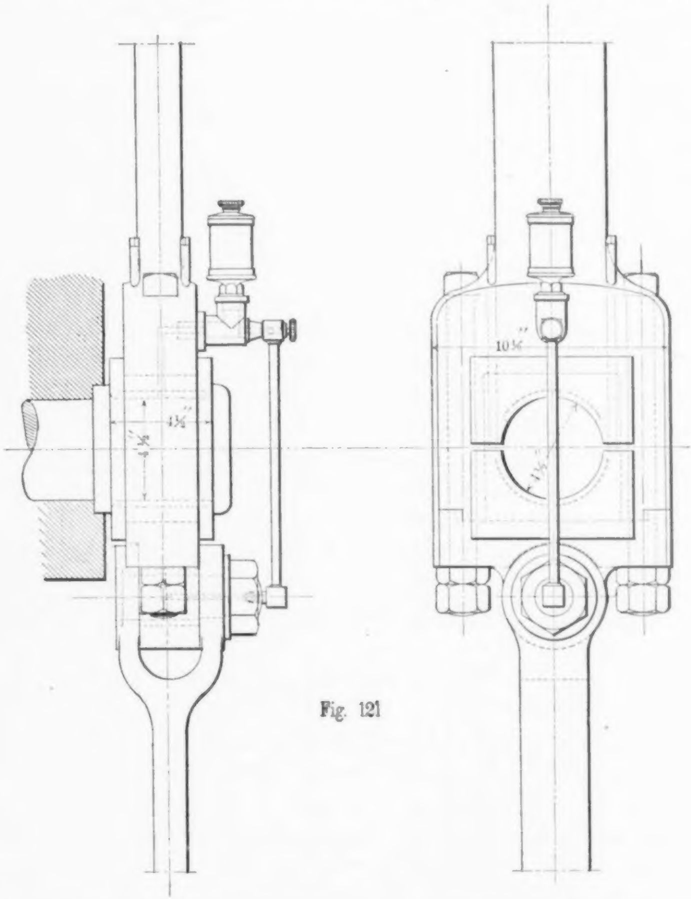


Fig. 121

to them ; leaving a middle tongue intact, to carry the centre. As the slotting, particularly in large sizes, is likely to cause a slight springing, it should be completed before the projection on the jaws are turned to a finish, which operation follows next in order. The middle block is then entirely slotted out, caps clamped on, and the sides planed off together.

CCXCVI.

NOTES ON WARMING RAILROAD CARS BY STEAM.

BY WM. J. BALDWIN, NEW YORK CITY.

(Member of the Society.)

DURING the early part of the winter (December, 1887,) the writer was engaged in making experiments on the Long Island Railroad for the purpose of determining the probable amount of steam required for the warming of a train or car, and also to ascertain the length of time one of Gold's storage car heaters would maintain the heat of a car after steam was shut off or the car side-tracked and the locomotive removed.

The better to record the experiments, a diagram was made of the day's observations, a copy of which is hereto appended (Fig. 123).

The line of figures at the bottom reading from left to right shows the time. The line of figures at the left reading upward shows the temperatures in degrees Fahrenheit. The ordinates of the lower irregular line show the temperature of the air outside the car. The ordinates of the next two lines show the temperature maintained inside the car by two thermometers placed in different positions, and the ordinates of the upper two lines show the temperatures of the surface of the outside cylinders. The points at which steam was turned on and shut off are noted, and the amount of steam, in pounds weight, condensed, as found by the water condensed, is also shown.

Before going any further, however, explanation will be given in detail as to what the storage heater consists of. Fig. 124 shows it in cross section as applied to car 190 of the Long Island Railroad. A is a 5-inch diameter boiler tube of any required length (18 or 20 feet), with a cap on each end; B is a similar tube 4½ inches in diameter with welded ends, seven-eighths filled with water and a solution of common salt; C is a steam space between the outer and inner tubes; D is a block of iron to hold the inner cylinder against the internal top of the outer cylinder; E a shield and foot-guard, and F a cast-iron stand or leg fastened to floor of car.

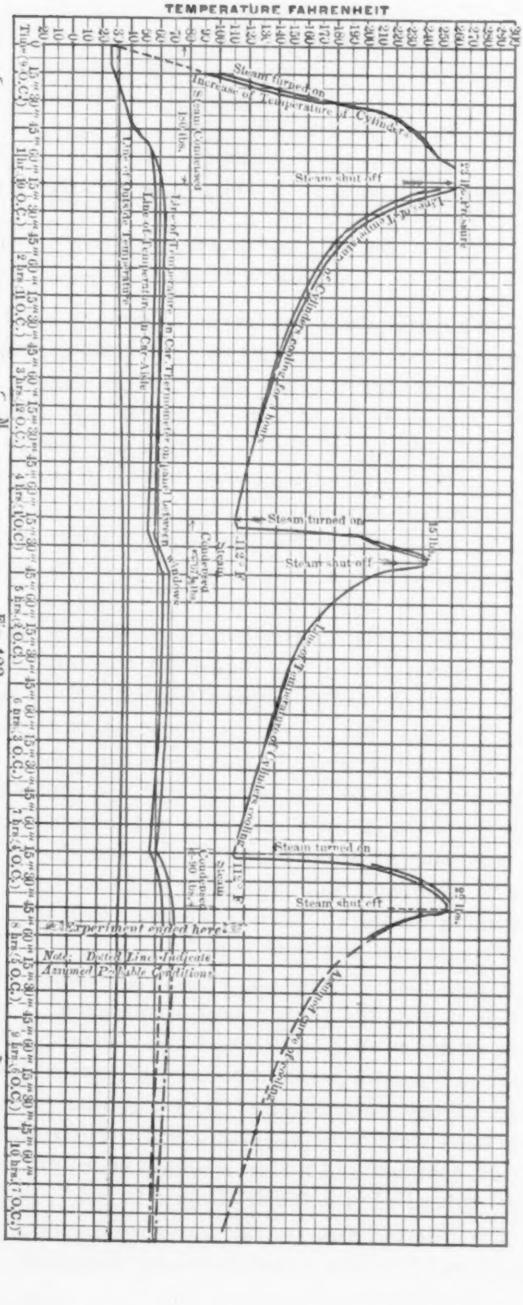

TEMPERATURE FAHRENHEIT
Steam turned on
Increase of Temperature of Cylinders
Steam shut off
Line of Outside Temperature
Line of Temperature in Car Aisle
Line of Temperature in Car, Thermometer on panel between windows
Cylinders cooling for hours
Line of Temperature of Cylinders cooling
Experiment ended here
Note: Dotted Lines indicate Assumed Probable Conditions
Assumed Curve of cooling

Fig. 123.

In reference to the measurements, capacity, etc., of the warming cylinders each .764 running foot has one square foot of heating surface; (2) one running foot of length of cylinder has 1.309 square feet of surface; (3) a running foot of the inside cylinder contains 7.303 lbs. of water, when seven-eighths full; (4) one running foot of the inside cylinder and one running foot of the outside cylinder weigh together 13.4 lbs.; and assuming that the water is put into the cylinders at 62° Fahr., the iron of both cylinders has a storage capacity equal to 1.5247 lbs of water at 62°, and (5) a

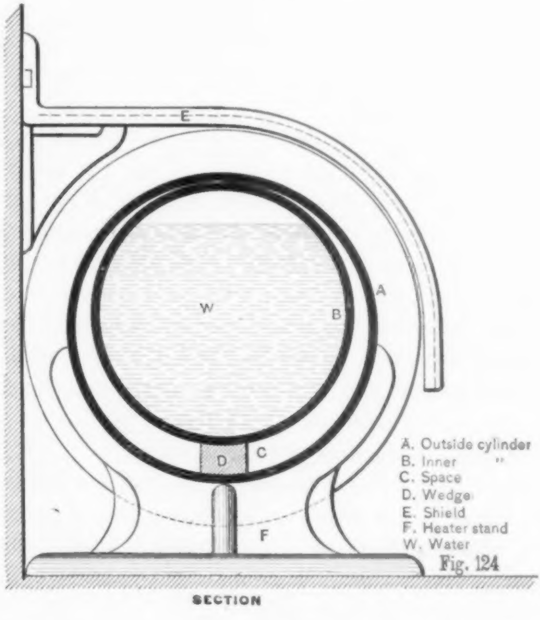


Fig. 124

running foot of the heater (including cylinders and water) has a heat storage capacity equal to 8.8277 lbs. of water.

The manipulation and operation of the heaters is as follows: Steam is admitted to the space C the same as to any radiator. The heat of the steam is at once apparent in the car through the wall of the outer cylinder A, while at the same time it commences to heat the water in the inner cylinder. When the steam is on for a sufficient time, the water in the inner cylinder becomes practically as hot as the steam itself, and remains so until the supply of steam is cut off, when it begins to give off its heat by radiation, etc., to the walls of the outer cylinder.

The diagram shows the result in a manner more obvious than I can explain, and therefore I will only point to questions which may appear to require an explanation or to draw deductions.

When steam was first turned on, it was unsatisfactory and low, the pressure being but 10 lbs. at the regulator. It gradually increased however, to 25 lbs. at the regulator and 23 lbs. at the gauge within the car. At 10 A.M. the steam was shut off and remained so for three hours, or until 1 P.M. The whole steam condensed for this time ($4\frac{1}{4}$ hours) was 180 lbs. weight, which was condensed between 8.45 and 10 A.M., or 42.35 lbs per hour.

During the first hour and a quarter the car was warmed from 27° Fahr. to 61° Fahr. After steam was shut off the temperature of the car advanced about one degree. Then its rate of cooling was about two degrees per hour.

At 1 P.M. steam was turned on again and kept on for twenty minutes. The condensation was $67\frac{1}{2}$ lbs., and it restored the heat to the cylinders for 3 hours longer or until 4 P.M., and sent the temperature of the car up to 69° Fahr. This required less than 23 lbs. weight of steam per hour to maintain the heat of the car for the time.

Steam was turned on again at 4 P.M. and kept on for 30 minutes. The condensation was then 90 lbs., and it is reasonable to assume it would have kept the car warm for 4 hours from the time of turning on. This is an average of 33 lbs. of steam per hour for *all day*, or say one *horse power* per car per hour.

During the high temperatures on the outer cylinder, it is very likely the water is not as warm as shown, and during the process of cooling below 212° or (after condensation ceases), the inner cylinder must be the hotter. Of course this is to be expected when steam is on for a short time only.

The total heat of the steam and the total heat of the cylinders, assuming then to be seven-eighths filled with water, in the second stage of the diagram shows this. Take for instance the total heat of the steam for the second stage of the diagram, and it is about 67,500 heat units. Then take the water or its equivalent for 78 feet lineal of cylinders, and it is 688.5 lbs., apparently cooled from 243° Fahr. to 112° Fahr. or 131°, equal to 71,593 heat units (or apparently more than was in the steam). The heat of the iron connections, etc., is not taken into consideration; but if it was, it would only slightly increase the apparent gain and add to the apparent discrepancy.

The difference, however, is not so very great, but I consider it necessary to point it out, to prevent any misconception in the matter.

In the first stages of the diagrams where the steam has been on for an hour or so this is reversed, as should be expected, and somewhat more heat is found in the steam used than in the cylinders.

Take for instance the 180 lbs. of steam condensed in the first stage of diagram. This is equivalent say to 180,000 heat units, as the water of condensation was somewhat cooled before the water in the inside cylinders heated. Then take 688.5 lbs. of water or its equivalent warmed from 27° Fahr. to 260° Fahr., and we have but 170,410 heat units.

The deductions to be drawn from the experiment in general are, (1) that 2 square feet of surface per running foot of car is more than ample for warming, even at this average low temperature. (2) That each square foot of surface requires heat for all purposes equivalent to about 2.5 heat units for each degree the air of the car is colder than the heater.

CCXCVII.

DUTY TRIALS OF PUMPING ENGINES.

BY J. S. COON, BURDETT, NEW YORK.
(Member of the Society.)

To those engineers connected with this Society whose interests, either as manufacturers, experimental experts, or chief engineers at pumping stations are wholly or in part identified with steam pumping machinery, the feature of paramount interest in connection therewith, from an engineering standpoint, is the "duty" which such machinery is capable of developing. In the early experience of the writer, when friends engaged in other and quite different branches of engineering inquired what was meant by the "duty" of a pumping engine, he was quick to explain what it meant; but after having been connected with over a dozen different pumping-engine tests, he is not quite so sure that he knows what he once thought was so obvious.

Like the horse-power of a boiler, until fortunately this Society took the matter in hand and settled it, the word duty as applied to a pumping engine, or at least the work of the pumping engine as applied to the word "duty," admits of vexatious and embarrassing elasticity. Exactly what is meant by the word, as it appears in water-works contracts, is very rarely clearly defined, and there is thus left to the discretion of the expert who makes the contract duty test the acceptance or rejection of certain allowances and other variables which materially affect the final tabulated results. Probably if half a dozen experts, who are familiar with making pumping engine duty tests, were to give independently of each other their views, in detail, of all the precautions, allowances and steps to be observed in making a duty test, no two would agree exactly.

In the matter of starting and stopping a boiler trial, good opinions have differed. The same element enters into a duty test of a pumping engine, together with many other variables of almost equal importance.

Where a so-called high-duty engine is contracted for, the duty

which it shall be capable of developing is specified in the contract, either as so many foot pounds of work for each one hundred (100) pounds of coal supplied to the boilers, without reference to the quantity of water to be evaporated per pound of coal, or in foot pounds of work per one hundred pounds of coal, predicated on an evaporation of ten pounds of water per pound of coal; in other words, so many foot pounds of work per one thousand pounds of water.

In the former instance, where no reference is made to the quantity of water to be evaporated per pound of coal, it is not generally advisable to make a boiler (or evaporation) test in conjunction with the duty test, for almost always the plant is so arranged as to supply the feed water to the boilers at a high temperature; and in breaking the feed-water pipe connections, as is necessary in order to weigh the feed water, the latter enters the boiler at a lower temperature than when working under normal conditions.

It is true that accurate correction can be made for such loss of temperature, and thus make the duty appear what it should be if working under normal conditions. But such corrections and interpolations are mystifying to the average water board, and can easily be avoided by making the evaporation test separate from the duty and capacity test. Since, in general, nothing of vital importance would hinge on the boiler test, it might be of shorter duration than the duty test.

Where the duty is predicated in the contract on a certain rate of evaporation per pound of coal, it has been customarily based on an evaporation of ten pounds of water per pound of coal, without reference to the temperature of the feed water or the steam pressure. When a certain duty is specified, under such stipulations, it is not necessary to weigh the coal or take the steam pressure or the temperature of the feed water. It is only necessary to weigh the feed water, and figure the duty in foot pounds per one thousand pounds of water. The feed water might be ice cold, yet the duty would be figured the same as if the feed water had been 200°.

This is a very slack, inexact method of stipulating the performance of an engine. A duty so stipulated might, indeed, require an engine to be economical in its consumption of steam, but it leaves an opening for the engine builder to ignore utterly a vital feature of a good pumping plant, viz., means for supplying the feed water to the boilers at as high a temperature as possible, and to do so by utilizing heat which would otherwise be lost, which, of course, means a saving in fuel.

If the duty were predicated on an evaporation of ten (10) pounds of water per pound of coal *from and at* 212° *F.* (or better still, per pound of *combustible* from and at 212°), that at once changes the conditions entirely, and compels the contractor to adopt economical means for procuring hot feed water. It also changes the character of the duty test.

It may be urged that in some instances such a stipulation would be unjust to the engine builder, as, for instance, where the contract for boilers was not taken by the engine builder, or where a new engine is supplied to old works having sufficient boiler capacity; but it is equally unfair to compare the performance of different styles of pumping engines on a basis of ten (10) lbs. of water evaporated per pound of coal without taking into account the feed-water temperature and the temperature of evaporation. Thus the injustice is manifest of comparing the duty of two engines, on the ten to one basis, one of which was supplied with feed water at 200° F. and used steam at seventy-five (75) pounds gauge-pressure, and the other with feed water at 90° F. and steam at 125 pounds gauge, without also taking into account the difference in their factors of evaporation, which, in the case cited, amounts to about twelve per cent., which is the difference between a ninety-million duty and one of over one hundred million.

The method of starting and stopping the trial is of prime importance. The engine under trial should be given every possible chance to do its best, without allowing any guesswork to enter into the calculations. Where a running start is made, without drawing the fires and kindling new ones, a wide margin for guesswork is then possible, particularly if the trial be a short one. If the expert has any prejudices for or against the engine which he is testing (and such have been known to exist), that bias must necessarily enter into his judgment as to the condition of the fires at the close of the trial as compared with their condition at the beginning, and there is absolutely no way of making those conditions exactly the same. Whereas, if the test is started with fires newly kindled, no element of uncertainty or guesswork enters into the test, and it gives no opportunity for unfairness or bias.

A method of starting and stopping a trial which appears to be free from objectionable features was, so far as the writer is aware, first used by Mr. R. H. Buel, of New York, in testing the Buffalo, Pawtucket and Lawrence pumping engines in 1879. So far as the boilers are concerned, it is a modification of the standard method

for testing boilers subsequently recommended by the Committee on Boiler Trials, of this Society. It is as follows:

After all preparations for making the test are completed, with the engine running, let the fires burn low, until in consequence the steam pressure falls to a point where it is little more than sufficient to keep the engine running. Then stop the engine and rapidly draw the fires with closed dampers and ash doors, so as to lose as little heat as possible. As quickly as possible kindle new fires with weighed wood (charged to the experiment at 0.4 times its weight in coal equivalent). At the instant the fires are relighted, the test begins at the boilers, time should be taken, the steam pressure recorded, and the height of the water in the boilers carefully noted. When the fires are in sufficiently good condition, and the steam pressure raised to nearly or quite the pressure to be maintained during the trial, the engine is to be then started, the engine counter taken, and the time also, for this is the beginning of the test so far as the engine capacity is concerned. After the last firing, care should be taken to leave as little unconsumed coal as possible, and when the steam pressure has dropped to the point at which it stood when the fires were lighted, time is taken, which is the close of the trial both for engines and boilers, the counter is read, and the fires are immediately and quickly hauled, so that any unconsumed coal may be picked out and deducted from the total charged to the experiment. The water level in the boilers should be as nearly as possible the same as at the beginning of the test, and any difference in level allowed for by calculation, and not by pumping or blowing off.

The reason why it is desirable to start new fires, with as low a steam pressure as will keep the engine in motion, is that it gives a chance completely to consume all the coal supplied to the furnaces. If the trial is started, at the boilers, with a high steam pressure, it must end with a high steam pressure, which means a very hot fire, and it leaves a good deal of unconsumed coal to be picked from the refuse. With a high duty rotative engine, carrying 100 pounds of steam, the start and the finish can be made with a pressure as low as 55 or 60 pounds at the gauge, and thus leave practically no unconsumed coal at the finish.

The above method of starting and stopping can almost always be used where no facilities are provided for a weir measurement of the water delivered by the pumps. Where the delivery of the pumps is to be gauged by a weir notch, it is desirable to make the

start, at the engine, with the engine running. If there is spare boiler power, the engine can be kept in motion by such boilers as are not to be used during the test, while new fires are being kindled under the boilers to be used during the test. The latter should have been under steam at least 24 hours prior to the trial. At the instant the new fires are lighted, the stop valve leading to the spare boilers is closed, and the test begins, both at the boilers and engine, the time and the engine counter being taken. A test started in this way cannot, in general, begin and end with so low a steam pressure as by the first method. The steam pressure in the spare boilers should subsequently be kept slightly below that of the boilers which supply steam to the engine, for obvious reasons.

Where a running start at the engine is imperative, and there is no surplus boiler power, the starting and stopping of the trial at the boilers can be done under the alternate method proposed by the committee of this Society for testing boilers, but in such a contingency the trial should last longer than would be necessary where the start is made with newly-kindled fires, to diminish the percentage of error due to guesswork.

As to the proper length of a duty test, there has been a growing tendency to diminish its length. If the test is made with starting and stopping according to the first method described, it would seem as though 24 hours were sufficient. There is much to recommend a test of that length; for while it is not so long that the expert conducting it may not, without much inconvenience, remain in constant attendance—a matter of some importance—it is not so short as not to give a fair record of the capabilities of the engine and boilers. A test very much shorter than 24 hours is open to objection, for the early part of a test usually shows a greater apparent duty than the latter part. This is especially true if the start is made by cleaning the fires only, and not rekindling them.

Another point upon which there is lack of uniformity in the published reports of duty trials is the basis upon which the duty is figured. This is a matter of great importance, and the report should clearly state whether the duty is based on plunger displacement or actual delivery. There are many situations where it is impracticable to gauge the actual delivery of the pumps by weir measurement, or reservoir capacity, and where it is also impossible to know the loss of action, or "slip" of the pumps. In such cases it has been customary either to guess at the slip, or to figure the duty

on the plunger displacement. Since, therefore, in some instances neither the delivery or slip can be known, in every report a figure should be given for the duty based on the plunger displacement—not the nominal displacement of the contractor, but by measurements made by the expert in charge. The difference between the nominal and the actual plunger displacement has amounted, in the writer's experience, to more than one per cent.; and unless the contract specifies that the duty shall be figured on the water actually delivered by the pumps, the work of the engine should be based on the plunger displacement.

The horse-power figured on the work done in the steam cylinders is the indicated power. The horse-power figured on the plunger displacement is the net power. Both of these can in all cases be determined, and their difference is the friction of the machine. The ratio of the net power to the indicated is the efficiency of the machine, which ought always to be given, but very rarely is, in expert reports. The effective power is figured on the water delivered.

In published reports of duty trials there is also a want of similarity in the allowance for friction in the pump. In many reports an allowance of one pound per square inch, or 2.31 feet, has been added to the total head for friction of the water in passing through the pump valves and chambers. In other tests no allowance has been made for friction, even where the expert was at liberty to use his discretion. In still other contracts it is expressly stipulated that no allowance shall be made for friction. This at once places the published duties of some engines in an unfair position for comparison, for in the case of a pair of engines known to the writer, working under a low head, if an allowance of one pound had been added, the duty would have been increased seven (7) per cent. over the published figure. It is manifestly as fair to allow the pound for friction on a 30-foot head as on one of 130 feet, as the pump friction, at the same speeds, would be practically the same.

The friction of the engine as a whole being also about the same under a low head as under a high one, the percentage which that friction is of the total work done is greater under a low head than under a high one. For this, among other reasons, a high head is favorable for the higher duty. Therefore, since the allowance of one pound for friction tends to offset this advantage, it would seem to be desirable to allow it in figuring the duty for purposes of comparison.

When the contract calls for a specified number of foot pounds of work to be done by the engine for each one hundred pounds of coal supplied to the boilers, with no reference to the quantity of water to be evaporated per pound of coal, that places the plant as a whole on its merits, not merely the engine and boilers, but all the appliances used in connection therewith. Thus it would seem the part of economy to attach the feed pump for supplying the boilers with feed water to the pumping engine, and thus gain the economy due to having the feed pump driven by the large engine instead of having the feed water supplied by a direct acting steam pump, whose duty could not exceed ten million foot pounds per 100 pounds of coal. If this very obvious chance for economy is omitted, it would seem that no allowance should be made for the steam used by the steam pump in pumping the feed water, for it is a part of the plant furnished by the contractor, who has agreed to do a certain amount of work with a certain quantity of coal. Neither should any allowance be made for the work expended in working the feed pump when it is attached to the pumping engine.

The work done in driving the boiler feed pump, when driven by the main pumping engine, amounts, in general, to less than one-fourth of one per cent. of the work done in the main pumps. To run a direct-acting steam pump for feeding the boilers would require, for a 120,000,000 duty engine, two and one-half per cent. of the total coal supplied to the boilers.

The coal and water used per hour per indicated and net horse-power are important and interesting information, and very easily obtained; yet this is rarely to be found in the published reports of expert trials. Almost all the requisite data are necessarily incident to the test. It is only necessary, in addition, to take perhaps a dozen complete sets of indicator cards from the steam cylinders, and note the simultaneous pump resistance. From these data the friction and efficiency of the machine can be obtained, with which, after figuring the net horse-power for the whole trial, on the plunger displacement, the indicated horse-power for the whole trial can be calculated.

To avoid errors in recording the quantity of water supplied to the boilers, the water should be weighed on scales, and not gauged. No attempt should be made either to fill or empty the weighing tank to any particular figure. The attendant is not nearly so apt to omit a record if he fills and empties the tank at random, and records the actual weights thus obtained. A time record should be

kept on the water-log, recording the time when the weighing tank is emptied.

It is desirable to know the quality of the steam, as to moisture. If the steam is superheated, the amount can be determined by an accurate thermometer screwed into the steam pipe, rather than by a calorimeter. In these days of splitting hairs for any advantage which may help to raise the "duty," where the contractor for the engine does not furnish the boilers, and the duty is to be figured per 1,000 pounds of steam, if the steam shows any moisture on the test, the contractor will probably claim *dry* steam. Then, for purposes of comparison, *all* duty tests should be figured on a basis of *dry* steam, and the same remarks apply to the coal. The duty should in all cases be figured on *dry* coal.

The results of the boiler test should be worked out in the form recommended by the committee of this Society.

No attempt is here made to lay down complete directions for conducting a duty test, but rather to call attention to some important features which are often neglected, and to others upon which there is lack of harmony in their treatment. Without doubt many of these unsatisfactory features are due to the fact that duty trials are often conducted by civil engineers connected with water departments, men amply qualified for the legitimate duties of their positions, but whose training and experience do not qualify them for expert work of this character. Such tests—and some are of very recent date—while they may be perfectly sincere, cannot be regarded as authoritative. Where there are so many vexatious omissions in the report, is it unreasonable to suppose there may have been fatal oversights connected with the tests? The competition among engine-builders is so keen, almost bitter, that where an extraordinary duty is claimed, too much candor and detail cannot be shown in the report of the trial. What is wanted is a tabulated statement of the results, plainly given, and an honest, clear description of the methods, precautions and allowances adopted in obtaining them.

DISCUSSION.

Mr. Geo. H. Barrus.—Mr. Coon has brought to the attention of the Society, in this paper, a subject which appears to me of quite as much interest and importance as that which led to the appointment of the Committee on Boiler Trials. The object of my

discussion is to recommend that a similar committee be appointed to determine upon a standard method of conducting duty trials of pumping engines. A wide custom prevails among pumping engine builders of writing into their contracts a guaranty of the duty which will be performed by the engine which they contract to furnish. It is customary for the public water boards with whom the contract is made to have some one, usually the engineer in charge, make a trial to determine whether the contract duty is realized, before the engine is accepted. In rare instances an expert, or a board of experts, is called in to do this work. In the case of the former, the engineer seldom follows any guide but himself. In the case of the latter, he adds to his common sense such information as he or the board may have upon some preceding tests which have been made, which they think are safe precedents to follow, and conducts the test accordingly. In either case, especially where there is dispute as to the proper method employed, no two persons will agree upon the manner in which the test should be conducted and the results worked out. If a code of rules could be adopted in the same manner as the rules adopted by our late Committee on Boiler Tests, it would supply a want which it seems to me is an urgent one. A standard method of conducting duty trials, which could be referred to in cases of dispute, would be of great use, and, it seems to me, would be appreciated not only by engineers and experts who are called in to settle these questions, but by the manufacturers of pumping engines as well.

A case which has come under my personal observation has brought the need of this matter most forcibly to my notice. A contract was made between a pumping engine manufacturer of high standing and a public water board, in which it was agreed to furnish an engine of the highest economy under a guaranty that it should secure a certain stipulated duty. The terms of the guaranty were made in language substantially as follows: "The main engine will be capable of developing a duty equivalent to the lifting of 100,000,000 lbs. one foot high, for each 100 lbs. of coal consumed by the boilers, on a basis of the evaporation of 10 lbs. of water from feed water temperature per pound of coal." The interpretation of this guaranty, as made by the expert who was employed by the water board to test the engine, was that the engine and its equipment should perform the duty named, when a grade of coal was used which will give an evaporation of 10 lbs.

of water per pound of coal into steam of ordinary dryness, no allowance being made for steam used by the feed pump or for slip or for friction of the water passing from the pump well to the water-pressure gauge. This interpretation corresponded to published records showing the manner of conducting trials of many other engines of the same build, and corresponded to the methods advocated by the builders of the engine themselves, as laid down in their printed circulars. The results of the tests were several per cent. below the guaranteed performance. When this had been determined, the builders objected to the manner in which the test was conducted, and to the interpretation of the contract. They claimed, first, that the duty should be figured on the quantity of dry steam furnished the engine, after deducting steam used by the feed pump and the steam jackets. They also claimed that the stipulated evaporation of 10 lbs. of water per pound of coal should be computed from a feed-water temperature corresponding to that in the pond which supplied the boilers. And, finally, they claimed that the engine should be credited with the friction of the water in the pump cylinders; that is, the work done should be computed from the indicator cards taken from the water cylinders. In short, they made every claim which could be thought of which could serve to increase the duty, except an allowance for the steam used by the independent air pump which formed a part of the condensing apparatus. It is needless to consider the justice of the position taken by either the expert or the builders in his particular case. I have cited it in order to draw attention to the fact that disputes are liable to arise in these matters, especially in cases like the one referred to, where the guaranty is in danger of being unfulfilled. In such cases, it would seem that even reputable builders will take advantage of every possible claim as to the manner in which the tests should be figured, so that the results may appear to be in their favor—a course which would not be taken if there were any standard method of conducting trials.

The argument advanced by Mr. Coon, that a comparison of records obtained from different engines makes it important that the same standard of conducting the test should be employed in all cases, is sufficient reason, were there no other, for some society like ours to recommend a code of rules for governing such tests.

Mr. J. T. Hawkins.—Next to the work performed by the committee appointed on boiler tests, I regard this as the most important work that could be entered upon by this Society to-day. I

hope that a committee will be appointed, as moved by Mr. Barrus. I merely rise to second that motion.

Mr. A. F. Nagle.—I have no objection to the appointment of such a committee. I am afraid, however, they have a difficult task before them. Uniformity of methods of conducting duty trials is desirable, and perhaps possible. But primarily a duty trial is for the purpose of making a money settlement between builder and purchaser, and hence it should be free from all reasonable possibility of dispute, and it should be inexpensive.

Let a committee determine, if possible, how that desirable result can be obtained.

On the other hand, there is always a great desire to obtain at a duty trial scientific data for the information of the profession.

How much of such a data should be obtained at a duty trial at the expense of either party may possibly be embodied in a contract, and a committee may be able to recommend a plan which will be practicable. To formulate a plan for duty trials which shall be generally acceptable may well be worth the efforts of a committee.

I have no objections to make, but I fear they have a difficult problem before them.

Mr. W. F. Mattes.—In view of the conditions mentioned by Mr. Nagle, I should think it unwise that a committee be appointed to *establish* rules. I think it would be well for the committee to make some recommendation. I doubt very much the propriety of their going further.

Mr. Hawkins.—I believe it has been thoroughly understood heretofore that the Society will not *establish* any rules of this kind. The investigation of a question of that kind, and a report by a committee, however, carries no little authority with it. It is true that no one need feel obliged to follow the rules. But if the committee was made up of gentlemen prominent in this direction, and *they* establish, so far as their authority can establish, rules or a method or system for conducting these tests, it will carry with it the weight those gentlemen have in their profession, and I think it would be a much better form to put it in, that they should establish rules on their responsibility rather than make a mere report.

Prof. F. R. Hutton.—My interest is known to several of the gentlemen in having this thing brought up and presented to the Society, with the view that the motion of Mr. Barrus should pre-

vail—that this committee should agree upon *recommendations*, with the idea that in any contracts which might be made between the consumer and builder of an engine it should be agreed in the contract, and specified, that the test should be made according to the recommendations of the American Society of Mechanical Engineers. That recommendation is then in black and white, and can be referred to in cases of dispute. We have established the point clearly, I think, that we are not in a position to enforce upon anybody who does not want them the use of any rules which we may recommend; but as a question of establishing something to go by, we can recommend to the profession generally, and adopt in our own practice, and say in the contracts which we may make as individuals, in cases of test, that we will report our test in the method recommended by the committee—the committee being constituted of men whose opinions carry weight—and that we propose to make our report in the form recommended by these gentlemen, than whom there can be no higher authority as to the way in which a pumping-engine test should be conducted.

Mr. Wm. Kent.—I think no member of this Society who was present at the Atlantic City meeting,* and who knows the action then taken, will say that this Society refused to adopt the Boiler Test Committee's report for the reason that it was not worthy of adoption, or any reason of that kind; but it was for the general reason that this Society would at no time adopt any standards on any subject whatever. We took the ground, as Prof. Hutton has stated, that we would not try to impose on the profession anything which they did not want; that the most this Society can do is to appoint committees who will recommend certain methods of testing, and if the Society accepts their report and orders it printed in the Transactions, that is as great an endorsement as this Society can give it, and that is a greater endorsement, I think, than even the adoption of the report by the whole Society, nine-tenths of whose members may not be experts in the particular branch on which the committee reports. The whole question was gone over so thoroughly three or four years ago, that I think it almost useless to bring it up now. But I am entirely in favor of appointing a committee on Mr. Barrus' proposition. I think it is impossible for any committee of this Society to establish rules. The best they can do is to recommend them. No one can be bound by such rules. The best we can do is to have a committee

* Trans., Vol. VI., Appendix VI., page 877.

appointed of say five experts, whose work will be taken as the highest law we can give. I would suggest that, in appointing this committee, the pumping-engine men be represented, as well as experts—that is, builders of pumping engines, and, if possible, some one representing the users of pumping engines. After the Boiler Committee was appointed, there was another committee appointed on standards for threads for tubes, which did the same thing. They reported and recommended certain proportions for screw threads, which, while the Society could not adopt them, have been adopted by the manufacturers, which is the very best kind of compliment to the Society.

Mr. Hawkins.—I imagine that this question is pretty much one of words—the signification of terms. Of course we understand that the committee cannot absolutely establish a system for pumping-engine tests any more than the Society could; but they could do a little more than merely report upon it. I think they could put it in a little more authoritative shape than merely making a report to this Society. The Society can accept a report, but will not adopt it. Now, if we alter the phraseology, so far as the committee is concerned, so that instead of establishing the report, they adopt it, it will be a little better, I think, than merely making a report. Here will be a number of gentlemen identified with this specific branch of mechanics; they make an investigation and make a report. There seems to be nothing definite done about it. If we could say that these gentlemen adopted the method or system, I think it would be putting it in more authoritative form, that is all.

[NOTE.—The question of appointing such a committee was put by the President at the close of this debate, and it was unanimously carried. That committee as appointed consisted of Messrs. Coon, Barrus, Reynolds, Nagle and DeKinder.* —F. R. H.]

* See page 356, Vol. IX. Trans. A. S. M. E.

CCXCVIII.

THE EFFECT OF CIRCULATION IN STEAM BOILERS ON THE QUALITY OF THE STEAM.

BY GEO. H. BARRUS, BOSTON, MASS.

(Member of the Society.)

A SOMEWHAT remarkable experience upon this subject has occurred to the writer, in connection with a small boiler used for steam heating.

The boiler was of the vertical type, and consisted of a plain shell (without a water leg) 40 inches in diameter and 48 inches in height. It contained 115 2-inch tubes 4 ft. long. The boiler was supported by a brick furnace which contained a grate 33 inches square, placed 24 inches below the lower head. A smoke bonnet having a side flue 12 inches in diameter carried the gases to a brick chimney close by, which was 40 ft. high. The side flue contained a damper operated by an automatic regulator set to close at a pressure of about 5 lbs. The shell was covered with fossil meal.

The heating apparatus was constructed on the low-pressure gravity-return system. The total extent of radiating surface amounted to 900 sq. ft. The lowest point of this surface was at a horizontal distance of 100 feet from the boiler, measured along the return pipe, and this point was 18 inches above the usual water line. The highest point was 14 ft. above the water line. The main steam supply pipe was a $2\frac{1}{2}$-inch pipe, and led off from the upper head of the boiler, near one side. The main return pipe was a $1\frac{1}{2}$-inch pipe, and at a point beyond the check valve it was increased to 2 inches, and entered the boiler through the bottom head. A short piece of the pipe and the elbow were exposed to the heat of the furnace.

The upper ends of the tubes in the boiler were provided with cast-iron ferrules, the openings through which were $\frac{3}{4}$ of an inch in diameter.

Soon after the plant was set to work, difficulties began to be encountered in the working of the boiler and in the return of the water from some of the pipes. The action was as follows: The

boiler was filled to such a point that with a low fire the level of water stood at the top of the glass. As soon as the fire was started up and steam began to form, the water gradually fell, and by the time the whole plant was in normal action the water had reached the bottom of the glass, which was some 12 inches below. At the same time there was ample evidence of priming. The sound of rushing water in the main pipe could be distinctly heard, even when the observer stood at some distance away. When the automatic damper closed and a momentary check occurred in the production of steam, the water came back at a rapid rate and soon nearly filled the glass. The radiators and circulations farthest from the boiler and lowest in elevation filled with water, and much trouble arose in freeing them of water and making them heat in a proper manner.

The priming of the boiler was at first attributed to the presence of oil in the water, but repeated blowing off and change of the water produced no beneficial result, as it should have done if this was the cause of the trouble. Two additional steam pipes were attached, each a 2-inch pipe, and applied to opposite sides of the boiler as high in the shell as possible. Each of these pipes entered as far as the tubes, and the upper part was cut away so as to draw steam from above, rather than from below. These were both connected to the main supply pipe. In spite of the distribution in the points of discharge which these pipes effected, there was no abatement of the principal difficulty.

Under advice from the chief inspector of a prominent boiler insurance company, the boiler was treated to a large dose of soda. This penetrated the whole system of pipes and radiators, and worked for several days. The boiler was then blown off and refilled with fresh water. Even this treatment failed to give relief, although the priming appeared to be less serious while the alkaline liquor was in use.

Finally a suspicion arose in the writer's mind that the priming was due to improper circulation of the water. The tubes in this boiler were laid out inside a circle 32 inches in diameter. There was a clear space outside the tubes all around 4 inche sin width. It was thought that an increase of this space might improve the circulation, and the plugging up of the outer row of tubes was contemplated. Before trying this plan, however, it was thought that the same result might be effected by removing some of the ferrules in the center of the boiler so as to centralize the heating effect of

the fire, thereby increasing the rapidity of ebullition at the centre, while reducing it at the circumference. Examination revealed the fact that these ferrules, as before stated, had openings $\frac{3}{4}$ of an inch in diameter, these openings aggregating in area 0.35 sq. ft., which bears a ratio to the grate surface (7.56 sq. ft.) of 1 to 21.6. This ratio, it must be admitted, is too small for efficient boiler work. Accordingly, 36 ferrules near the centre were removed, and the collective area of the openings increased to 0.84 sq. ft., which bears a ratio to the grate surface of 1 to 9.

The effect of this change was instant. The priming disappeared at once. The water line became nearly constant, the extreme variation being reduced to 2 inches. There was no further difficulty with draining and heating of the pipes and radiators, and the whole apparatus worked from this time on with perfect satisfaction.

It appears that the small area for draught that existed when the ferrules were all in use caused such a distribution of the heat through the whole number of tubes, that an active ebullition occurred through the whole body of water. This interfered with the natural circulation of the water, and the result was a commotion which caused the water to boil over into the steam pipe. The concentration of the heat at the centre of the boiler, in the manner pointed out, changed this disordered condition, produced an easy circulation, and furnished a complete remedy for the whole trouble.

The amount of water-heating surface in the boiler is approximately 120 sq. ft., and this suffices to warm the building in a proper manner in the coldest weather. The building is a church, having wooden walls, and it contains some 160,000 cubic feet of space.

DISCUSSION.

Prof. J. M. Whitham.—A case somewhat similar to that described in the paper by Mr. Barrus occurred in connection with the boilers of the U. S. S. Galena about six years ago. This vessel has ten horizontal cylindrical boilers, 8 feet in diameter and 8 feet 2 inches long. The total grate surface is 250 square feet; total heating surface, 5,462.6 square feet; calorimeter through the tubes, 7.83. There are five boilers on each side of the ship (with a longitudinal fire-room), and all connect with one smoke-stack resting over the fire-room opposite the centre boilers. During the trial trip, and for some two years afterward, whenever the centre boiler on either side was used, the priming became so excessive that it was neces-

32

sary to haul fires and shut off that boiler. There were two furnaces to each boiler, having a nest of tubes over each. There was a clear space of perhaps six inches between the nest of tubes and the cylindrical shell. Chief-Engineer H. B. Nones, U. S. N., remedied the priming in these boilers by having two vertical rows of tubes in the centre of each nest removed, and putting tie rods in their places. The effect was that the steam, as it formed rapidly on the crown sheet, had an unobstructed passage to the steam space, while the circulation around the sides of the tube nests was accelerated. The priming occurred only in the central boilers under the smoke-stack, on account of the draught of those boilers.

Mr. A. F. Nagle.—The boiler appears to have been an exceedingly low one—as I understand, only 48 inches high. I should like to know if Mr. Barrus thinks a similar occurrence would have happened if the boiler had been, say, 8 feet high—just double the height—if the trouble was not in a measure due to the exceedingly low height of the boiler?

Mr. Barrus.—I do not know whether it would or not.

Mr. Nagle.—It is not an unusual type of boiler. All Corliss boilers, and other boilers of that type, are of greater height, and no such difficulties occur. Hence I should attribute this trouble to the exceedingly low height, although Mr. Barrus' experience and remedy for the difficulty is instructive.

Mr. J. S. Coon.—The Amoskeag Company at Manchester, N. H., have a large number of such boilers in use; but of course the ratio of the length to the diameter is very much greater than the case cited by Mr. Barrus: probably the ratio would be three and a half to four, or at least three, and the arrangement of the tubes is not exceptional. They are nearer together at the bottom, at the fire-box, and spread at the top. At the top head they are quite close to the shell, and so far as I know they never had any difficulty in priming. I think Mr. Nagle's suggestion that the priming is due to the exceptional shortness of the boiler is correct.

Prof. H. B. Gale.—A word occurs to me now which I would like to say. It has seemed to me that perhaps the employment of the term "steam boiler" is rather a misuse of words, at least in some cases. Boiling means ebullition, and I have sometimes thought that in some boilers with good circulation, when working easily, there is often probably no ebullition at all, but simply a circula-

tion of the water, the heated water rising to the surface and evaporating there, instead of forming bubbles and rising up through. Perhaps the phrase "steam generator" would better describe the action in some vessels of that kind than the word boiler. I would like to hear an expression of opinion from others on that point.

Mr. H. P. Minot.—I would like to ask the last speaker why the ebullition in this boiler might not take place.

Mr. Gale.—I did not intend to say that there was no ebullition in this boiler.

Mr. Minot.—In any boiler?

Mr. Gale.—When there is a good circulation, it seems to me that the water might be heated as it rises up around the tubes; but that it would not be heated enough if it was moving rapidly to cause it to evaporate until the pressure was reduced, when it reached the surface. Of course, as the water rises the pressure gets less and less by the amount of head of water that is over it, and the evaporation will not take place until the pressure is reduced to the pressure of saturation. If no steam is being drawn from the boiler, there is no evaporation, and boiling is of course out of the question; now it seems to me that when the draft of steam is rather light, and the circulation rapid, the water would probably rise to the top before it is heated hot enough actually to form bubbles of steam, and that, instead of actual boiling, we would have simply a rapid circulation of water, and surface evaporation. A little air mixed with the steam in a boiler would also, by increasing the pressure, tend to prevent ebullition.

Mr. H. H. Suplee.—In relation to this matter, there were some experiments brought to my notice several years ago made on a plain cylinder boiler without any internal flues of any sort—a long boiler—in which to determine the water level and the character of the boiling. A number of water-gauge glasses were placed along the whole side of the boiler—I think some eight or ten in number—and a level placed along to determine if there was any difference in the front and back of the boiler, and it was found that the water was some three or four inches higher over the fire, and that it gradually diminished as it went back, showing that there was an ebullition that lifted the water in the front part, making a circulation. This showed that there must have been actual ebullition going on there in order to lift the water so much higher in front of the boiler, and that this was really the cause of the circulation, and

that the boiling must occur in order to produce the circulation, as the one is the cause and the other the effect.

Mr. Barrus.—I should like to say a word about this boiler in connection with what Mr. Nagle states, and that is, that there are a great many small vertical boilers in use for portable engines which have tubes no more than four feet long, and it seems to me that this difficulty could not have been due to the short lengths of the tubes. In a fire engine boiler, which is certainly no larger than this one, there is no trouble of that kind. At least, it would be out of the question to have any trouble of that kind, on account of the injury which it would bring to the engine. I think if the difficulty had been due to the length of the tubes, it would not have appeared when the boiler was running slowly. But this trouble was one which was constantly occurring, even when there was a very slow fire.

Mr. Nagle.—I am very glad to hear of the success of the remedy.

Mr. Minot.—Isn't it a fact that there are any number of steam fire engines running in this country with tubes as short as 11 inches or 15 inches? I think Mr. Barrus has given us the right solution of this matter.

Mr. Barrus.—It seems to me that this subject of the effect of circulation of water in boilers is an important one, and one that needs some ventilation. I had hoped that members would give some of their experiences.

Mr. Huston.—The difference between the front and the back of the boiler—could not that occur simply from the circulation itself, simply from the direction of the currents, right over the hottest part of the fire, constantly lifting the water at that end? I should think that would be the case somewhat.

Mr. Suplee.—I think that is exactly what the experiments did indicate, namely, that the circulation did lift the water, but that indirectly the rising of the bubbles was the cause of the lifting and also the cause of the circulation. I simply mention it in regard to what Mr. Gale said, that a boiler with proper circulation should not also possess ebullition. I think they should both occur together, and that this case went to prove that point.

Mr. Gale.—It seems to me that it is probable that we have all degrees between no ebullition and a very rapid ebullition in different boilers, according to the amount of circulation that we have, and the way the boilers are worked; and that it is not absolutely necessary to suppose ebullition in order to make a difference in

level, because a simple circulation of water does make a difference in level, though whether it would make as much difference as is referred to in this case I do not know. It seems to me that we might have in some cases no ebullition at all, and from that up, in all grades, to a very violent case, such as is described in this paper.

CCXCIX.

A FOUNDRY CUPOLA EXPERIENCE.

BY FRED'K A. SCHEFFLER, ERIE, PA.
(Member of the Society.)

RECENTLY one of the large manufacturing firms in the western part of Pennsylvania outgrew the capacity "output" of their foundry cupola—an occurrence which has probably taken place with other similar old-established firms who are on a good commercial basis.

The capacity of the old cupola was forced to 34,000 pounds per day, and it required the blower to be in operation from 3 P.M. until the final charge was withdrawn, which occurred about 5.30 P.M. The cupola was then so hot that in the morning it was quite unfit for the cupola tender to make ready for the daily "heat," although the work *had* to be done, but it was undoubtedly not very invigorating to the tender.

The size of the cupola was 60 inches outside diameter, and 44 inches inside, the fire-brick lining being 8 inches. The distance from the bottom or outlet spout to the charging door was between 13 and 14 feet. There were two rows or sets of tuyeres, one above the other, being virtually a well-known Detroit cupola. The greatest amount of blast obtainable was 9 ounces, and the blast gate was wide open almost continually throughout the heat, notwithstanding the fact that the blower (Sturtevant's make, No. 8) was speeded 100 revolutions faster than the list called for to produce a 12-ounce blast. The difficulty was of course shouldered upon the blower and the catalogue list of the makers, but, as will be shown later, the entire fault lay with the tuyeres and blast-box on the cupola. The fan was situated 90 feet from the cupola, and was connected by an 18-inch galvanized iron blast pipe, which was straight with the exception of the elbow where it left the fan, and the Y-shaped connection into the cupola, the two arms of the Y forming at the same time vertical elbows each 10 inches in diameter. Thus the connections between the fan and cupola were

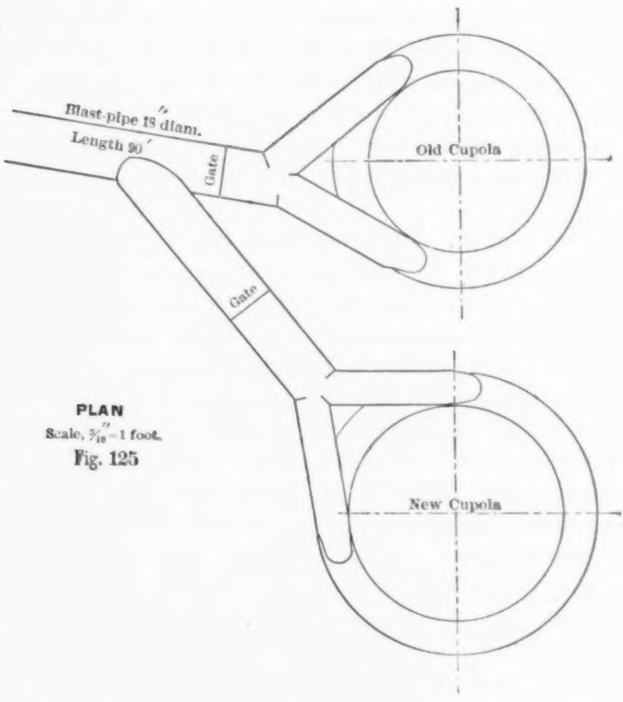
such as to produce the best possible results, especially as the outlet from the fan was only 13 inches.

The capacity of the new cupola was desired to be 10 or 12 tons per hour, which would increase the output about 20% in the same length of time, and shorten the time required for a "heat" of the ordinary size. The dimensions and style agreed upon called for a 72″ shell, which when lined up with 8″ of fire brick would make 56″ inside diameter. The height was 15 feet from spout to charging door, and this space would contain 10 tons, not including fuel. The tuyeres were eight in number and all located on the same horizontal plane, being in shape the form of an equilateral triangle, each side being six inches long. They were placed at a height of 24 inches from the base plate, and were surrounded by a blast box 12 inches square. The new cupola was to be erected in close proximity to the old one, and it was desired to utilize the same connecting blast pipe for both cupolas. The question of size of blower required to give the proper blast for the new cupola was a serious one, for it was fully believed that the one already in use could not possibly supply the amount required, when it was so evident that it was doing all that could possibly be obtained from it.

Bids were therefore asked for a fan which would be *guaranteed* to melt 12 tons of iron per hour in a cupola 56 inches inside diameter. These were the only data given upon which the bidders were to base their estimates. The bids were all about the same in regard to size of fan required to produce the work and necessary (12 ounces) blast. One well-known firm advised a fan so close in size to the one already in use, that the surprise was *very* great, as both fans were similar in construction. So positive was the bidder that the fan mentioned would do the work that it was not deemed advisable to purchase a fan of the *same* size as the old one, for it was considered that two fans of the same size and general construction ought to do the same work under the same conditions. Hence the suggestion arose that there must be something radically wrong with either the fan or both the blast box around the old cupola and the tuyeres in the same, and it was determined to discover, if possible, where this defect lay, by simply connecting the 18-inch blast pipe into the new cupola. So vastly different was the construction of the new cupola tuyeres and blast box and that of the old one, that if there was anything wrong with the latter it would be shown by the difference in the working qualities of the same and the new cupola.

The branches of the Y connections were 12″ diameter. The general arrangement of cupolas and connections is shown in the engraving (Fig. 125). It will be noticed that either cupola can be run independently of the other, and all connections are so arranged that it was not necessary to shut down a day to make the *new* connection.

The first heat taken from the new cupola was very successful.



PLAN
Scale, 3/16″=1 foot.
Fig. 125

The succeeding ones were still better, and not only were they "run off" quicker and *better* than any of those taken from the old one, but the blast was so great that the water in the gauge, which was only intended to register up to 10 ounces of blast, was blown out of the top of the same, and when the new one was made, the pressure was found to be between 11 and 12 ounces. No change of speed of blower had been made, and the same main blast pipe was utilized. The "heats" were taken off with the blast gate only one-quarter open, whereas with the o'd cupola this gate was required to be wide open almost continually throughout the heat.

Great satisfaction was the result, as the price of the new blower had been saved as well as the expense which would necessarily have been involved in putting down a larger main blast pipe and a possible loss of two days' castings.

As will be noticed, the trouble was with the blast box on the old cupola, and the tuyeres in the same, and not with the fan. The tuyeres were five in number in each row (there being two, an upper and lower row) and were too small in size to admit the amount of blast which the fan was capable of developing, consequently the latter was "choked." It is simply impossible to get the full benefit from a fan unless it has practically no outward friction to the passage of the greatest blast which it is capable of producing, or at least unless this friction has been largely reduced. The facts or lessons which are presented by the above experience are as follows:

That by using a small amount of practical common sense, a fan which had been pretty well "rattled" was made to bring out that which it had long been striving to do by making the final exit passages (tuyeres) of a combined area greater than the area of the pipe at the outlet of the fan, and by *reducing* the blast box to a size which would be no larger than actually required, this being in cross section about 10% more than the area of the fan outlet. When forcing air into the new cupola under the above circumstances, the fan does not require as much power to drive it, and in the same time more iron can be melted with it than it was possible to melt in the old cupola. The latter melted at the rate of 8 tons per hour, the new one at the rate of 10 tons.

These results may seem surprising, but they are positive facts, and are given to the readers for their own consideration, and possibly old cupolas in daily use might be altered by a trifling expense and made to produce better and more economical work.

DISCUSSION.

Mr. H. I. Snell.—I am interested in this paper because the results are obtained from an experience and are not theories. It gives some facts, some figures, and we can make our own calculations and deductions. It also shows a reason for one of the seeming discrepancies between a manufacturer's catalogue and actual experience in practical work, and might serve as an addenda to a paper I read before the Society at the Philadelphia meeting, upon "Experiments and Experiences with Blowers."

I regret the author of the paper is not present, that he might answer some questions I would like to ask, which might be of advantage to the members in discussing the subject.

I would like to know how his cupola was charged; the proportion of fuel to iron melted; the amount of fuel for the first bed, and then the amount of iron, amount of fuel, and again, the iron, etc. What was the distance of the tuyeres from the bottom of bed? I look upon the amount of iron melted in the 44-inch cupola as being extraordinary, and do not recall ever having known so much per hour to be melted with any blower, under any pressure. The kind of castings furnished will determine the amount melted to a certain degree; but I think 9,000 or 10,000 lbs. per hour good practice with a cupola 44 inches inside diameter. Did we know the amount of fuel used, it would furnish data for the solution of the interesting problem of how many cubic feet of air that No. 8 Sturtevant Blower was discharging per minute.

The chief interest in the paper outside of the extraordinary work done by the blower is in the fact that we have a blower that under certain circumstances fails to give the pressure the manufacturer says it should, and yet, under the same circumstances, so far as the blower and its attachments are concerned, it does support the maker's statement when applied to a larger cupola. This is a point I can hardly reconcile with my own experience, and I feel interested in learning all the details of the experiments that I may harmonize the quoted experiences with my own.

Will Mr. Scheffler tell us how the blast was measured, and at what point between the blower and cupola; and at what period of the heat—the beginning, middle, or end? From tests I have made upon cupola blowers, I have found that, unless the blower is very large for the work, the pressure at first will be smaller, increase as the heat goes on and the iron and coals settle down, and becoming larger about the middle of the heat, then often reducing towards the end when the furnace is exhausted and the resistance to the escape of the air diminished.

The card or diagram (Fig. 309) representing a heat in Smith's Foundry, Pittsburgh, Pa., where the blast was furnished by a No. 8 Sturtevant fan running about 2,300 revolutions per minute, will illustrate the point that the same fan running the same speed will give different pressures at different points in the heat.

I suppose, as the pipe was not changed when the cupolas were, the pressure gauge was applied at the same place on the pipe when

each cupola was tested; but sometimes the application of the water column is different in different tests, and then different readings of the gauge obtained when the actual pressure is the same, for instance, if the gauge in one case is applied in such a way that the line of the flow of air will be directly into the tube, the pressure indicated will be somewhat greater than if the flow was across the tube or if the tube was turned from the current. These are interesting points in relation to this paper, and I hope Mr. Scheffler will be able to give us some information upon the points raised.

I have not been able to reconcile some of the results he has ob-

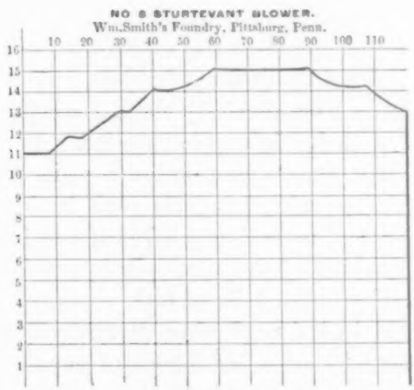


Fig. 309.

The horizontal lines represent the pressures in ounces per square inch.

The vertical lines represent the time in minutes from the beginning of the blast.

tained, for I have been taught by my own experiments that a centrifugal fan, at a certain speed or number of revolutions, will generate a pressure or centrifugal force due that speed, and I also have found that the speed of the outer circumference of the fan-wheel will be about the speed or velocity at which the air will escape into the atmosphere through any size discharge less in area than one-third the diameter of the wheel multiplied by the width of the wheel at its outer edge.

I have tried the experiment so many times, I feel justified in making the assertion that the pressure obtained by a fan blower will not change practically whether the discharge pipe is entirely closed or discharging through any size opening within the area of the "capacity of the fan," or one-third the diameter multiplied by

the face of the wheel. The volume discharged will vary, the power required will vary, but the pressure per square inch remains practically constant, and for this reason I fail to understand why Mr. Scheffler obtained only 9 ounces pressure in the small cupola with small tuyerage, and obtained 10 or 11 ounces in the large cupola with increased size of tuyeres, unless the charging of the cupolas was different, having in the first case greater spaces between the particles or pieces of coal than in the last. I believe in large tuyerage for cupolas, as I think the area of the interstices between the coal is the real measure of the tuyerage and large conduits or pipes to this point desirable if made tight.

I am pleased to know of this experiment of Mr. Scheffler, as he has demonstrated that the fault was not in the fan but in its application, and while I fail to reason as he does from his experiments, which I do not doubt were carefully made, I accept it as a welcome addition to our knowledge in cupola practice. As a counterpart to his experience, I will state a case where a plain round cupola 44 inches inside diameter, with 5 tuyeres each 3×5 inches, 8 inches above the sand bottom, melted only 7,764 lbs. of iron per hour, the blower giving a pressure of 8 ounces per square inch, and only 63 lbs. of iron was melted with 1 lb. of fuel.

I think, as I said in the beginning, Mr. Scheffler found extraordinary results before he made his change of cupolas, and I trust in his closing remarks he will be able to give us further particulars of this interesting and useful experiment.

Mr. J. F. Wilcox.—I might say in this connection that during the last ten years in steel works, where the cupola goes on Monday morning and runs just as long as the lining will last, day and night, the practice has been constantly toward increasing the size of the tuyeres. The one requisite kept in view is to reduce the consumption of coke without reference to blowers or any other consideration. The amount of coke that is used in a well-proportioned cupola to-day—I am not speaking about foundry practice but steel works practice—is taken at about eight and a half to nine tons of melted iron to the ton of coke used. A steel works manager watches that figure right along, week in and week out, but the one thing kept in view is the amount of coke used. We do not care anything about the pressure, except as it affects the coke consumption. The pressure will go up and down, according as the cupola is working.

Mr. J. L. Gobeille.—Some of us younger men find that a cupola

is a pretty big thing to tackle. Because of a fire in a certain foundry it was found necessary to change the plan a little. There is an alley on the premises, and as I wanted to put in an overhead traveling crane, the cupola being on this side of the alley and the blower on the other side, I thought I would put a glazed sewer pipe there for the blast instead of the ordinary metal pipe, as it must go underground. If any one has had any experience under such conditions, I would be pleased to hear how they overcame the difficulty. I would be pleased to hear from any one who has used similar appliances for blast pipes.

Prof. Sweet.—We have been using sewer pipe for years. It is made of hydraulic cement. It is 16 inches in diameter.

Mr. Wilcox.—I would like to ask the question of any member of the Society if they have any knowledge or have ever seen any use made of the gases from the cupola. From the very nature of the melting there must always be an excess of carbonic oxide passing up above the charging door. And it has often occurred to me, particularly at night-time, seeing that great flame coming out of the top of the stack, to ask why in the world no use is made of it. I do not know of any use, except seeing in some of the earlier Holley reports where he has a warm air apparatus up above the charging door—a sort of hot blast where he brings his heated air down afterwards into his tuyeres. But with three and four cupolas running steadily until the lining goes, I do not see why it is impracticable to treat it the same as we do in the blast furnace—taking the carbonic oxide right from the top of the cupola down through a flue, closing the cupola entirely if necessary on top, but taking that waste gas which is rich in carbonic oxide—far richer than the gas that comes from the blast furnace; and with two or three cupolas working all the time so that, when the blast is off one, the effect is not felt, you may always have surplus heat enough around your cupolas to run a respectable sized battery of boilers. I thought of trying it sometime this summer,—putting in a water-tube boiler and placing it right above the charging door of the cupola itself, carrying the gases up.

Mr. W. F. Mattes.—I have been familiar with the Holley reports to which Mr. Wilcox refers. I recollect rather indistinctly the drawings which he presented of steel works cupolas in England, showing a hot blast arrangement above the charging doors. As I recollect it, however, he did not consider the arrangement advantageous, after considerable practical experience.

In that connection attention might be called to the tendency on the part of a hot blast to produce an incomplete combustion of the fuel; in other words, burning solid carbon to carbonic oxide rather than to carbonic acid, which is not exactly the action you want in a cupola. It is the action you want in a blast furnace. Then again a heated blast in a cupola would entail the application of jackets, etc., to preserve the lining. I think the other line which Mr. Wilcox has indicated, that of raising steam from the waste heat of the cupola, offers more promise of advantage, and it is one upon which I have had occasion to make plans which have never yet been experimented with. But I hope that somebody, before I get around to that point where I shall have to experiment with it, will report something in that line.

Mr. Wilcox.—That is the only direction, to my mind, where the gas could be utilized. It has no value as a reducing agent in the furnace, and it is of very little value in a hot blast stove. The quantity is not sufficient, and the temperature to which your blast would be brought up would be too insignificant to think of achieving any economies in that direction.

CCC.

THE BEST FORM OF NOZZLES AND DIVERGING TUBES.

BY A. F. NAGLE, CHICAGO, ILL.
(Member of the Society.)

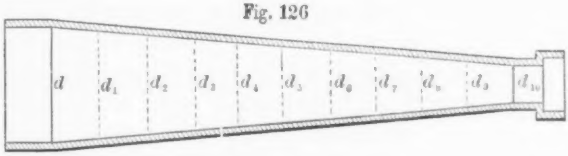
THE subject of the form of nozzles will be discussed first, and it will then probably appear that whatever theory should govern their form will apply equally as well to diverging tubes. The purpose of all nozzles is to change the velocity of a moving fluid from a comparatively low one to a higher, or, in other words, to accelerate its velocity. It is the belief of the writer that in all such changes of velocity the accelerating force should be uniform throughout the length of the tube. The first application of the accelerating force, as well as its cessation, might be gradual; but for the main portion of the tube, should it not be uniform? It would seem to be true that whatever fluid friction necessarily occurs in making changes of velocity would be a minimum if the applied force were uniform.

In studying any prescribed form of nozzle we would naturally turn to the line of acceleration from which to judge of its fitness or merit. If we take the ordinary straight-taper nozzle, and construct therefrom a line showing the acceleration at all points of its length, we should be much surprised by its extreme variation. It is well known that such nozzles do not give good practical results. The stream of water is not solid when it leaves the nozzle, and it is soon broken into a spray. Fig. 126 shows a straight-taper nozzle, line 1 of Fig. 129 the velocity at all points, and line 1 of Fig. 130 the accelerations. Here it is clearly seen that at the very instant of leaving the nozzle the fluid is subjected to its greatest acceleration, and hence it is not surprising that it should be broken and scattered, instead of moving like a homogeneous and solid body.

An improvement upon the straight taper form was first suggested, tried and adopted by Capt. Eyre S. Shaw, Chief of the London Fire Department, in 1869. Capt. Shaw proposed that the velocities within the nozzle be increased in a uniform manner, that is,

the velocities exhibited in line 1, Fig. 129, instead of curving as there shown, should be a straight taper as shown in line 2. From this line of velocities the diameters of Fig. 127 are obtained. The

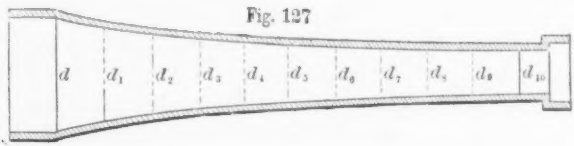
Fig. 126



great practical superiority of this form of nozzle was fully demonstrated by Capt. Shaw, and if we construct the line of acceleration shown in line 2, Fig. 130, from line 2 in Fig. 129, we shall not fail to see why better results followed.

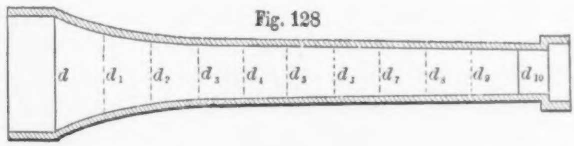
If it was a great improvement to change the line of acceleration from that shown in line 1 to line 2, Fig. 130, it would seem that still better results should be obtained by making the line uniform

Fig. 127



throughout, as shown in line 3. A gentle approach and descent might be made, as shown in the dotted lines. From line 3, Fig. 130, line 3, Fig. 129, is obtained, and from the latter the diameters of the nozzle shown in Fig. 128. It is this form which the writer would recommend for all forms of nozzles. It is to be regretted that practical tests cannot be adduced to verify these theoretical deductions, but intelligent firemen have said that Fig. 128 is a form

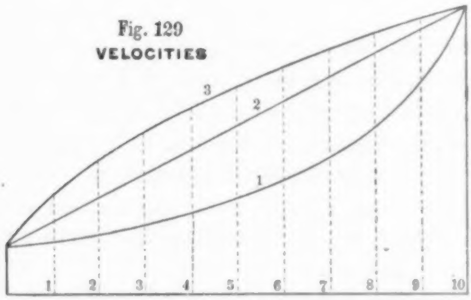
Fig. 128



which they believe would be very efficient. Should any member have an opportunity to test said form, it is to be hoped that he will communicate the results to this Society.

For the purpose of illustrating the necessary calculations in the three forms of nozzles shown, a nozzle has been assumed 10 inches in length, 2½ inches in diameter at the large end, and 1 inch

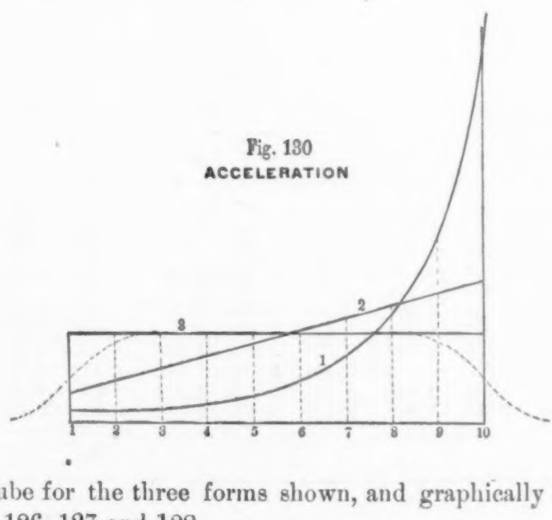
Fig. 129
VELOCITIES



at the small end, and the velocity taken at the beginning of the nozzle at one foot per second. Figs. 126, 127, 128, 129 and 130 are drawn to one-quarter full size.

Table 1 gives the diameters at 10 equal divisions of the length

Fig. 130
ACCELERATION



of the tube for the three forms shown, and graphically exhibited in Figs. 126, 127 and 128.

Table 2 gives the velocities at corresponding points, graphically exhibited in Fig. 129, lines 1, 2 and 3.

The necessary calculations for obtaining the velocities and diameters are too simple to need explanation. The formula for obtain-

ing the acceleration necessary to effect the change of velocity within a given space is,

$$= \frac{v_2^2 - v_1^2}{2s} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (A)$$

where v_1 is the velocity at the beginning, and v_2 at the end of space s. From the known values of v, as given in columns 1 and 2, Table II., substituted in formula (A), the values of p are found as given in columns 1 and 2, Table III. Column 3 is, of course, a constant upon the theory already suggested.

By proper substitutions in formula (A) column 3 in Table II. is obtained, and from these values the diameters are readily obtained as given in Table I.

Formula (A) can be expressed directly in terms of d, for

$$v_1 : v_2 :: d_2^2 : d_1^2 \text{; or, } v_1^2 : v_2^2 :: d_2^4 : d_1^4.$$

Taking $v_1 = 1$, and making proper substitutions, we obtain

$$2ps = \frac{d_1^4}{d_2^4} - 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (B)$$

	Table I.—Diameters.			Fig. 129. Table II.—Velocities.			Fig. 130. Table III.—Accelerations		
	Fig. 126.	Fig. 127.	Fig. 128.	Line 1.	Line 2.	Line 3.	Line 1.	Line 2.	Line 3.
d	2.50	2.50	2.50	1.00	1.00	1.00	0	0	0
d_1	2.35	2.02	1.688	1.132	1.535	2.192	.140	.663	1.903
d_2	2.20	1.756	1.464	1.292	2.050	2.933	.193	.938	1.903
d_3	2.05	1.558	1.332	1.487	2.575	3.533	.272	1.211	1.903
d_4	1.90	1.420	1.246	1.731	3.100	4.028	.392	1.492	1.903
d_5	1.75	1.313	1.182	2.041	3.625	4.475	.584	1.760	1.903
d_6	1.60	1.237	1.132	2.441	4.150	4.882	.892	2.040	1.903
d_7	1.45	1.156	1.090	2.973	4.675	5.258	1.438	2.360	1.903
d_8	1.30	1.090	1.056	3.699	5.200	5 608	2.422	2.600	1.903
d_9	1.15	1.045	1.026	4.726	5.725	5.938	4.330	2.860	1.903
d_{10}	1.00	1.000	1.000	6.250	6.250	6.250	8.36	3.150	1.903
							1.903	1.903	1.903

Table 3 gives the mean acceleration for each tenth division, graphically exhibited in Fig. 130, lines 1, 2 and 3.

Formula (B) may be considered a fundamental formula applicable to all sizes of nozzles constructed upon the theory that the acceleration should be uniform throughout the length of the tube.

To find the value of p for any proposed nozzle, the extreme diameters d_1 and d_2 and length s being known, we need but make proper substitutions in formula (B).

When p is known, any intermediate diameter d_2 at any distance s from d_1 is of course readily found.

Let us now pass to the application of this same theory to diverging tubes. Until the invention of injectors these tubes had scarcely any practical usefulness, but they are now used so extensively for so great a variety of purposes that a careful consideration of this subject will be of importance to all having occasion to use them.

It is not known what first suggested the use of these diverging tubes. Early experiments, however, were confined to small sizes and low heads. There does not appear to have been any reasonable explanation of their philosophy. Bernouille suggested that the velocity of the water at its *final discharge* should be that due to the head. As he gave no limitation to the size of the two ends of the tubes, we can readily see how quickly absurd results would be found under such a theory if we assume extremely large or small dimensions. The simple truth is now understood to be that if the fluid passing the small end of the diverging tube with a high velocity can be changed to a lower velocity without waste of energy, it must have an excess of energy above that due to its lower velocity, for the residing energy, or *vis viva*, of a moving body is proportional to the *square* of its velocity; that is, if its velocity be reduced one-half, it has but one-quarter the *vis viva* it had at the greater velocity, and hence this excess exerts a pulling influence—a negative pressure, or a suction, upon the particles behind it. This action again accelerates the fluid in the throat of the tube, and only finds equilibrium between the friction head of the water (both surface and fluid) plus the head due to its final velocity, and a perfect vacuum plus the original head. In other words, theoretically, that is, if no fluid or surface friction existed and a perfect shaped tube were used, a perfect vacuum would be obtained in the throat of a diverging tube with any appreciable head of water, however small.

One of the conclusions reached by early experimenters was that the angle of divergence should be small, generally limiting it to between 4 and 7 degrees, but in no case is it found that they made it dependent upon the size of the tube. We can readily see that a small tube having a divergence of 5 degrees would change its

velocity at a very different rate from one with the same divergence and ten times the diameter. These facts led to the conclusion that the straight taper tube of any fixed angle of divergence was not really a theoretically correct form, but rather that it should be constructed upon the theory already applied to nozzles. A careful analysis was therefore made of two sets of experiments at command, those of Mr. Brownlee as given in D. K. Clark's *Manual for Mechanical Engineers*, page 931, and Mr. James B. Francis as given in his *Lowell Hydraulic Experiments.*

The tube used by Mr. Brownlee is reproduced as nearly as possible to one-half size in Fig. 131, and the one used by Mr. Francis to one-sixth full size in Fig. 132. There is also shown within the same drawings a form of tube such as would be obtained by formula (B) upon the theory of a uniform acceleration for nozzles.

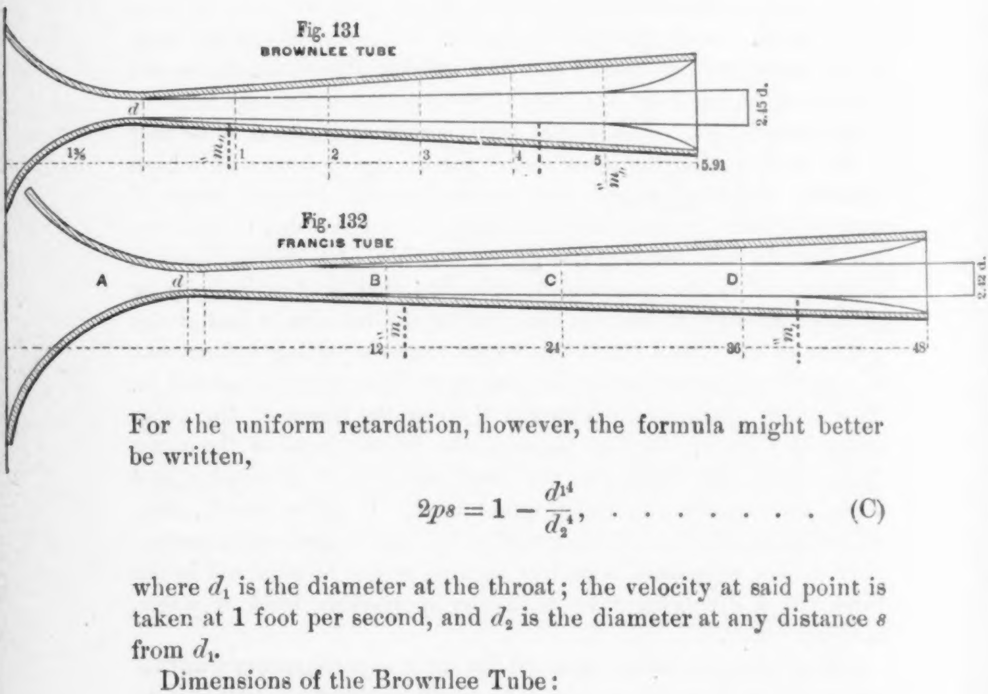


Fig. 131 BROWNLEE TUBE

Fig. 132 FRANCIS TUBE

For the uniform retardation, however, the formula might better be written,

$$2ps = 1 - \frac{d_1^4}{d_2^4}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (C)$$

where d_1 is the diameter at the throat; the velocity at said point is taken at 1 foot per second, and d_2 is the diameter at any distance s from d_1.

Dimensions of the Brownlee Tube:

Angle of divergence, 7° 5′

Length, 5.95″.

$p = .083866$ by formula (C).

$p = .42$ by formula (B).

				By Formula (B) or (C).	
Diameter at throat		.1982	inch	.1982	inch
"	" 1 inch from throat	.3214	"	.2075	"
"	" 2 inches from throat	.4446	"	.2196	"
"	" 3 " " "	.5678	"	.2361	"
"	" 4 " " "	.6910	"	.2617	"
"	" 5 " " "	.8142	"	.3127	"
"	" 5.95 " " "	.9375	"	.9375	"

Dimensions of the Francis Tube.
Angle of divergence 5° 1′.
Radius uniting straight and taper portion of tube = 22.69 feet.
Length, 48 inches.
$p = .010324$ by formula (C).
$p = 2.69$ by formula (B).

					By Formula (B) or (C).	
Diameter at throat	.1018	foot or	1.2216	inch	1.2216	inch
" at end B	.1454	"	1.7448	"	1.2967	"
" " C	.2337	"	2.8068	"	1.4496	"
" " D	.3209	"	3.8508	"	1.7163	"
" " E	.4085	"	4.9020	"	4.9020	"

It should be noticed that while the angle of divergence in these tubes did not differ greatly, their diameters differed as much as 6 to 1, and the retardation about 8 to 1, or nearly inversely as the product of the sines of the angles of divergence and diameters of the tubes. This would indicate that if a straight taper were used, and no other guidance, it should be one whose angle of divergence depended upon the diameter of the tube.

Mr. Brownlee's experiments gave the following data :

TABLE 4.

1	2	3	4	5	6	7
Head in Feet.	Vacuum at Throat.	Sum of Head and Vacuum.	Velocity Due to Head.	Vel. Due to Total Head.	Actual Velocity.	Ratio of Column 6 to 4.
.25	.52	.77	4.01	7.04	6.66	1.66
.50	1.30	1.80	5.67	10.76	10.23	1.50
.75	2.4	3.15	6.95	14.24	13.6	1.96
1.	3.5	4.5	8.02	17.02	16.34	2.04
2.	8.2	10.2	11.35	25.63	27.74	2.18
3.	14.	17.	13.90	33.09	31.95	2.30
4.	19.8	23.8	16.05	78.84	37.90	2.36
5.	26.0	31.	17.94	44.69	43.45	2.42
6.	31.1	37.1	19.66	48.88	48.14	2.45

Mr. Francis's experiments with *D* tube gave the following data :

TABLE 5.

1	2	3	4	5	6
Head in Feet.	Vacuum at Throat.	Sum of Head and Vacuum.	Velocity Due to Head.	Actual Velocity.	Ratio of Column 5 to 4.
.181	.65	.831	3.41	7.30	2.14
.274	1.02	1.294	4.17	9.12	2.19
.410	1.76	2.20	5.32	11.97	2.25
.630	2.67	3.30	6.37	14.65	2.30
.920	4.28	5.20	7.70	18.26	2.37
1.180	5.82	7.00	8.70	21.15	2.43
1.36	6.64	8.00	9.36	22.71	2.42

There is so little difference between the values of columns 5 and 6, Table 4, that for the purpose of this paper it may be assumed that the velocity in the throat is that due to the sum of the original head plus the vacuum, and hence column 2, Table 5, is obtained from column 5. It will be observed that the Francis tube produces a much greater vacuum with the same head than the Brownlee tube, and this is as it should be, when we see how much more gentle the change of velocity is, and how much nearer it conforms to the theoretical form which I am endeavoring to establish. Imperfect as the Brownlee tube was, yet with only 6 feet head it produced a vacuum of 31.1 feet. It would have been very instructive if Mr. Francis had extended his experiments to somewhat greater heads, and ascertained with what head a perfect vacuum could have been obtained. Evidently with less than 6 feet.

The greatest efficiency of the Brownlee tube was 2.45% ; hence the diameter corresponding to this increased flow, if moving with the velocity due to the original or outside head, would be $.1982 \times \sqrt{2.45} = .312''$.

With the Francis tube this diameter would be $1.2216 \times \sqrt{2.42} = 1.900$ inches.

These diameters are indicated by the letter "m" on the drawings, both for the straight-taper tubes and the proposed form.

It is noticeable how near to the throat in straight-taper tubes this diameter is found, while in the proposed form it is not found until near the very end, thus tending to hold the stream solid and homogeneous.

That the straight-taper tube does not hold the water solidly is testified to by Mr. Francis, page 215: "There is reason to think

that the flow through a diverging tube is, to a certain extent, in a condition of unstable equilibrium. In Venturi's experiments, the water discharging into the air from diverging tubes was observed to have great irregularity of motion, even eddies within the tube; whence the jet comes forth by leaps, and with irregular scatterings. These irregularities are undoubtedly due, in part at least, to unstable equilibrium, and there must be a corresponding irregularity in the exhausting power of the diverging tube, which would be indicated, in our experiments, by oscillations in the elevation of the surface of the water in compartment E, which would rise and fall as the exhausting power of the tube was less or greater."

It remains to be proven by future experiments whether the proposed form is really an improvement over the straight taper.

Formula (C) can be reduced to a still simpler form. The ratio of $\frac{d_1^4}{d_2^4}$ should really be very small, so small as to be practically equal to zero; and hence (C) becomes $2ps = 1$. S should, in fact, be as long as is practicable, but in this equation it may be taken at unity. Then p becomes equal to .5. Let us also assume the diameter at the throat of the tube equal to unity, then formula (C) $2ps = 1 - \frac{d_1^4}{d_2}$ becomes $2 \times .5 \times s = .1 - \frac{1}{d_2^4}$ or,

$$d_2 = \sqrt[4]{\frac{1}{1-s}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (D)$$

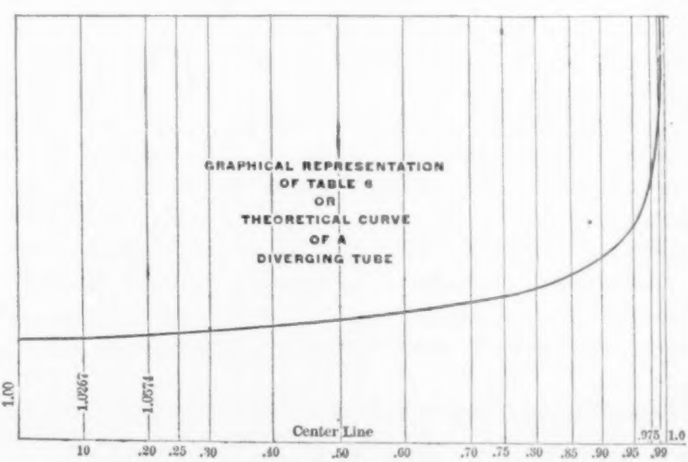


FIG. 133.

Formula (D) will give the diameter d_2 at any distance s from the throat.

Table 6 gives these values for the principal divisions of a tube, and Fig. 133 shows graphically the curve which the tabular values for a diverging tube would impose, and we must observe how far this form departs from the straight line usually adopted for such tubes.

TABLE 6.

Distance from Throat.	Diameters.
.00	1.00
.10	1.0267
.20	1.0574
.25	1.0746
.30	1.0933
.40	1.1362
.50	1.1892
.60	1.2574
.70	1.3512
.75	1.4142
.80	1.4954
.85	1.6068
.90	1.7783
.95	2.1147
.975	2.5150
.99	3.1623
1.00	Infinite

CCCI.

STEAM EXCAVATORS.

BY WILLIAM L. CLEMENTS, BAY CITY, MICH.

(Member of the Society.)

THE term steam excavator, steam navvy, or steam shovel is applied to that division of excavating machinery in general which has for its object the removal of material in localities inaccessible to water-excavating machines or dredges, and therefore requiring and possessing special features which adapt it to this service. Of necessity it is portable, and possesses much lighter machinery than used with dredges, and is adapted to work with material impracticable to remove under the surface of the water.

The appliance here considered is the portable land machine, or steam excavator, with the machinery always mounted upon a base or car body, and supported by wheels for propulsion.

As to the designs of construction of steam excavators, there are as many as the manufacturers who have them among their specialties. Several makers have old, well-tried, and successful machines, and each of them has distinctive designs in many parts, which are owned and have been developed by their makers, and which have been the result of their experience. Very often these differences of designs and developments of different manufacturers have made the production of one maker preferable to all others for a class of work or for shipment to different localities. One maker, for transportation either by boat or railway, manufactures a machine easily taken apart and which may be put together and perform its work at some point otherwise inaccessible. A machine of this type, the first offered for service, is of American design, and for work in a country difficult of access has no competitor.

The steam excavator in the United States is confined almost exclusively to railway use, either in the construction of new lines or in the maintenance of way, and is a distinctive railway tool. For

easy transportation on its own wheels over any line, for work on its extensions or maintenance, and for work wherever a track may penetrate, another design more suitable than the first for such service is manufactured by other makers. With this machine transportation to inaccessible points is difficult, and its portability and shipment to its destination are dependent upon the existence of railroads. The gauge of the wheels is standard, and the running gearing supporting the machine is not to be removed, nor the machinery taken apart for transportation. Primarily these are the two types of steam excavators. The operations of each, and their motions for removing earth are the same.

In the construction of this class of machinery, iron or steel is generally used in all principal parts. The body of the car, upon which is mounted the boiler, engine, and other machinery, is sometimes constructed of wood, generally oak, but such construction in this important part cannot be recommended if long and continued service is to be expected from it. It soon fails to endure the work required of it in climates of extreme dryness or dampness, and when thus subjected, the alignment of the machinery, as well as the security of its parts, is lost. In well-constructed machines the car bodies are made up of I-beams or channels securely riveted and bound together, supplied with wrought-iron end sills, and with an iron frame throughout. It is evident that only to an iron body should the machinery be secured necessary to receive, as it does in some work, the most severe shocks and strains.

Upon the car body is mounted the machinery for hoisting the dipper, consisting generally of a small vertical or horizontal double cylinder engine, the boiler with its water tank, the mechanism for slewing the crane, and the chain spools, clutches, etc., necessary to operate the dipper and the crane.

The crane is usually of simple construction, of iron, or in some machines wood, and is fastened to a mast or post so as to allow it to swing on either side of the car at right angles to it. The mast or post is firmly guyed and secured to the car body, and the substantial security of this post is necessary for good work with a machine. The dipper is of different shapes and sizes, depending much upon the material it has to work in and the capacity of the machine. Its capacity is usually from 1^1 to 2 cubic yards.

The operation of excavating requires the dipper to be thrust into the bank or cut, and at the same time the dipper is raised from a lower to a higher position. These movements require on the crane

an appliance to thrust the dipper into the bank in addition to hoisting, and there are many methods of accomplishing this successfully; one, by means of an endless chain attachment from the engine on the car body, giving motion to a pinion through a clutch, and this pinion to a rack attachment, and that in turn to the handle of the dipper; another arrangement used is a pair of engines applied directly to the rack of the dipper handle; and yet another, of using steam direct in a cylinder, the extension or contraction of the piston rod of the cylinder extending or contracting the dipper.

The mechanisms for slewing are often used with a small double engine actuating a drum, winding a chain attached to a large sheave on the crane of the machine. This arrangement provides for perfect independence of the motions, and the crane may be slewed when all other motions have ceased, which is often a necessity. Such an arrangement was probably used on the first steam excavator built, and was successful but for the large number of wearing parts and its slow motion. Steam cylinders, with pistons attached directly or indirectly to the large sheave of the crane, are more successful, as the loss by friction and wearing is much less, and the motion of slewing much quicker.

One of the most important parts of machinery of this class is the chain drum actuating the dipper, and accompanying clutches for releasing it or putting it in motion. Since through this drum and its clutches the dipper is urged through the material and its contents hoisted and the dipper again released, its very frequent use, together with the shocks in hard digging, necessitate the most careful construction and design. There are many forms of drums and clutches in use, but few withstand for any length of time the severe strains to which they are subjected. Some are friction, others positive clutches. The friction clutch for this use has many advantages to recommend it; its use gives less severe shocks to all parts of the crane and car, and the rapidity with which it may be thrown in or out of clutch is most advantageous. But experience has shown that in this class of work and for this use the perfect friction clutch is yet to be found. They are found to wear rapidly, requiring constant attention and repair, and the expense attending them is sometimes excessive. The positive clutch was probably the first in use, and is still thought by many to be the most effective for its purpose, and to require the least amount of repair and expense.

The question of chains for use in dredge or excavator work is also a most important one. Obviously it is desirable to use for this work chains of as small diameter as possible, and with the introduction and manufacture of steel chains, with their great working strength, their use became extensive. Experience has shown from trials of American steel chains that they are not adapted for the service. In general, the steel used for chain work possesses a low tensile strength, generally not exceeding 55,000, a necessity for the perfect and easy welding of the links. With this low grade of steel it is found to wear excessively, the most carefully made and tested steel chains enduring service but for a comparatively short time, and while their adaptability and use for other purposes is unquestioned, they are found to endure service a shorter time than iron chains of similar strength. Steel chains of sufficient strength and extreme hardness have been used, but with their use, with the severe shocks to which they are subjected, they soon acquire great brittleness and require frequent annealing. This defect may be explained by the fact that the chain is frequently strained beyond its elastic limit. With the chains at present available, the tested refined iron crane chain of sufficient strength is much cheaper and more durable. Their action in this service is peculiar, requiring for the most severe work a chain whose working strength is double that which is ordinarily brought upon it, and the larger the diameter of the iron, with the increased wearing surface of the link, the more durable.

The cranes, receiving the shocks and strains incident to digging, were in the earlier machines constructed of wood. They have not the durability, are more cumbersome, and with modern designs are not used, but a secure and firmly braced and tied framework of channel and tee iron receives and transmits the strains.

The design of the dipper is also of importance, its shape, size and strength depending upon the material in which it is to be worked. A dipper suitable for clay with its tenacity is totally unfit for use in hard or cemented digging. Its shape for soft and tenacious material necessitates the mouth or opening to be of smaller size than the drop or bottom, preventing in a measure the adhesion of the material to its sides. For hard digging in cemented gravel or boulders, no such provision is necessary, and the strength of the dipper itself, together with the teeth then used for breaking or cutting the material, is of first importance. But even with the best of construc-

tion, their severe use would prevent any extended life, and repairs are frequently necessary.

With all the machinery necessary for the operation of a steam excavator, the quantity of steam used is considerable, and therefore the design of a boiler suitable for this service has been a work of importance to manufacturers. For an effective and compact portable boiler, the locomotive design, with its horizontal flues and internal fire box, is not excelled, but such a design, of suitable steam-generating capacity, occupies too large a floor space on the car, and prevents any compact and convenient arrangement of the machinery. In general it may be said that with portable excavating machinery the upright boiler is used. This type of boiler has but few recommendations for use. It is far from economical in its working, and it is difficult to construct so as always to be tight.

It may be said that the question is never raised as to how economical in fuel are the boilers for these machines. There is no opportunity, working where they do, and under very unfavorable conditions, to develop the problem of saving in oil, waste, or supplies, where the order from headquarters is to work the machine constantly and keep no trains waiting. The boiler is used to the limit of its capacity, fuel is consumed with a forced draft, and with no storage for steam; as much fuel is used in one day as would in the same period with careful working, operate a boiler with proper settings of three times the capacity.

In the economical working of fuel and under unfavorable conditions, with forced draft, etc., for portable boilers, the locomotive type is ahead of the vertical design. Experiments under different conditions and circumstances with the vertical type show that, with care as good as may be expected, a safe assumption for boilers of this class is that the amount of water evaporated per pound of coal is about eight, and the amount of bituminous coal consumed per hour, per square foot of grate area, with forced draft is about sixty, or in other words the average evaporative effect of a square foot of heating surface is about eighteen pounds of water per hour.

These results, however, compare very favorably with some from locomotive practice, and to accomplish them requires a boiler built in a most thorough manner and with several peculiarities in its construction. For the essential of safety, these boilers should never be made of other than the best flange steel or iron, and accidents have often occurred through the use of the cheaper and more de-

fective material. We are warranted in saying that only with the grossest negligence can accidents occur with the use of flange or fire-box steel, and such treatment these boilers may often receive. In consequence of the necessary hard work to be done, the ordinary types as generally used endure the service but a short period. With strong firing the upper portion of the vertical flues, left unprotected by water, soon becomes injured, and then the connections with the upper head will leak. To remedy this, all boilers of the vertical type should have submerged flues, with the accompanying smoke chamber, thus shortening the flues and carrying them below the water level. With this necessary provision, and the flues jointed to the heads with copper ferrules, these boilers may be said to be generally successful for this service.

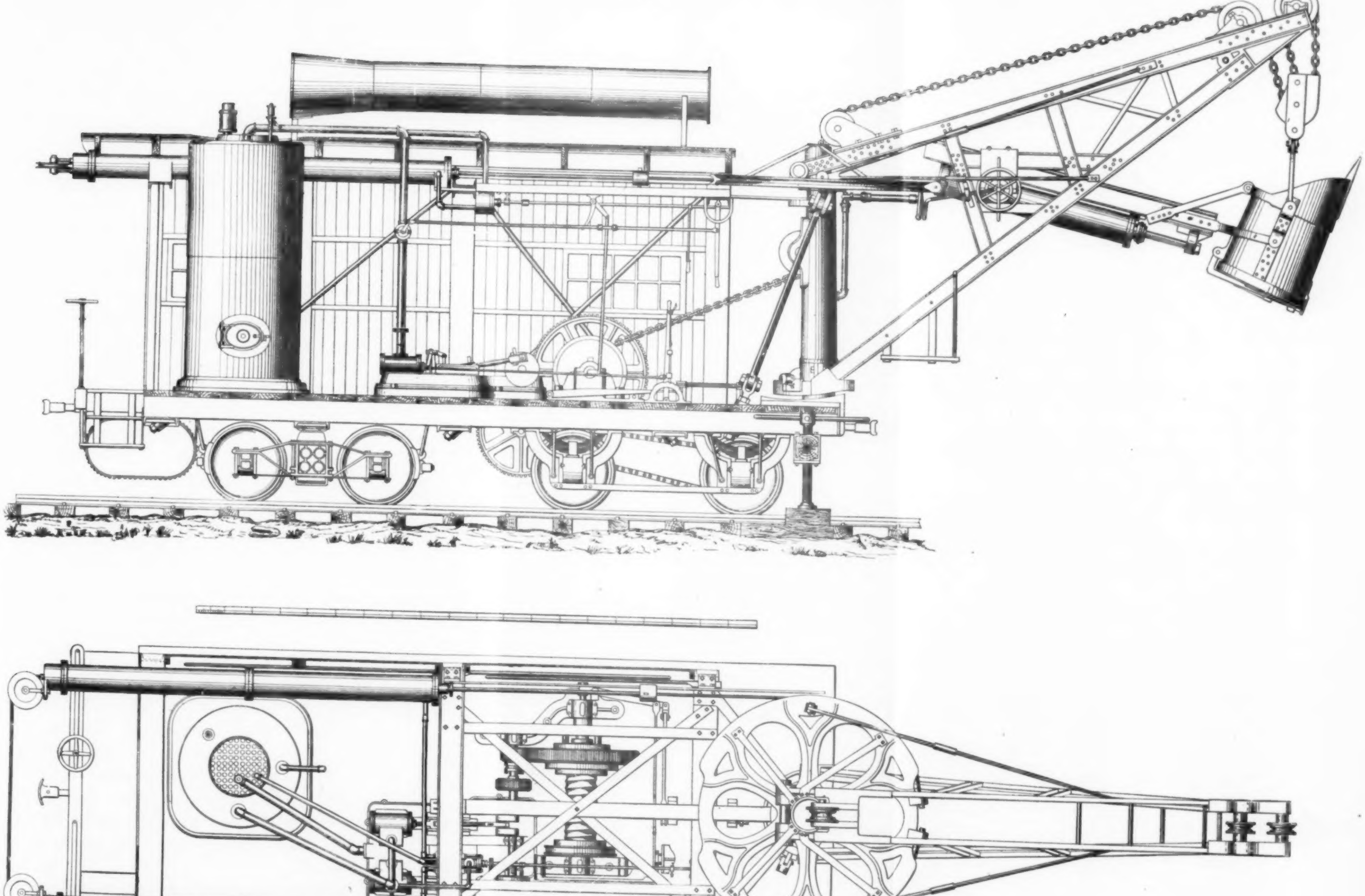
The peculiarities of design developed from experience and long trial in the use of steam excavating machinery, have made the productions of the older manufacturers preferred and more extensively used than machines of more recent date. They are successful from practical test and development in many kinds of work, and the theoretically correct steam shovel is more likely to be useless than most other forms of machinery. The details as given have been confined to no particular machine, but apply to portable steam excavating machinery in general. To be more specific, it is the purpose to discuss the production of a successful maker who has had many years of experience, and whose design is eminently successful, being in constant use on the largest and best railroads in the country.

This excavator, shown in Fig. 135, performs all its motions by steam direct without intermediate gearing, except that for hoisting the dipper and propelling itself.

In the crane is mounted a cylinder directly connected with the dipper through its piston rod and the dipper handle, which takes steam on either side of a piston through a trunnion bearing. The dipper can thus be thrust in or out at the will of the attendant in charge.

The operation of digging is a combination of a thrust to the dipper by the piston rod of the steam cylinder, and the raising of the dipper by the hoisting engine. While the attendant, operating the swinging and engine levers, regulates the angle and height of the dipper, the dipper cylinder varies the radius, and places the mouth at a proper angle in the cutting, thrusting the dipper into it while the engine is running.

Fig. 133.



The crane slews through an angle of 200 degrees, and this movement is effected through two wire ropes, one on either side, attached to the crane sheave, which revolves about the crane post. These wire ropes are also directly connected with the piston rods of two steam cylinders in the top of the car. Steam is given to the front of the right-hand piston if slewing to the right is to be accomplished; to the left-hand, if slewing is to the left. The exhausts from these cylinders are so arranged for economy in steam that the exhaustion of one cylinder is partially the supply of the other. To render the motion uniform, the backs of these pistons are connected by a wire cable, which always keeps the sheave ropes taut.

Provision is made through a pinion on the engine shaft and a gear working into it under the car, for self-propulsion. This enables the excavator to make its way through a cutting without the assistance of a locomotive.

The stability of the excavator in operation is increased by an iron transom beam provided with screw jacks, which levels the car and renders its working easier.

Safety attachments, allowing the crane to be slewed and the dipper to be raised but to fixed points, and an automatic attachment for unclutching after the material is delivered in the car, greatly assist the operator, and are essential for the preservation of every machine of this class.

Many of the mechanisms for the performance of the different functions of this steam excavator belong to the manufacturers of this machine, and it has been developed by a series of trials and tests extending over a period of nearly twelve years. Several features are novel and interesting. The idea of slewing the crane and dipper by steam applied directly to a piston actuating the sheave attachment to the crane, was conceived, perfected, and the motion controlled, only through many trials and tests, and the excessive wear of gearing and chains, together with a slow and unreliable slewing motion, led first to a necessary change in the mechanism, and the development of the steam slewing. As before stated, chains or cables attached to the drum and actuated by clutches are successful in many cases where properly designed.

The arrangement for thrusting the dipper out of or into the cutting is simple and effective. Fig. 136 represents the dipper cylinder with its piston and piston rod, the rod of which is attached to the

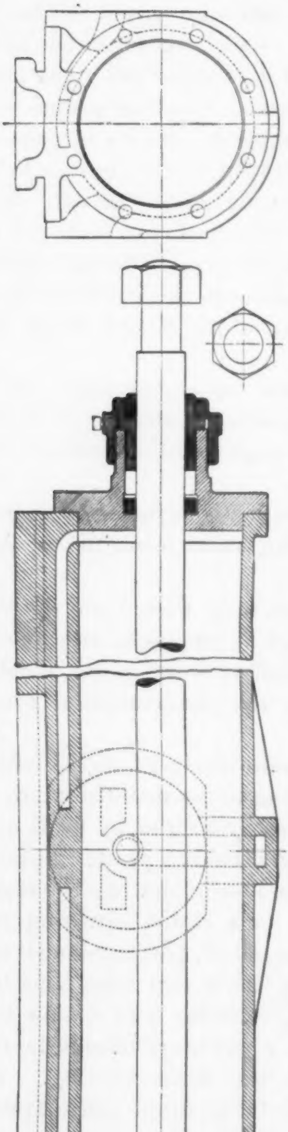
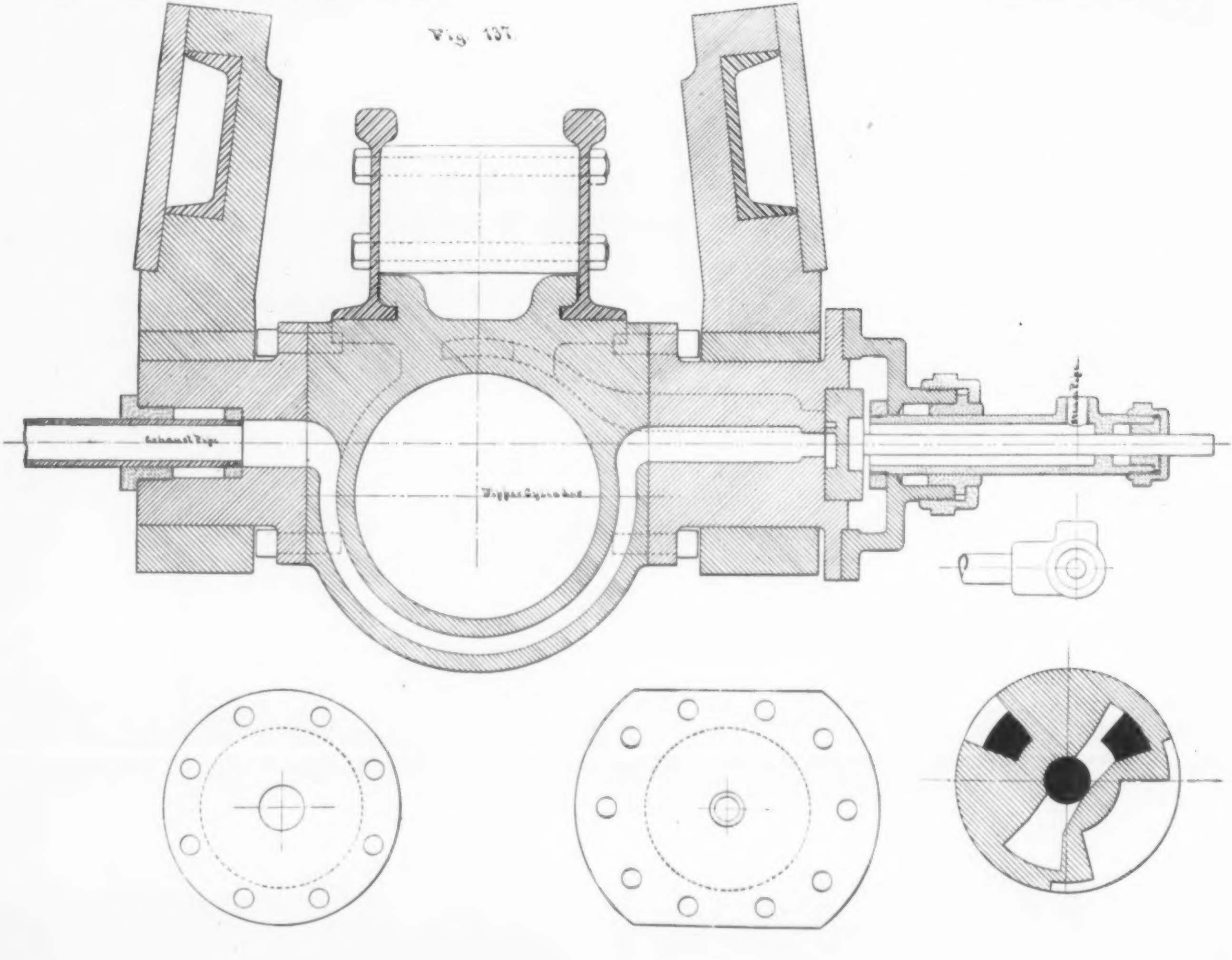


Fig. 135.

dipper handle, which in turn is bolted to the side arms of the dipper, as shown in Fig. 135. The control of the motions of the dipper is through the dipper cylinder valve, as shown in Fig. 137. Steam is admitted through the trunnion bearing on one side, and the exhaust takes place from the other. The steam ports are arranged very similar to those of an engine cylinder with a sliding valve, except that the valves are arranged about the centre, and a movement of the hand wheel in any direction opens the valve and admits steam to either side of the piston, while the opposite port is open to the exhaust. Such an arrangement gives a strong and steady motion to the dipper, is very quick and easily controlled, and is a very simple and effective appliance. In these parts, as before stated, gearing, chainwork, or engine power direct are successfully used and do excellent service, but the simplicity of a device for the attainment of a motion is of greatest importance, operated as it is by all grades of engineers and under unfavorable conditions.

The hoisting engine, consisting of a small engine with double cylinders, geared from the engine shaft to a spool shaft on which is placed the chain drum for hoisting the dipper, and the clutch actuating it, deserves mention. The engine has two cylinders, 8″ × 10″, is supplied with a link motion for use in the self-propulsion of the car, and is made to operate at a speed of 350 revolutions per minute.

Fig. 137



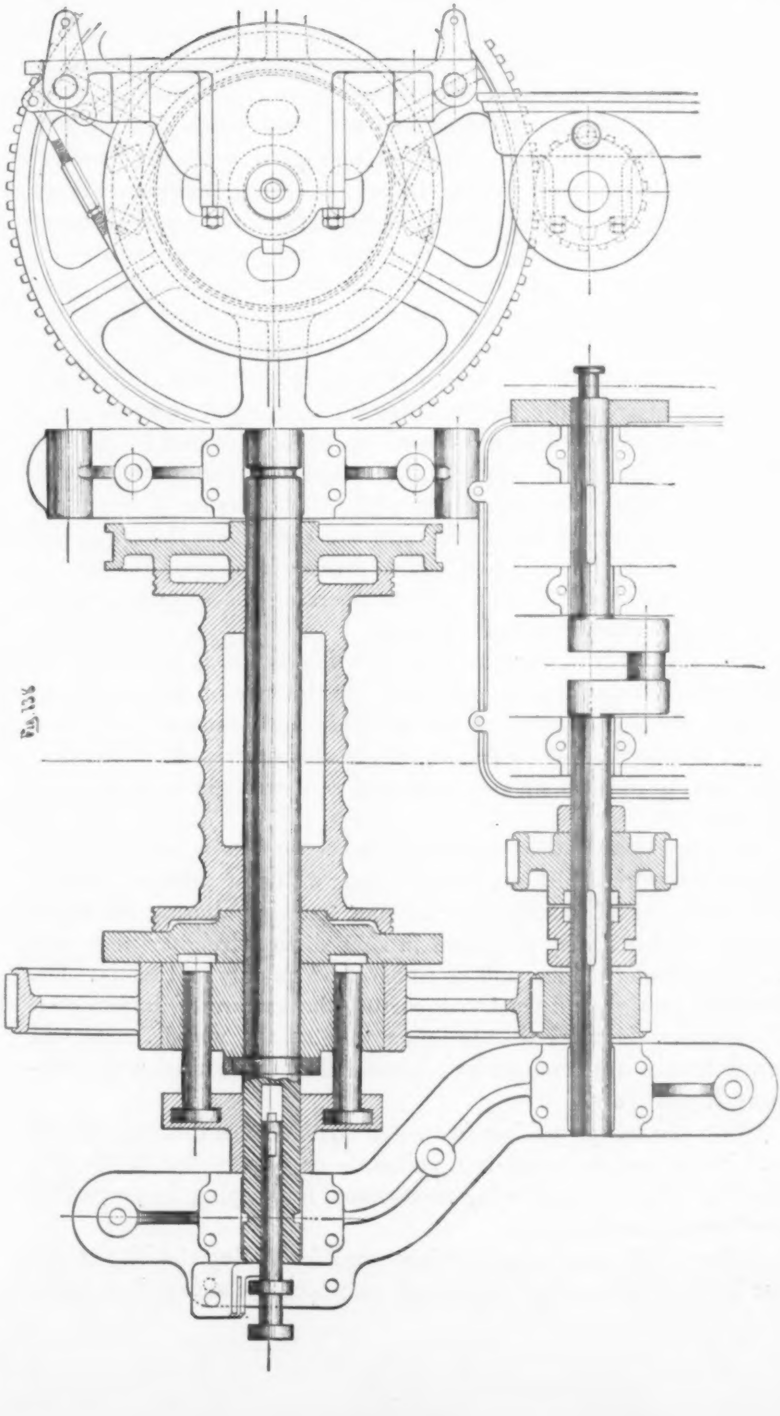


Fig. 138

The clutch is a positive one; and when it is remembered that it is put in and out of gear in many hundred operations each day, its construction is important. Fig. 138 represents the drum and clutch. The clutch is loose on the shaft and has a heavy side disk or plate into which are fitted about the centre several hard steel pockets which receive the clutch pins. These steel clutch pins pass through the enlarged hub of the large gear wheel actuated by the engine pinion. This gear is keyed to the shaft and upon the pins being thrust through the hub clutch into the steel pockets of the drum disk. The shaft is hollow and receives a pin which operates a loose disk to which the steel pins are fastened, and with the connection of the shaft pin the operator may easily raise the dipper, or by disengaging the clutch lower it; a too rapid downward motion being controlled by a friction stop brake, operated by the foot of the attendant. The rapidity with which an operator may clutch the chain drum, raise the dipper, while the operator of the crane cylinder is thrusting the dipper into the bank, slew the crane and dump the material is dependent to great extent upon the experience of the operator, with a good operator the average number of dumps being about one a minute.

The best form of upright boiler for this service is illustrated by Fig. 134. It conforms in general to the description already given.

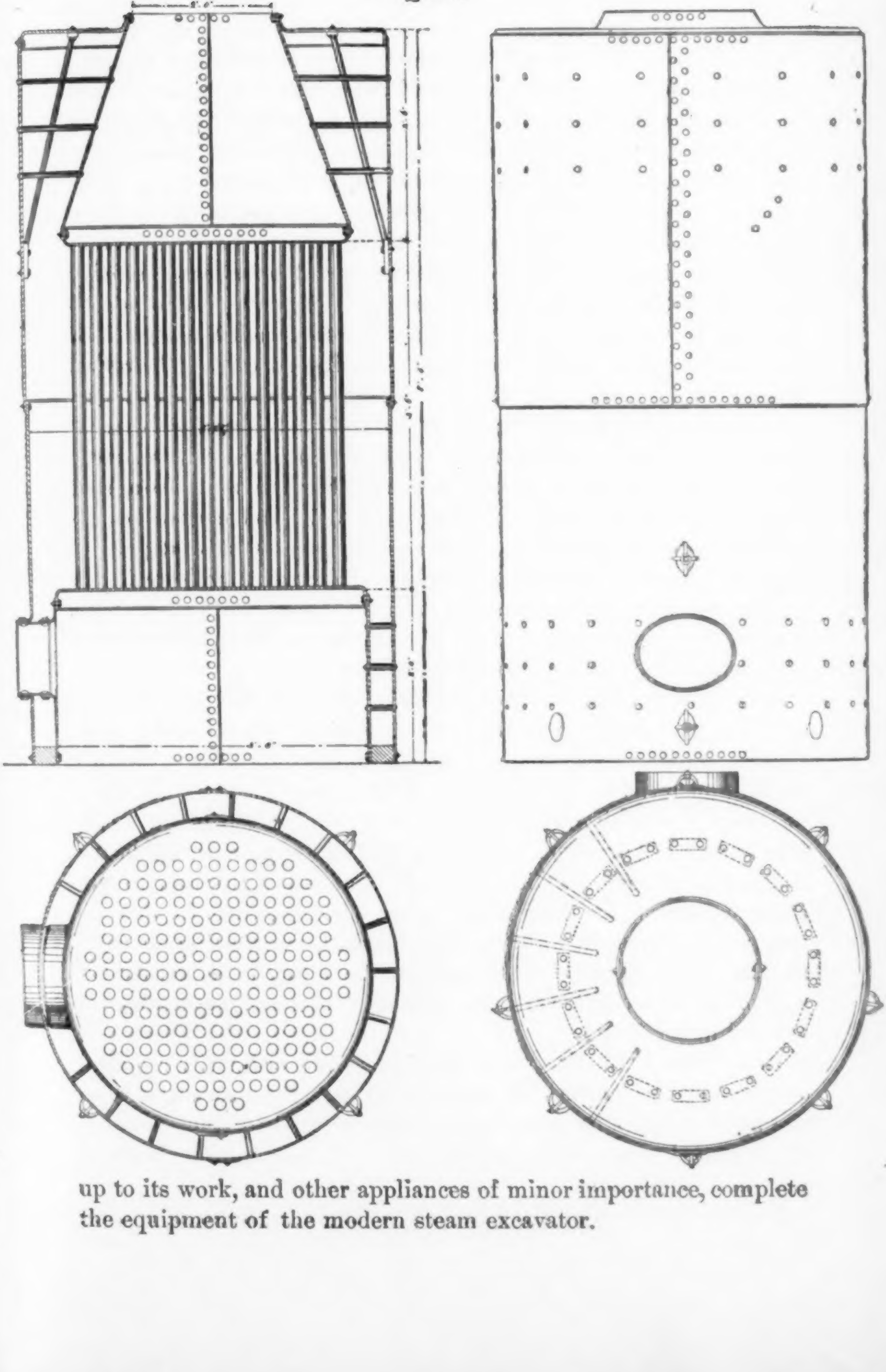
As to the construction of the car body and crane, channel iron and I beams form their frames, while the framework supporting the slewing mechanism and crane post is of wrought iron, as shown in Fig. 135.

A steam excavator is portable in the sense that it may be transferred for work at different points along a line of railway, without the necessity of taking the machine apart or altering its wheel gauge, so that it may be placed in an ordinary freight train with its tool car and tender, and be transferred quickly and safely. The running gearing and trucks supporting the car are of importance. To support the more than ordinary weight, the wheels, axles, and truck frames are stronger and heavier than those used with ordinary freight cars.

With the self-propelling feature of the machine, the front wheels and boxes operate in cast-iron pedestals or jaws bolted to the car, this rigid arrangement being necessitated for keeping the propelling mechanism in alignment.

Other additions, including jack arms for adding stability to the car body, a light and portable track on which the shovel is to move

Fig. 134.



up to its work, and other appliances of minor importance, complete the equipment of the modern steam excavator.

As before stated, the uses and importance of portable steam excavators are principally in connection with railroad work. It may be safely said that no well-equipped railroad is without one, and many extensive lines have several of them, which are placed in service at different times and places. Where work is to be done to which they are adapted, and in volume sufficient to warrant the use of a machine, the excavator is rarely dispensed with. The amount of material removed, otherwise accomplished by large crews of laborers, is enormous, and the time in which it is accomplished is short. It may be said that very few miles of railway are built without its use, and yet before it may be used to advantage the way must be built and tracks laid to permit the passage of the machine, and for the delivery of the loaded cars. Their first uses, then, in new work, are for lowering a grade, widening a cut, or furnishing ballast or filling, and the railroad excavator is rarely used, and is nearly useless in the construction of the first roadbed, or in the penetration of obstructions. For the first passage of trains a temporary roadbed having been built and connection established, the excavator is then used to greatest advantage. It may be placed in position for widening a cutting or lowering a grade, and with connection and transportation established, with successful operation, the amount of material moved and transferred is generally about all that can be taken care of.

In operation on an established line of road, the method of management, and the purpose for which it is useful are different. Ordinarily its services are required much of the time either for furnishing material for ballast or for widening cuts. It is necessary on every line with gravel roadbed to renew or repair the track foundations which severe rains have washed away, or which heavy traffic has affected. It is the practice of many railroads to use steam excavators for this purpose alone, operating them for one section, and when their work is there finished, transferring them to another. The approaches to bridges, the filling in of permanent way of wooden trestlework, are important uses and necessities for old and new railroads.

In addition to the excavator itself there are ordinarily required one or more locomotives for transferring cars, from seventy-five to one hundred and fifty gravel cars properly fitted for the operation of the gravel plow, and the gravel plow or ballast unloader with its cable and connections.

For the operation of the machine, an engineer, fireman, cranes-

man and several laborers for work in the pit preparing a way for the machine are required. The train crew consists of the ordinary working crew, together with other men for the direction of operations.

Fig. 140 represents the plan of a shovel's work in a cutting. As stated, the main line is open, at least for a distance allowing the length of the gravel train beyond the position of the excavator. If traffic is upon the line, there is usually a side track of sufficient length to allow the gravel train to remain upon it, preventing the interruption of traffic. In loading these cars, operations are commenced either from the rear or forward end of the train, and as each car is loaded the succeeding one is hauled into position to receive material. After the completion of the train load the train awaits orders, or is transferred immediately to the point of dumping, and it is there that the gravel plow or unloader is of use.

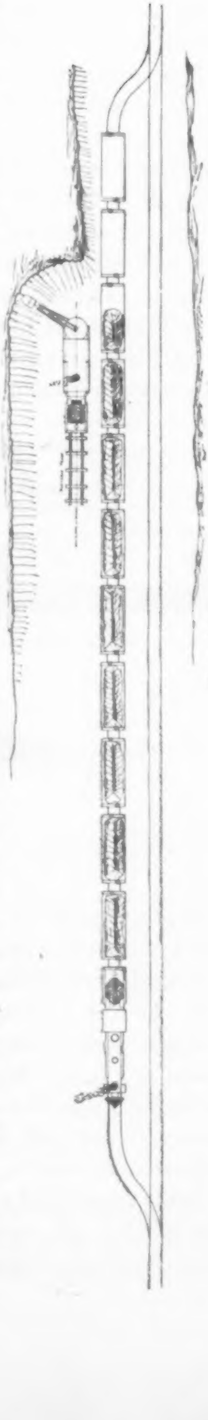


FIG. 140.

Fig. 139 illustrates such an unloader very often used. The gravel cars for use with this appliance must be fitted with a centreboard or wooden strip placed on each car as shown, and running the entire length of the train. To the plow is attached a wire cable running the entire length of the train, and with this cable attached to the draw-head of the locomotive, beginning with the farthest car, the material may be thrown from either side of the train. It accomplishes its work rapidly, and a train length may be unloaded at the rate of about two feet per second. Its construction is simple, having a cast-iron or steel headpiece to which are riveted wrought-iron side pieces, so curved and formed that they may work under the material and push it aside, while the weight of the material itself keeps the plow on the track, and prevents it from riding on the material.

Several other designs are in use differing in the details of construction, but accomplishing practically the same work.

The excavator is sometimes worked in material requiring the aid of blasting to loosen it preparatory to the work of the machine. When so working, the greatest skill of the attendants is necessary, for if this is not exercised, and the machine used as for ordinary work, it is likely to be subjected to strains which it cannot endure. However, with care the machine may endure indefinitely the most severe work in rock digging, and its use for the removal of such material has been found very economical.

In the hardest kinds of clay, in cemented gravel or hard pan, in ordinary gravel or sand, the shovel performs its functions most satisfactorily, and for this service is ordinarily used. The machine

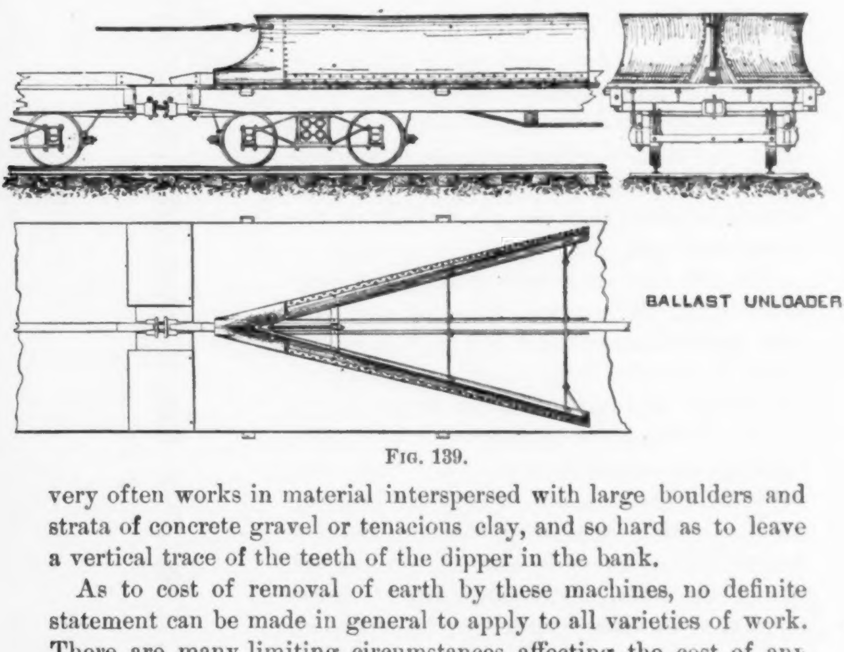


Fig. 139.

very often works in material interspersed with large boulders and strata of concrete gravel or tenacious clay, and so hard as to leave a vertical trace of the teeth of the dipper in the bank.

As to cost of removal of earth by these machines, no definite statement can be made in general to apply to all varieties of work. There are many limiting circumstances affecting the cost of any particular piece of work, but a few statements as to what has been done with some of these machines may be made, and in material ordinarily met with.

Aside from local circumstances of hardness of the material, and the skillful and successful operation of the machine itself, one most important feature affecting the cost and volume of a day's

output is the constant operation of the machine. While working in the same material obviously a machine may make very different reports, when it is kept constantly at work, or when it is delayed by a want of proper equipment in the number of cars, or in unwarrantable delay waiting for the right of way for the gravel trains. Any such delays affect very seriously the result of a season's work for a machine, and it may appear to have done badly when in reality it has done all that could be expected.

In the operation and management of a railway, the gravel train is accepted as an outcast, and the right of way for transportation from a gravel pit to its destination along the line is given only when all other trains have had their way; and, as a result, by the detention of the train the operation of the shovel is often stopped and its results severely affected. These detentions are most common occurrences, and it may be said that only in exceptional cases are excavators kept constantly at their work, and when from fortunate circumstances or by good management they are kept in constant operation, their results as to volume of material removed and cost per cubic foot is reduced often one hundred per cent. As a consequence, there are very few reports furnished by contractors or operators, without some limiting conditions affecting materially the cost of the removal of the material. The reports now in possession of the writer from excavators which have been in service in many parts of the United States may give an idea of the cost of excavation with good management and fairly satisfactory conditions. From a report of the General Roadmaster of the New York Central and Hudson River Railroad Co., of work done by two shovels on the Eastern and Western Divisions, we find the largest day's work for one shovel at Yost's pit was 174 cars, the average for the month of August being 121 cars per day, and for July 116 cars per day. We could have made a larger average than this with twenty more cars. The trains making long runs could not keep cars in the pit.

The largest day's work at Bergen pit with one machine was 156 car loads, the June average being 117 cars, and the July 116 cars per day, and for two weeks in August 134 cars per day. At this pit they came in contact with cement, hard pan, and very coarse material.

At Yost's pit they have loaded 10,511 cars in four months up to August 1. I have figured these at 9 yards per car, which is low, making 94,599 yards. The cost of delivering on road bed was

$5,161.25 or about 5½ cents per yard. The average cost for handling by men loading and unloading is 14 cents per yard.

From the report of a machine working in New Mexico on the line of the Atchison, Topeka and Santa Fé, we find from the report of the Superintendent that "the material which we could handle with the least trouble, and the cheapest, is of no use for the purpose needed. Our work has been almost wholly in cemented gravel. In this material, however, we find no difficulty under favorable circumstances in loading 75 to 100 cars per day, at a cost not to exceed 10 cents per cubic yard."

The engineer of the Cleveland, Mt. Vernon and Delaware Railroad gives some statements as to the cost and amount of some excavating work done under his direction. Shovel worked about 5½ months in stiff clay.

In	March	loaded	1154	cars;	worked	24 days.
"	July	"	955	"	"	24 "
"	Aug.	"	1157	"	"	22 "
"	Sept.	"	1556	"	"	23 "
"	Oct.	"	1552	"	"	23 "
"	Nov.	"	539	"	"	12 "

A total of 6,915 cars, 41,490 yards.

Greatest number of cars loaded in a single day, 97.

Shovel supposed to work 10 hours a day, but did not average more than 6½ hours on account of waiting on cars.

Car loads average 6 cubic yards per car.

Average cost of loading is 3 cents per cubic yard, including expense of all men around, shovel and oil, waste, etc.

Loaded, hauled material, and unloaded at a distance of ten miles from pit or shovel, at ten cents per mile, including all costs, shovel, use of cars, engines and crews. A twenty-mile haul on this road costs 15 cents per yard, and a thirty-mile haul about 20 cents per yard, while I have known some roads on which a thirty-mile haul costs over 75 cents per yard, depending on frequency of trains running on road.

The reports show how variable is the cost of excavating, depending as it does upon delays unavoidable on every line of railway, upon the weather, character of the material, length of haul, and many other conditions. When conditions are favorable as to material, prompt and short hauling, with no delays, the results show a very large increase in the output, and often a decrease in cost. The following report from the Superintendent of the Sioux

City and Pacific Railway gives the operations of a shovel for nine months working in a yellow clay bank from 30 to 40 feet in length, and with a one-mile haul:

The total number of cars loaded was 31,420 in 209 days, giving an average of 150¾ cars per day. The greatest number of cars loaded in one day was 275, with an average of 6 cubic yards per car. The average cost of loading per cubic yard is 6½ cents, including expense of all men about shovel, and shifting of shovel track. Average cost of unloading with one-mile haul, 7.8 cents, including wages of all men with trains and engines, use of cars and locomotives, with all supplies and repairs of same, making a total cost of 14.3 cents per cubic yard, or 85.8 cents per car delivered on track.

A report showing the largest amount of work, with the most complete detail as to the expense of operation, is furnished by the resident engineer of the Missouri Valley and Blair Railway and Bridge Co., contractors for the Chicago and Northwestern bridge across the Missouri River at Missouri Valley, the material excavated being used in the approaches to the bridge.

The work, a report of which is herewith given, was done under the most favorable circumstances, with but few delays, and with but one locomotive, as the cars ran down the hill themselves while being loaded, the locomotive being employed to haul the empty cars back; the haul was short, and a round trip was made in thirty minutes.

The report shows that during the work of six months the average number of cars loaded per day was 205, including delays and movings, and that the average cost per cubic yard was 7 cents, which, as shown, included labor of loading, moving shovel about once a month, moving track to suit, dynamite for caving bank, repairs of shovel, fuel, oil, waste, wages of watchman, rent of cars and locomotives, labor of engineers, firemen and wipers, labor conductors and brakemen, and in fact absolutely everything connected in any way with filling the embankment.

Such results are very satisfactory and are seldom equaled, and would not be accomplished were the road open to other traffic or with long hauls.

For work in connection with the maintenance of the roadbed, the excavator is growing yearly in favor. From their increased manufacture there comes a development in its capabilities which renders its use almost indispensable in the maintenance of way, and at the same time, with this competition there is a reduction in the

STATEMENT SHOWING WORK PERFORMED BY STEAM SHOVEL AND COST OF SAME FOR 6 MONTHS IN 1885.

	REPAIRS TO LOCOMOTIVE, SHOVELS AND CARS.		Supplies for Steam Shovel.	Rent of Locomotives and Cars.	Supplies for Locomotives.	Wages of Eng'rs, Firemen, and Wipers of Locomotives.	Wages of all other men including trainmen and men on bank.	Total Cost.	Cars Loaded.	Cost per Car.	Hours worked by Gang.	Hours worked by Shovel.	REMARKS.
	Material.	Labor.											
April . . .	$56.64	$29.22	$229.90	$232.25	$200.75	$174.34	$1,153.28	$2,076.38	$3,339	.6218	257	233	Moved shovel from end to end of emb'k't.
May	145.53	25.31	248.69	234.00	359.48	272.75	1,980.33	3,266.90	6,518	.5010	430	366	"
June. . . .	73.78	27.76	328.18	236.50	335.15	285.85	2,033.33	3,338.05	6,326	.5276	421	355	"
July	104.71	43.47	540.67	234.00	333.55	289.73	2,067.17	3,615.80	6,529	.5538	449	376	"
Aug	50.80	74.73	247.00	234.00	260.98	286.58	1,961.93	3,116.02	4,474	.6964	418	308	Moved shovel from end to end of cut.
Sept	25.68	11.13	165.56	234.00	291.61	199.12	1,463.97	2,391.25	4,955	.4826	340	288	Moved shovel track one side cut to other.
Total . . .	$457.14	$211.80	$1,760.00	$1,404.75	$1,781.52	$1,508.37	$10,680.01	$17,803.59	$32,141	.5538	2,325	1,926	

cost of the machines, which places them in the market at a very low figure.

Their use obviates the necessity of large crews of laborers, and this fact alone, when the striking element is employed, increases the advantages of the machine and the tendency to its general use.

CCCII.

STRAINS IN LOCOMOTIVE BOILERS.

BY L. S. RANDOLPH, MT. SAVAGE, MD.

(Member of the Society.)

THE failure of locomotive boilers by cracking in an apparently mysterious manner caused the writer to take a special interest in the subject a number of years ago in order to determine, if possible, the cause.

The first point which came under observation was the breaking of stay-bolts, large numbers of which were found broken, invariably close to the thicker sheet, the fracture being square across, and, in 90 per cent. of cases examined, flush with the inside surface. These stay-bolts were of the usual form, threaded throughout their entire length, and screwed through both sheets.

When the fracture was not flush with the sheet, it occurred slightly under the sheet.

Upon breaking the stay-bolt away from the thinner sheet, the fracture was invariably crystalline, the bolt breaking like cast iron with one or two blows of a hammer; the fresh portion of the fracture sometimes included the whole area of the bolt, sometimes only a portion, which was generally elliptical in form, showing that the crack had started some time before.

The central portion of the bolt when broken in a vise showed a silky fibrous fracture, proving that the material had originally been good, but had been injured at the ends where it entered the sheet.

It was also observed that the stay-bolts near the upper corner of the fire box were always the first to go; those in the central portion of the water space near the mud ring very seldom giving way.

The only theory which would fit the observed phenomena is that the difference of temperature between the outside and fire-box sheets causes a difference in the amount of expansion, which, by constant repetition, " wiggles the bolts in two."

Assuming a difference of three hundred and forty degrees Fahr-

enheit between the temperature of the two sheets, we would get a motion of one quarter of an inch ($\frac{1}{4}''$) in a fire box ten feet long, which would give one-eighth inch for the end bolts, assuming the expansion to take place uniformly from the centre.

In order to test the effect of this motion, a stay-bolt was fitted up as usual, one sheet being three-eighths inch ($\frac{3}{8}''$) thick, the other five-sixteenths inch ($\frac{5}{16}''$); by fastening one to the head of a shaping machine, and the other to the bed, the $\frac{1}{8}''$ motion was obtained.

The following table gives the results, the distance between the plates being four and one-half inches ($4\frac{1}{2}''$) and the diameter of bolts over threads seven-eighths inch ($\frac{7}{8}''$).

No.	No. of Vibrations.	
1	1400	Fastened with nut under one plate, viz., firm.
2	5569	" " " " not firm; used same plate as No. 1, hole worn.
3	3345	Riveted with four-pound hammer.
4	2040	" " " " "
5	5400	" " two " "
6	2600	" " " " "
7	1600	" " " " " centre of bolts had threads turned off to diameter of 0.726". Threads $\frac{3}{8}''$ above plate.
8	600	Same as No. 7, diameter 0.625".
9	1540	Same as No. 8. Threads remain $\frac{1}{8}''$ above plate.
10	1700	" " " Threads turned off to plate.

All of the above were with the same shipment of stay-bolt iron. Several tests of stay-bolts made of common iron gave from 800 to 1,300 vibrations, and one test of a special sample of stay-bolt iron gave 12,000 vibrations.

Assuming that fires are banked twice a day for 300 days in the year, would give 600 vibrations per year, which, for 2,500 vibrations as the life of the stay-bolt, would give over four years as the life of the bolt under maximum vibration. This is about a year longer than they usually last.

The use of the two-pound hammer increased the durability considerably, probably on account of the bolt being held with less firmness in the sheet.

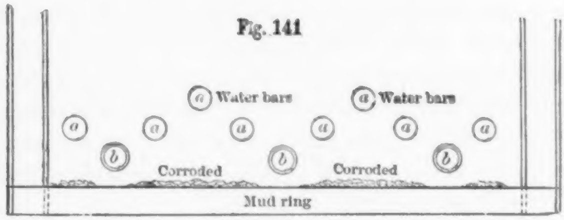
The writer has attempted to increase the durability by making the bolt fit loosely in the plate, but found that corrosion so soon cemented the two together that the trial was abandoned.

The results obtained on stay bolts naturally led to the question,

What is the effect of expansions and contractions on the boiler generally.

The first observed effect was a bad case of corrosion in the back

Fig. 141



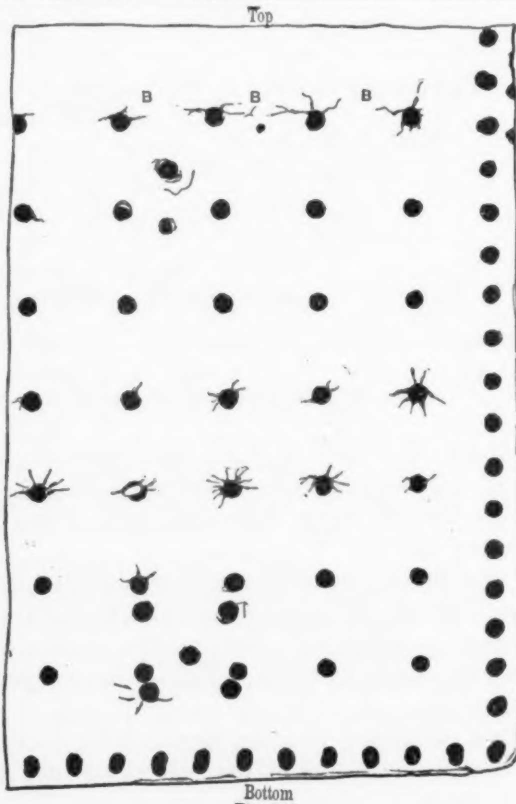


Fig. 142.

sheet of a fire box of an anthracite coal-burning engine at the mud ring. Fig. 141 shows the arrangement, *a a* are water-tube grate bars attached rigidly to the front sheet; *b b* are thimbles through

which the solid bars passed. Under *a a* there was very bad pitting or grooving; under *b b* none at all, the bending of the sheet by the expansion and contraction of the water-grate bars *a a* kept the scale broken so that the water could get in to the iron.

Indications of the same action were found in several bad cases of grooving, both on longitudinal and circumferential seams with the ordinary single riveted lap joint; and in the flanges of the front flue head, although the latter, in all but one case, showed very decided evidences of cracking of the metal from the repeated strain, the corrosion being more of an after effect.

Fig. 142 shows very distinctly the effect on the sheet by the

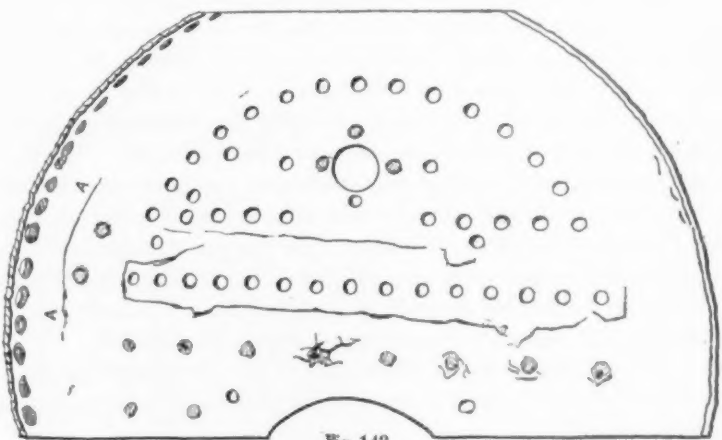


Fig. 143.

bending of the stay bolt in the cracks radiating from the stay-bolt holes. At B B the cracks extend almost from one stay-bolt hole to another.

Fig. 143 shows the effect of expansion and contraction on a flange, the first intimation that there was anything wrong with the boiler being the steam blowing through the crack marked A A. The cut shows the appearance from the inside.

The writer's experience has shown that in nearly every case the failure of boilers is due, except where corroded by bad water, to failures of metal, from repeated straining beyond the elastic limit by difference in amount of expansion and contraction.

So far the use of large easy curves for flanges and very soft

metal throughout the boiler are the only methods which have met with anything like success in overcoming this difficulty. Corrugated flues and fire boxes would probably save a great deal of the trouble, but they are open to objection, especially where the coal used contains much sulphur, on account of corrosion caused by soot and ashes lying in the corrugations.

DISCUSSION.

Mr. Harvey Middleton.—My experience will bear out all that Mr. Randolph has stated as to that peculiar form of boiler. But recent practice has shown that the Belpaire boiler overcomes it. I can account for that only by the fact that it gives us a better chance of staying the boiler, both to the crown sheet and the roof sheet, and outside sheet to side sheet of fire-box. Mr. Randolph's paper has opened up a field which will develop, I think, a reason why the Belpaire boiler is giving us this satisfaction in not breaking as many stay-bolts as the old form of boiler. We find the Belpaire boiler much more satisfactory in that respect than the old form, on account of our being able to stay it in perfect line. Take the cylindrical form of boilers; the threads of the stay-bolts are not in many cases over one full thread in the sheet, while with the Belpaire we get full threads in both sheets. The accompanying illustrations show its construction, Figs. 307 and 308.

Mr. H. de B. Parsons.—I should like to ask Mr. Middleton if the bolts in the Belpaire boiler are not longer than the stay-bolts in the other class?

Mr. Middleton.—Not in the side sheets; they are in the roof sheets. Instead of using bars we use stay-bolts about eighteen to twenty inches long. The stay-bolts, as a rule, are all in perfect line. The body of the Belpaire boiler is such that it is nearly all straight lines, except the curve from the side sheets to the roof sheets.

Mr. Wm. Kent.—There is one little fact mentioned in that paper which is of some importance in other branches than locomotive building. I refer to the apparent crystallization of the ends of stay-bolts. It has been a disputed question as to whether a piece of wrought iron which was good originally could be crystallized. Here we have evidence showing that iron which was originally good can be crystallized, and if it happens in hundreds of instances, it seems to be strong proof that iron which is originally of excel-

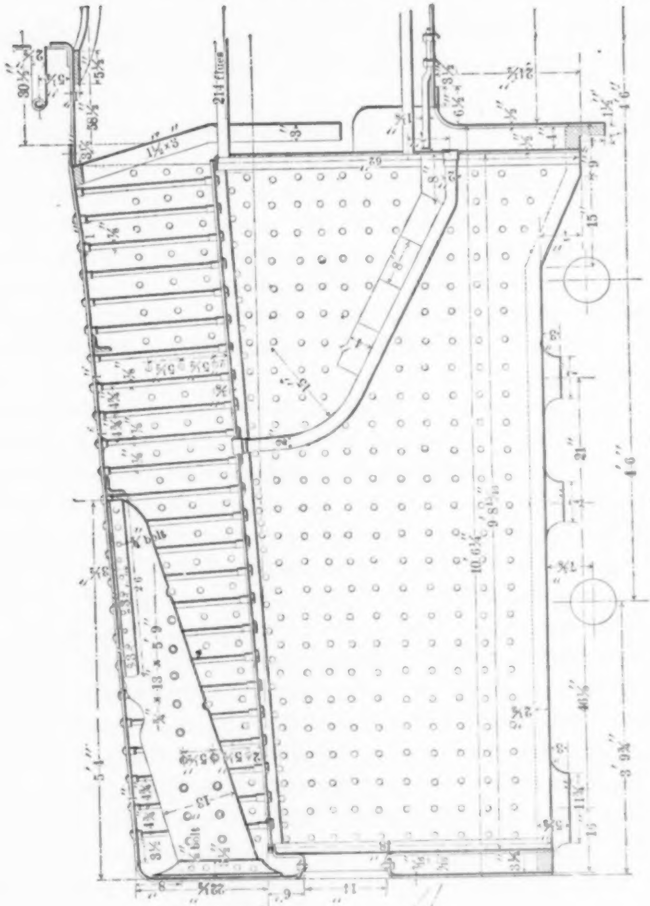


FIG. 308.

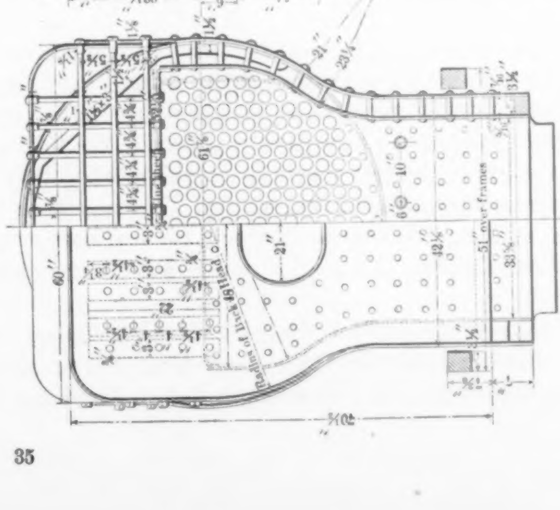


FIG. 307.

lent fibrous structure will, under repeated vibrations, assume a structure which looks crystalline, whether it is crystalline or not.

Mr. S. W. Baldwin.—Mr. Randolph mentions that his stay-bolts in the shaping-machine test stood a number of vibrations corresponding to a life a year longer than is usually expected in practice. I would suggest that this difference could be accounted for by the fact that in actual use the stay-bolts are under tension when they are flexed. In the machine test the bolt was probably not under tension. A bolt could not be expected to last as long under the sum of the strains as when vibrating without being under tension.

Mr. J. T. Hawkins.—I think the case cited of stay-bolts is an instance warning the engineer to avoid, in every possible way, anything like a re-entering angle in members of structures which receive strains, such as a thread in these stay-bolts where they are to be subjected to any such action as is characterized as "wiggling" in the paper. It seems to me to be akin to that which I described in the topical discussions of this meeting as coming from the disintegration of the material in shearing plates. We have here a stay-bolt which is threaded throughout its entire length, and fracturing invariably, as shown, either near or within the plate, and it appears to me that this is a case where the re-entering angles of the threads give every facility for initiating a crack or cracks in the bolts from the continued vibration or oscillation produced by the difference of expansion of the two sheets, and the lesson that is taught principally by that is to avoid that kind of a stay-bolt where there is any possibility of "wiggling," which is a good term for it.

Mr. Parsons.—At the foot of the first page the writer observes that the stay-bolts in the upper corner of the fire-box are always the first to go. The reason for that, it seems to me, is the fact that the mud-ring at the base gave that part of the boiler so much strength, and held the two sheets so very firmly together, that the expansion was upward, and the greatest difference then would be at the top, and the top stay-bolts would be the ones subjected to the greatest wiggling, if I might be allowed to use the writer's words.

Mr. H. P. Minot.—Isn't it always the case that the stay-bolts give out near the outer sheet? I understand that to be so. If it is so, why does it give way there more than in the inner sheet?

Mr. Middleton.—I think that is only a supposition. In examining a boiler that exploded two years ago, I noticed that there were as many broken stay-bolts in the inner sheets as in the outer, some right in the centre.

The President.—Don't you think the upper stays had a greater strain on them than those below?

Mr. Middleton.—They had in a measure; but at the same time I think it is owing to the peculiar form at that point. There is a flat spot right at the top of the outer shell, opposite the crown sheet, and I think it is owing a good deal to the vibration as mentioned in Mr. Randolph's paper.

Mr. Jacob Reese.—Are the stay-bolts iron or steel?

Mr. Middleton.—Iron.

Mr. Reese.—And presumed to be fibrous when put in?

Mr. Middleton.—Yes, sir.

Mr. Reese.—The cause of fibre, as I take it, is in the method of making iron, either by the catalan fire or by the puddling process. I will specially describe the puddling process. The process is conducted at a temperature, say, about three thousand degrees Fahrenheit. The metal is melted at about two thousand. As the carbon is taken out the fusion point is raised up to about four thousand, and the iron freezes in the furnace at a temperature of three thousand. It is too cold for it. It freezes the same as water crystallizes when it is subjected to cold. And this freezing iron in the puddling furnace comes up in little crystals, the same as water does in freezing, and they unite with each other until we get the puddled ball. That puddled ball is a regular sponge, and there is an oxide that runs over it, and it runs down and permeates the whole thing. When it is squeezed and rolled out, the oxide is interlaid between the particles of iron, so that there is an interlamination of iron and oxide distributed throughout the whole bar, so that when it is pulled out its fibre is threads of laminæ between the particles of oxide. By concussion and vibration the oxide breaks and the iron exhibits a granular texture. It is the iron which is tough and ductile; the oxide is hard. The vibration breaks that and gives the phenomenon of crystallization.

Mr. Middleton.—Yes; but the explosion developed the fact that there were quite a number of stay-bolts that had been broken in the manner described by Mr. Randolph. All those show good material, but at their points they showed crystallization.

Mr. Reese.—It is as impossible to get a perfect piece of iron as it is to find a perfect man. You take a bar, and it will break just where you did not expect it to.

The President.—Were those stay-bolts all iron?

Mr. Middleton.—Yes, sir; steel has not generally come into use in locomotives. We usually try to get a tough fibrous iron similar to the Sligo. Sligo is taken as a standard—an iron that will bend cold to itself without cracking. That is the usual test for all stay-bolt iron.

The President.—They are using steel in marine boilers.

Mr. Hawkins.—I would like to ask Mr. Middleton, or any other member present, if he knows from his experience whether the well-known thimble stay-bolt gives way in the same manner that the threaded stay-bolt does? I think that would be the point that would show whether the position I take on that question is sound. It would be pretty well settled by that—if the thimble stay-bolts do not give way and the threaded bolts do.

Mr. Middleton.—I cannot answer that question. Thimble stay-bolts are used very little on locomotives.

The President.—The trouble with the stay-bolts, the type which you speak of, is the very great difficulty in keeping those bolts tight on heavy pressures. We abandoned them, and use in marine practice a screw stay-bolt with a nut on each end; a nut inside the fire-box and a nut outside the fire-box.

Mr. Middleton.—We only use the nut stay-bolts on the crown sheet. On the side sheets they are just merely screwed into the sheet and rounded over in the usual manner.

Mr. Minot.—I think that it is a pretty general thing for these stay-bolts to give way close to the outside sheet. I have that from the master mechanic and superintendent of motive power at the Fort Wayne shops of the Pennsylvania system. They drill a small hole, I should think about $\frac{3}{16}$ of an inch, into the outside of the stay-bolts, so that they may know at once when that fracture takes place. I looked into the matter considerably when I was in the shop, and they told me that they invariably broke next to the outside sheet, and never at the inside sheet.

Mr. Kent.—I would like to ask if any one here has any experience in hollow stay-bolts which are expanded into the sheet instead of screwed.

Mr. Hawkins.—I would like to ask the President if he has any experience of the kind of stay-bolts mentioned by him

giving out at any point? It would seem to me that that kind of stay-bolt would give out at the nut if anywhere.

Mr. Horace See.—We have been using them now about two years, and have not had any of them to give out, nor have we had the socket-bolt give out so far as breakage is concerned. The trouble with the socket-bolt, where the head on one end is driven by hand, is to make it tight. A man in driving such a bolt will sometimes upset it all on one side, and there will be nothing left for the joint or to calk it tight. It is necessary in all boiler work of to-day to bring it down, as near as possible, to machine accuracy. The marine boiler of to-day is built very much like the steam engine, the most of the work being done by machines. The shells are all drilled; in fact, very nearly all of the holes in the boiler are drilled. Screw stays are used wherever it is possible, because they can be made and fitted with much greater accuracy than a stay-bolt driven by hand. Hand-riveting is avoided, and machine-riveting is used as much as possible. I think in the case of trouble with the upper stay-bolts that expansion played a very important part. Expansion is greater on the inside, where the fire is located, than it would be on the outside. The outside diameter of the boiler is very nearly a constant one, the changes are not as great or rapid as on the inside, and that continual change of the length of the inside has a racking effect on the stay-bolts: the racking effect is a maximum close to the outer sheet, and the leverage is against the stay-bolt at the outer sheet.

Mr. Parsons.—As regards the breaking of the stay-bolts near the outer sheet, I think there is a very good reason in the fact that the outer sheet is stronger than the inner sheet. In the first place, it is generally thicker; in the next place, it is a part of the whole shell structure of the boiler. The inner shell is weaker, because it is generally thinner. Now for differences in temperature, the plates are trying to pass, one by the other, and cause a bending strain in the stay-bolts; the inner sheet, being thinner and weaker, may bend a little; a little is enough to release this strain. The outer sheet being thicker and stronger, holds the bolt firmly at that point, and in consequence the section near the outer sheet is the one to receive the greatest strain; and, in general, if the bolts are equally strong and of uniformly good material, they will break at that point.

Mr. Kent.—In answer to Mr. Hawkins' question, I think such a stay-bolt is used in the Heine boiler.

Mr. Wilcox.—You are mistaken about that, Mr. Kent. They are screwed in the Heine boiler. The hollow stay-bolt is .88 inch inside diameter, and 1.66 inches outside diameter; it is cut in lengths of about 12 inches. The distance between the sheets is only 11 inches, less the thickness of the sheet, and it is screwed in in both cases. We have an ingenious way of putting these stay-bolts in. We use the Stowe flexible shaft. We have what is equivalent to an expanding reamer, which runs into the short tube, and then it is put into the boiler and screwed as tight as it can go.

Mr. Parsons.—In answer to Mr. Kent, I would say that hollow stay-bolts are used very often in the English locomotive practice. I think they are both expanded in and screwed in. I have, however, had no experience in the English shops; my information merely comes from the designs of recent practice in that line.

*Mr. L. S. Randolph.**—Mr. Kent refers to the *apparent* crystallization of the stay-bolts. Although I have seen well-formed cubical crystals in the fractured surface of a wrought iron axle broken under the drop test, the word there is used to describe that peculiar silvery fracture which is usually called crystalline. I do not believe that simple vibration can produce it in iron originally good, but always regard it as proof of the metal having been strained beyond the elastic limit several times. I am not prepared to state that a single overstrain will do it, although I think that it will unless the metal is exceptionally good.

I agree with Mr. Baldwin. The bolts in the machine test were not subjected to a tensile strain.

In reply to Mr. Minot, would say that the outside sheet is almost invariably the thickest, with the exception of the flue-sheet; and as the stay-bolt has a longer bearing, it is held firmer, is bent more, and consequently breaks sooner.

Mr. Middleton speaks of Sligo iron being the strongest. The sample mentioned as standing 12,000 vibrations was Sligo iron.

Since this paper was written, I have tested samples of the iron from sheet shown in Figs. 143 and 142. Fig. 142 stood 49,200 lbs. per square inch, and gave 6% elongation. Fig. 143 stood 48,500 lbs. per square inch, and gave 6½% elongation.

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* Author's closure, under the Rules.

CCCIII.

MEMORANDA ON THE PERFORMANCE OF A COMPOUND ENGINE.

BY GEO. H. BARRUS, BOSTON, MASS.

(Member of the Society.)

THE records of the Society contain little information regarding the performance of compound engines, and there are few data anywhere on record, so far as I am aware, as to the work of a certain class of mill engines which is now coming into use, which consists of two unjacketed cylinders placed side by side and connected with an unheated intermediate receiver. Having recently made a test of such an engine under favorable circumstances, I embrace the opportunity to submit the data and results which it gave.

The diameter of the high-pressure cylinder was 26 inches, that of the low-pressure cylinder 48 inches, and the stroke of both was 60 inches. Steam was furnished by Harrison sectional boilers, after first passing through a steam drum of ample size. The exhaust steam was condensed in a surface condenser, the circulating water for which was supplied by a duplex steam pump having steam cylinders 9 inches diameter, water cylinders 10 inches diameter, both 12 inches stroke, which also exhausted into the condenser. The engine was fitted with a connected air pump which discharged the condensed water into a hot well, consisting of a large rectangular tank. The intermediate receiver was drained by a trap which discharged into the hot well. The engine was of the Corliss type. The governor operated on the cut-off of the high-pressure cylinder. This had no connection with the cut-off of the low-pressure cylinder, which was arranged so as to be fixed at any desired point.

The results of the test are based on the quantity of steam exhausted from the engine, the condensed water discharged from the surface condenser being measured for this purpose. The use of the surface condenser enables this determination to be made with unusual accuracy.

JAN. 18, 1888, 3.27 P.M.

Scale H. P. Cyl. 60; L. P. Cyl. 20.

Boiler pressure by engine-room gauge,	94.5	lbs.
Receiver " " " " "	6.7	"
Revolutions per minute	52.3	

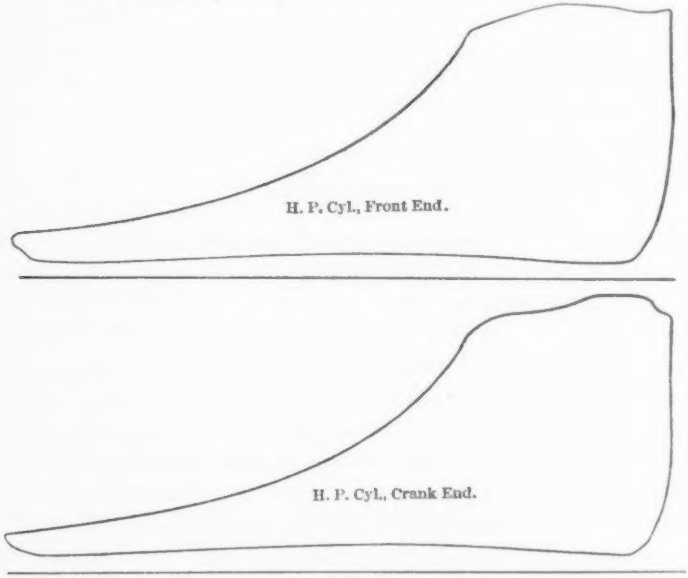


Fig. 144

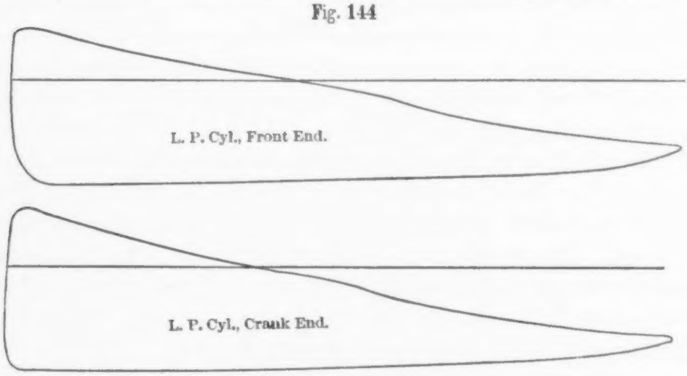


Fig. 145

The data and results of the test are given in the following table, appended to which are copies of a representative set of indicator diagrams. Fig. 144 represents the cards from the high-pressure cylinder, and Fig. 145 from the low-pressure cylinder.

1.	Duration	hrs.	4.5
2.	* Total quantity of steam consumed	lbs.	44,436.3
3.	Steam consumed per hour	"	9,874.5
4.	Average boiler pressure by engine-room gauge	"	94.3
5.	" receiver " " " " "	"	6.4
6.	" vacuum " " " " "	"	27.2
7.	" revolutions per minute	rev.	52.3
8.†	" mean effective pressure	H. P. Cyl. L. P. Cyl.	41.14 9.27
9.	" indicated horse power developed	H. P.	606.53
10.	Steam consumed per I. H. P. per hour	lbs.	16.28
11.	Coal consumed per I. H. P. per hour, based on stipulated evaporation	lbs.	1.63

‡ MEASUREMENTS AND COMPUTATIONS BASED ON TWO SETS OF SAMPLE DIAGRAMS.

			H. P. Cyl.	L. P. Cyl.
				[Receiver]
12.	Boiler pressure by gauge in engine room	lbs.	94.2	6.2
13.	Highest initial pressure in cylinder above atmosphere	"	89.4	6.0
14.	Average cut-off pressure in cylinder above zero	"	91.9	12.6
15.	Average compression pressure in cylinder above zero	"	22.6	3.7
16.	Average release pressure in cylinder above zero	"	28.4	7.5
17.	Proportion of direct stroke completed at cut-off		.505	.544
18.	Proportion of return stroke uncompleted at compression		.047	.027
19.	Back pressure at mid stroke + or − atmosphere	lbs.	+8.1	−10.8
20.	Back pressure at lowest point + or − atmosphere	"	+5.7	−11.5
21.	Mean effective pressure as measured	"	41.26	9.28
22.	Equivalent M. E. P. referred to one cylinder	"	73.10	21.31
23.	Steam accounted for by diagram at cut-off	"	12.60	11.78
24.	Feed water consumed per I. H. P. per hour [line 10]	"	16.28	
25.	Proportion of feed water accounted for at cut off	H. P. Cyl.	.774	
26.	Proportion of feed water accounted for at cut-off	L. P. Cyl.	.723	

The valves and pistons were tested independently for leakage. The valves were found in excellent condition. So also was the piston of the low-pressure cylinder. The piston of the high pressure cylinder leaked to some extent.

* Includes that used by circulating pump.

† Average of diagrams taken every 20 minutes.

‡ Assumed clearance each cylinder 3 per cent.

DISCUSSION.

Mr. J. S. Coon.—I would like to ask Mr. Barrus how he accounts for the rise in the return line of the high-pressure card when there should be a drop.

Mr. Barrus.—I supposed it was due to the manner in which the steam was drawn from the receiver. I had not looked at that particularly.

Mr. H. B. Gale.—There is a little addition to the data given here which I would like to obtain if Mr. Barrus can furnish it; that is, what was the volume of the receiver, and how were the two strokes timed in relation to each other?

Mr. Barrus.—The receiver was simply a pipe, perhaps a little larger than just sufficient to carry the steam, and the cranks were set 90 degrees apart.

Mr. Coon.—If it would be in order, I would like to give some data about a compound engine which I tested. It was one of the engines at the Boston Sewage Works. The diameters of the cylinders are $25\frac{1}{2}$ and 52 inches, the stroke is 9 feet, and the piston speed was about 190 feet. The steam pressure carried was 100 pounds. The cylinders are thoroughly steam-jacketed and the interspace is also jacketed—or the superheater, as it is called, the volume of which is almost exactly equal to the volume of the high-pressure cylinder. There were two tests made; one was made in the winter, and the other in the spring. The consumption of feed water per hour was $14\frac{7}{10}$ pounds per indicated horse-power, and the consumption of coal, which was of picked quality, was $1\frac{33}{100}$ pounds, which, so far as I am aware, is the best record which has ever been made in this country.

Mr. Barrus.—I would like to ask Mr. Coon if the quantity of steam used in the steam-jacket was measured?

Mr. Coon.—It was not.

Mr. H. P. Minot.—I cannot understand that card the way Mr. Barrus has explained it—that the crooked line there is due to the receiver. I do not think the steam got into the receiver at the time that line was made. I think that we ought to find out what the matter is there.

Mr. H. de B. Parsons.—If the cranks were set at 90 degrees apart, and the receiver had a capacity equal to that of the high-pressure cylinder, or a little larger, then at the end of the stroke, when the valve would release the steam, it would go into that receiver and

the diagram would show a low pressure at that point. Then, as the piston returned and pushed the used steam out of the high-pressure cylinder, forcing it into the receiver, that is reducing the space, it would gradually raise the pressure in the receiver until the engine had come to the quarter-stroke on the return, when the valve opens to the low-pressure cylinder, slowly at first, and allows the steam in the receiver to flow into the low-pressure cylinder. We thus have the fall shown on the bottom curve of the diagram until the point of closing of exhaust of the high-pressure cylinder. Then follows the compression and the admittance of live steam, and the card is traced over again. If the one cylinder had exhausted directly into the other, the whole card would have been of an entirely different shape.

Mr. E. S. Cobb.—I would like to inquire of Mr. Barrus if I understand what he said correctly. I believe he said that the steam in the high-pressure cylinder had its cut-off regulated by the governor, and the steam in the low-pressure cylinder had its cut-off fixed at a certain point before the engine was started. Is that correct? I would like to inquire then what effect the action had upon the indicator diagram, when the engine automatically cut off at a greater or less point in the high-pressure cylinder? Did not that change the shape of these diagrams very materially, or was not that point considered? It seems, from the size of the receiver between the high and low pressure cylinders, that any difference in the cut-off in the high-pressure cylinder must necessarily affect the action in the low-pressure cylinder. How much that effect was, I would like to know.

Mr. Barrus.—I do not think I can state that without reference to the diagrams. I presume an examination of the diagrams would show that.

Mr. Coon.—I have taken a very large number of indicator diagrams from compound engines of this type, cutting off in the same engine from very early to very late in the high-pressure cylinder, with no change in the point of cut-off in the low-pressure cylinder, and the only variation is in the area of the cards. The low-pressure card would appear just as it does on this paper, except that the steam line would be lower. The points in it would be just as they are here, except the card would be thinner, so to speak, vertically, and the high-pressure card, part of it, might perhaps, in cutting off earlier than this, drop below the atmospheric line; but otherwise the relations would remain as they are here, and there

should be no change in the point of cut-off on the low-pressure cylinder, no matter what the range is in the high.

Mr. Cobb.—I would like to ask Mr. Coon if he would consider that last statement true without regard to the proportion the receiver held to the high-pressure cylinder; that is, if practically it was simply a pipe leading from the high-pressure cylinder to the low-pressure cylinder, would not that point of cut-off make an effect on one of the diagrams?

Mr. Coon.—If the interspace were very small, it would.

Mr. Cobb.—Was the size of the steam pipe or the size of the exhaust pipe what would naturally be used for the size of the high-pressure cylinder?

Mr. Coon.—I shall have to retract what I said then. It would not. You take the Holley pumping engine, which has the smallest interspace of any engine built, so far as I am aware, in which the cylinders are placed as close together axially as they can be, with only one valve between the high and low at either end, the clearance space is nothing, it is simply a clearance in the cylinder proper, and there is never any change in the point of cut-off between the cylinders, no matter what the range is in the high-pressure cylinder. That is a fixed thing, and it should be no matter what the size of the interspace is. But, of course, the size of the interspace would determine the point at which the low-pressure cylinder should cut off.

Mr. Cobb.—Does Mr. Coon intend to state that that would be true whether the cranks were at 90 or 180 degrees of one another? It seems to me that must be true when the cranks are 180 degrees of one another; but when they are at 90 degrees of one another I hardly see how that could operate.

Mr. H. H. Suplee.—In regard to this matter of cut off in compound or multiple-expansion engine in a low-pressure cylinder, I would like to ask if it is not customary, particularly in triple-expansion engines, to vary the cut-off in order to divide the work equally among the cylinders, and if the cut-off is changed in the high-pressure cylinder, to readjust as equally as possible the cut-off in the other cylinders, in order to permit the work to be as nearly as possible divided equally among the three cylinders? I think that is done in marine practice.

Mr. Horace See.—I might say that in marine practice not much attention is paid to that. If an independent cut-off is used at all on an engine, it is only on the high-pressure cylinder.

Mr. Suplee.—Is there no link motion on the other two cylinders?

Mr. See.—The link motion is used, but no particular attention is paid either to its independent adjustment on any of the cylinders, and the same is true if the Joy or the Marshall gear is used, although the reversing arm may be slotted.

Mr. Suplee.—Simply for reversal only?

Mr. See.—Yes; there is no independent cut-off.

Mr. Suplee.—I mean similar to the adjustment of link on a locomotive engine, with the notches.

Mr. See.—No; nothing similar to that. On a three-cylinder engine all of the links must be reversed together. You cannot reverse them separately. There is not that attention paid to the equal distribution of work in the different cylinders which there is in land practice.

Mr. Parsons.—The remarks made refer to the usual practice, but there are special cases where the variable cut-off is placed on one of the cylinders to equalize its work, where a vessel's trip is at different speeds. Some classes of men-of-war, which are in peace run at slow speed, have been arranged with a variable cut-off for the intermediate cylinder; but it is not the usual practice.

Mr. See.—That is so only on two-cylinder compound engines, not on the triple-expansion engine. As a rule there is no provision made on the triple-expansion engine. The plain compound engine generally has an independent cut-off on the high-pressure cylinder.

Mr. Parsons.—I could not state now from memory the name of the ship (though I could doubtless ascertain it from reference) on which there was an independent arrangement to equalize the work done on the three cylinders, because the ship was expected during her voyage to make variable speed; full power during one part, and two-thirds during the rest.

Mr. See.—They sometimes use a slotted arm, but a slotted arm is not often used.

Mr. Gale.—It seem to me that there is a very good reason for not varying the low-pressure cut-off in marine practice, and that there is often very good reason for varying it in land practice. In marine practice the work done by the engine is usually very nearly constant; the ship is generally run at a certain speed, and there is seldom any reason for departing from that for any length of time; but in land engines, where the work has to vary consider-

ably, it seems to me that the cut-off should be varied simultaneously on all the cylinders, whether there be two or three; otherwise the work would be at some times very unequally distributed among the cylinders; also the efficiency is more altered if the cut-off is varied simply in one cylinder than if it is varied proportionally in all the cylinders. If we suppose an engine to be adjusted for a certain work to run at the greatest efficiency, and there is a certain amount of work done in each cylinder, then, when the whole work done by the engine is changed, we ought to change the cut-off to correspond in each cylinder, and keep the ratio of work the same. If I understand the theory of the compound engine, that would be the way to get the highest efficiency; though in cases where the work of the engine ordinarily varies but slightly, the gain would probably not pay for the increased complexity. I would like to hear the reason for the statement that the cut-off on the low-pressure should remain fixed, while that on the high-pressure is varied.

Mr. Coon.—The point of closure of the low-pressure inlet valve is determined at that point where the compression in the interspace between the cylinders shall be such that at the end of the expansion line of the high-pressure cylinder card, where the high-pressure exhaust valve opens, there shall be no drop in it as there is in this. This engine, as shown by these cards at the low-pressure inlet valve, closed too late. In other words, there was not sufficient compression in the interspace to prevent a drop at the end of the high-pressure card. That point is determined by the relation of the interspace to the volume of the high-pressure cylinder, and that relation is a constant quantity, and therefore the point of closure of the low-pressure inlet valve is a constant point, no matter what the expansion is in the high-pressure cylinder; that is, I mean, for maximum theoretical economy. So far as equalizing the work done in the cylinders is concerned, I doubt if the gain in that respect would equal the loss which you would incur by allowing the drop to occur at the end of the high-pressure card, and therefore the initial drop of the low-pressure inlet.

Mr. See.—The engines of the four ships built at Philadelphia for the American Steamship Line had cut-off valves on both cylinders. Some of the engineers adjusted the cut-off on the high-pressure and others on the low. The economy of all the engines was about alike. It did not appear to make much difference

whether adjusted on the low-pressure or on the high. The difference in power was sometimes 50 per cent. greater in one than the other, but the economy of the machine was about the same in all cases. The adjustment of the cut-off on the low-pressure cylinder did not appear to make any difference in the economy of the engine.

Mr. Gale.—I would like to ask Mr. Coon what is the authority for the statement that there should not be a drop at the end of the high-pressure card? It is found in single-cylinder engines that the greatest economy is attained with a drop. It is a loss of economy to expand clear down to the mean back pressure, because, for one reason, when you get nearly down to back pressure you have simply effective pressure enough to overcome the friction of the engine. Expanding lower than that is a loss. It not only reduces the net power, but also increases the loss due to cylinder condensation. I see no reason why the demonstration of this principle for a single-cylinder engine does not apply equally well to each of the cylinders of a compound engine, provided they are correctly proportioned. If there are any experiments which prove that it is not so, of course that would settle the matter; but theory seems to indicate that the best result would be obtained when the terminal pressure is somewhat in excess of the mean back pressure; that is, in a receiver engine of the class described in this paper, when the high-pressure card shows a slight drop at the end of the expansion curve.

Mr. Coon.—As Prof. Gale has asked a question, I may be permitted to speak again. I would say my authority for making the statement is the fact that the engines that have been constructed on that principle have given the highest economy, and I am sorry that the data which I have in regard to that are of a confidential character. It was collected while I was connected with Mr. Leavitt, and it is the property of the Calumet & Hecla Mining Company. They have a very large amount of data on those points, which of course are of a private nature. But I am at liberty to say that they all indicate that the point which I make there is correct, that there should be no drop at the end of the high-pressure card when the steam is admitted at the end of the low-pressure cylinder.

Mr. Barrus.—I understood Mr. Coon to say that the amount of drop at the end of the stroke was proportionate to the size of the receiver.

Mr. Coon.—No, sir; I did not say that.

Mr. A. F. Nagle.—I would like to ask Mr. Barrus, who has had such a large experience in testing engines, what he thinks would be the saving in this particular engine if the steam cylinders had been steam-jacketed. For an unjacketed engine it is a very low water consumption, and I would like to know what he thinks would be the saving, accounting, of course, for the water contained in the jacket if it had been steam-jacketed.

Mr. Barrus.—The difference between the figures given by Mr. Coon for the case which he states, which was $14\frac{7}{10}$ pounds of water per horse-power per hour, and the quantity in this test, which was $16\frac{3}{10}$, is due largely in my opinion to the use of the jacket.

Mr. Nagle.—He did not measure the jacket water.

Mr. Barrus.—I know he did not; but that, as I understand, is included in the amount of feed water given. I have not very much data on the subject. I made a test on a high-pressure engine of small size in which the cylinder was jacketed, and the use of the jackets saved about 4 per cent. The difference between $16\frac{3}{10}$ and $14\frac{7}{10}$ would be about 10 per cent.

Mr. Nagle.—The water should have been measured in the jacket to make that of any value.

Mr. Barrus.—I should like to add to the discussion that the relative horse-power developed in the two cylinders in the case cited in the paper—or, rather, that the difference in power developed by the two cylinders—was about 78 horse-power. Supplementary tests were made to determine what the difference would be when the low-pressure cylinder was made to cut off at an earlier point. An earlier cut-off here causes an increase in the back pressure of the high-pressure cylinder, that is, the receiver pressure, and both cylinders are therefore affected by the change. The cut-off was shortened so as to increase the receiver pressure about 4 pounds, the original pressure being 6.4 pounds above the atmosphere, and the difference in power developed by the two cylinders was reduced to about 30 horse-power. No feed water test was made to determine the true economical effect produced, but the cards were measured for steam consumption, and these showed practically the same economy.

In reply to Mr. Coon's inquiry as to the cause of the peculiar form of the exhaust line of the high-pressure cylinder, a little study shows that this cylinder begins to exhaust about the time

the cut-off takes place in the low-pressure cylinder. Consequently the steam exhausts into a closed space and the pressure rises. The low-pressure begins to take steam about the centre of the high-pressure stroke. From the middle to the end of the return stroke, the high-pressure cylinder is therefore exhausting into a rapidly enlarging space, and the pressure falls.

The question raised by Mr. Cobb as to the effect of different loads on the form of the diagrams is well answered by Mr. Coon. Examination of the diagrams from the engine in question taken with various loads (which has been made since the meeting) shows that the only apparent effect produced by a change of load, besides the necessary change of cut-off in the high-pressure cylinder, was a change in the back pressure of the high-pressure cylinder, and a corresponding change in the initial pressure of the low-pressure cylinder, and the consequent new position taken by the steam end expansion lines. All the general features of the diagrams remained the same.

CCCIV.

THE DISTRIBUTION OF STEAM IN THE STRONG LOCOMOTIVE.

BY F. W. DEAN, CAMBRIDGEPORT, MASS.

(Member of the Society.)

THE writer desires to state at the outset that this paper is written at the request of the Secretary of this Society. He regrets that he has not had an opportunity to experiment exhaustively with the Strong locomotives on the Lehigh Valley Railroad (Nos. 383 and 444), but there is more or less propriety in his preparing a paper upon this subject, because he has had the opportunity of watching the development of the Strong locomotive from its inception; was the first person to make tests of efficiency in comparison with common locomotives by means of the indicator [only]; has taken several hundred cards from the locomotive No. 383, fitted with the Strong valve gear and a common boiler, and has ridden many hundreds of miles thereon, at different times, during two years. Through the kindness of Dr. E. D. Leavitt, Jr.,* he has access to data collected by Mr. J. S. Coon,* under the general supervision of Mr. Leavitt, of which some at the time of writing have been published, and many have not. He regrets that he has not been directly concerned with the collection of all the data to which reference will be made, but is pleased to notice that deductions from Mr. Coon's data confirm conclusions from his own experiments. An author must always regret that he is obliged to use other persons' experiences, but in this connection he wishes to express his high regard for Mr. Coon's accomplishments as an observer, and particularly for his skill as a manipulator of the steam engine indicator, about which there is so much incapacity in general.

In this paper reference will be made to seven locomotives, viz.: No. 444, Lehigh Valley Railroad, fitted with the Strong boiler, valve gear and cylinders; No. 383, Lehigh Valley Railroad, fitted

* Member of the Society.

with an ordinary boiler for burning anthracite coal, and the Strong valve gear and cylinders; No. 357, Lehigh Valley Railroad, fitted with an ordinary anthracite-burning boiler, and the Stephenson link motion and D valves, with De Lancy's balancing device; No. 158, Delaware, Lackawanna & Western Railroad, similar to No. 357 above; No. 169 Central R. R. of New Jersey, similar to No. 357; No. 129, Boston & Albany Railroad, a bituminous coal burner, with an ordinary boiler and Stephenson link motion and D valves; and finally a locomotive of the London, Brighton & South Coast Railway, England, fitted with an ordinary bituminous coal-burning boiler and a Stephenson valve gear, working directly on the valve stems, and D valves. But slight reference will be made to the last three.

Before proceeding to a consideration of the qualities and performances of the new locomotives, a brief description of their valve gears and cylinders is necessary, and here it is worthy of note that they form the only departure of importance since the days of the Stephensons and the "Rocket."

The new valve movement is originated by an eccentric for each cylinder on the driving axle, which eccentric has an ordinary strap and rod moving in a vertical plane and giving motion to the steam inlet valves, of which there are two to each cylinder. Alongside of this eccentric rod is another similar rod which is free to move independently in a vertical plane, its rear end being articulated with a pin passing through the strap at a point as near the eccentric as possible (Fig. 259). It is thus seen that one eccentric and its strap give motion to two systems of valve links, one being utilized for the steam inlet, and the other for the steam exhaust, the main eccentric rod actuating the inlet valves as above mentioned. The end of each rod farthest from the axle is connected by a link to the horizontal arm of a bell-crank rocker, the vertical arm of each being articulated to a rod which passes forward to the cylinders. From a point on each eccentric rod, intermediate between its ends, a connection passes vertically to a block, which, by means of a lever in the cab and a reach-rod for each block, can be moved back and forth on a quadrant. Thus the point of cut-off can be varied, the engine reversed, and the steam valves handled independently of the exhaust.

The intermediate point, before mentioned, on each eccentric rod, is so taken that when the sliding block is in mid-position the vertical vibration of the outer end of the rod is just sufficient to move

the valves in each direction the sum of the lap and lead, and the engine would go equally well in either direction. When the sliding block is moved either forward or backward, the inclined curved movement of the lower end of the vertical link attached thereto so modifies the quantity and quality of the motion of the outer end of the eccentric rod as to determine whether the engine shall go forward or backward. It also changes the point of cut-off, and, unfortunately, the port opening. The valve gear thus has the fault of all reversing constant motion valve gears, viz., to reduce the port opening when cutting off early. The gear possesses the important virtue, however, of permitting the exhaust ports to open fully at all times. The exhaust lever in the cab is always at one extreme end of the quadrant, and is never "hooked up" like the steam inlet lever, except slightly to change the compression. There are two notches close together on each end of the exhaust quadrant in the cab, and one in the middle.

It will be noticed that the motions of the bell cranks are derived from the vertical motions of the ends of the eccentric rods.

The valve gear can, of course, have any inclination to a horizontal line. The words "horizontal" and "vertical," as here employed, must be taken with due allowance, as they are used for convenience.

The rods which proceed forward from the vertical arms of the bell cranks impart motion to the cylinder valve gear, which is the most characteristic feature of the system. It might receive its motion from any of the well-known link motions, but its function is to convert an ordinary eccentric motion into one having important and elegant qualities. By means of it, a gridiron valve, having an enormous port area, moves with so slight a resistance that, without any balancing device, the levers are handled with ease, and the parts move without apparent effort—qualities possessed in general only by cam or trip-motion valve gears. (Plate IX., Figs. 245, 250.)

The steam valves, after closing and getting their lap, which they do with ease, as there is steam of high pressure on both sides of them, remain nearly quiet until they are partially balanced by compression, when they easily open. The exhaust valves, of course, close without resistance, and they remain closed until the steam has expanded nearly to the terminal pressure, when they, in turn, easily open. This peculiar motion is caused by a two-armed rocker and a short link in connection with the valve stem. An examination

of the diagrams annexed will make this matter clear. (Plates IX. to XII.)

The fact that these valves need no balancing device is much in favor of economical steam consumption, as there can be but little doubt that all balancing devices perform their function by steady or at least intermittent leaking, thus allowing steam to enter the cylinder without working expansively, and also allowing it to pass directly out of doors through the exhaust port. The writer has the best reasons for believing this to be true.

Continuing the description of the valve gear, the valves are vertically working flat plates of cast iron with numerous transverse ports separated by bridges having a form of least resistance, each port being $4\frac{5}{8}$ inches by $\frac{3}{4}$ inch, and for an 18-inch cylinder being nine in number. These valves slide upon seats similarly perforated. The seats are turned and fitted into bored enlargements of the steam passages to and from the cylinders, there being no steam chest in the ordinary sense. The seats are held in place by a single bolt in the bottom. The steam passages are remarkably free from changes in direction, and present slight obstruction to the flow of steam.

Any disturbance of the quality of the valve motion from the rise and fall of the axle is prevented by mounting that portion of the gear about the axle in a frame, which is carried by the axle box, and which therefore rises and falls with it.

It has been urged against the Strong valve gear that, as the travel of the valves is small, the accumulation of lost motions of the joints would seriously place the valves out of position, causing unequal cut-offs and lame exhausts. This, however, can never be urged by a person who has studied the valve gear, for it is not true. If there should be a total lost motion of $\frac{1}{16}$ inch from the axle to the outer joint of the rockers on the cylinders, this error would be reduced to $\frac{1}{32}$ inch when it reached the valves, by the mechanism above them, when in the most unfavorable position, and to 0 when in the most favorable,* which is a far better result than can be secured with the D valve.

An incidental advantage of the valve gear lies in the fact that the cut-offs and exhausts can be equalized, without moving the eccentrics, by simply adjusting the lengths of the rods connecting with the cylinder valve gears. This, of course, would somewhat affect the lead and port opening.

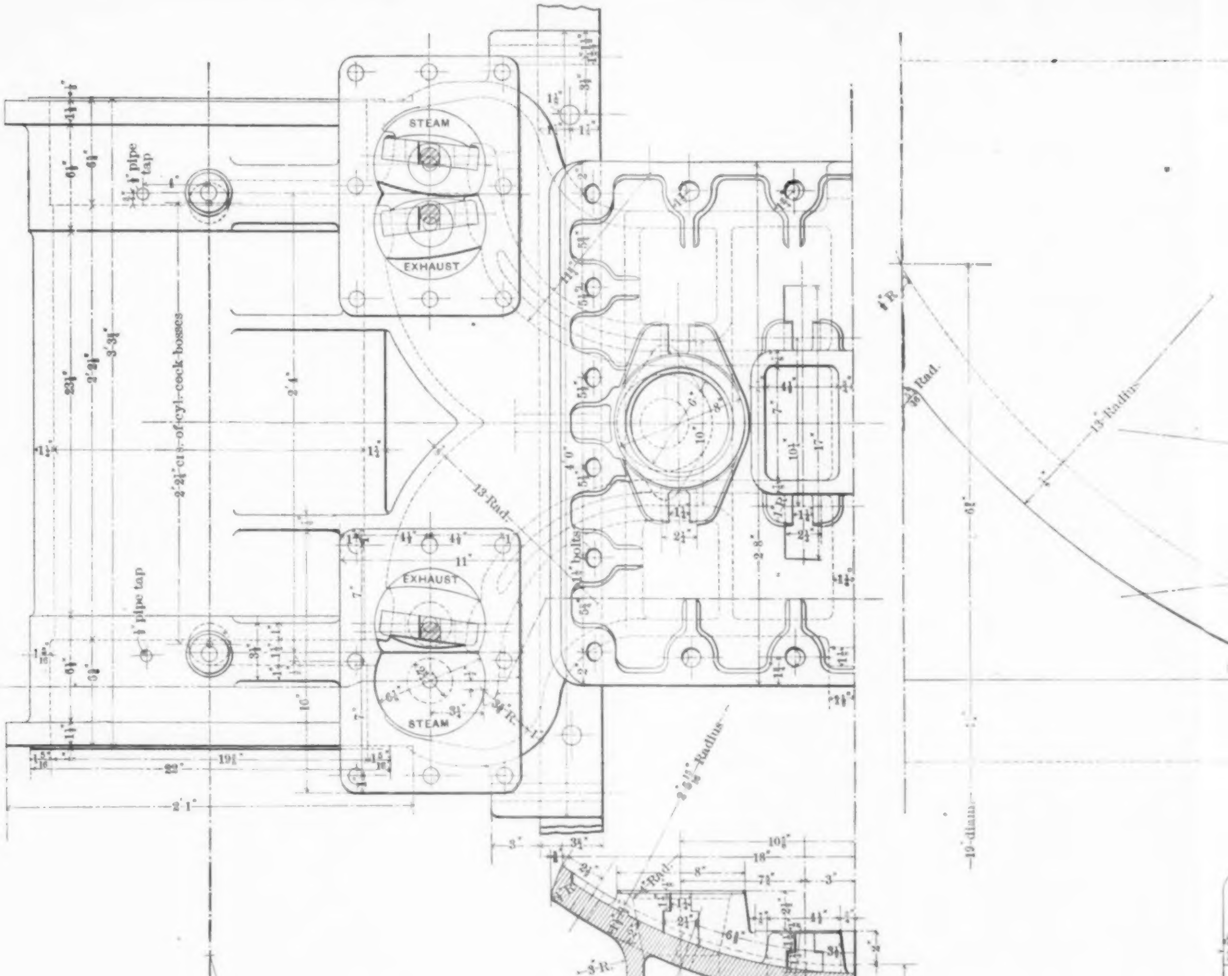
*** At the position of cutting off the error would be $\frac{1}{64}$ inch.**

We now properly come to comparisons between the new and ordinary (Stephenson link) valve gears, the latter working a D valve.

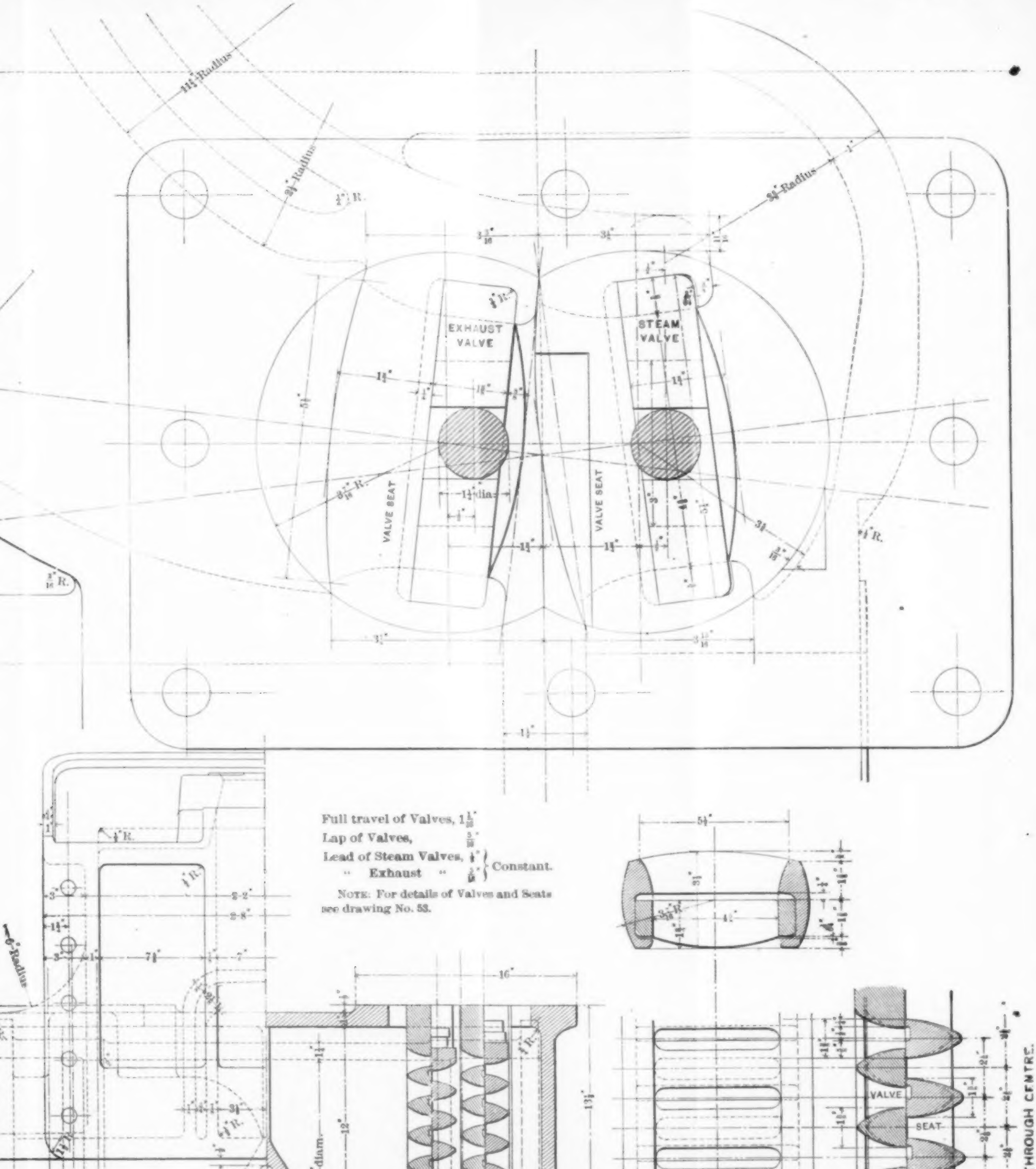
The new gear gives a constant lead to the steam and exhaust valves, while the other, with the usual length of eccentric rods, increases the lead from 0 to about $\frac{1}{4}$ inch. Among railway master mechanics there are many who believe that an increase of lead with an increase of speed is advantageous, because it assists in absorbing the increasing force of inertia of the reciprocating parts. Such a belief is doubtless confined to those persons who have had no experience with a constant-lead valve gear, and who have an exaggerated idea of the amount of the force of inertia referred to. They overlook the fact that since back pressure increases with an increase of speed, the steam trapped in by the closure of the exhaust is of a greater tension the greater the speed, and that therefore the resistance to the piston, due to compression, is increased. Thus the absorption of momentum is automatically accomplished by a gear with a constant lead and constant exhaust closure. The absorption of momentum will again be considered when discussing compression.

Except as a means of securing a large port opening in the early part of the piston stroke, lead is worse than useless on any engine having compression, particularly if the compression is adjustable, as is the case with the new engine. Belief in the need of an increase of lead with an increase of speed can quickly be dispelled by riding on engines Nos. 383 and 444.

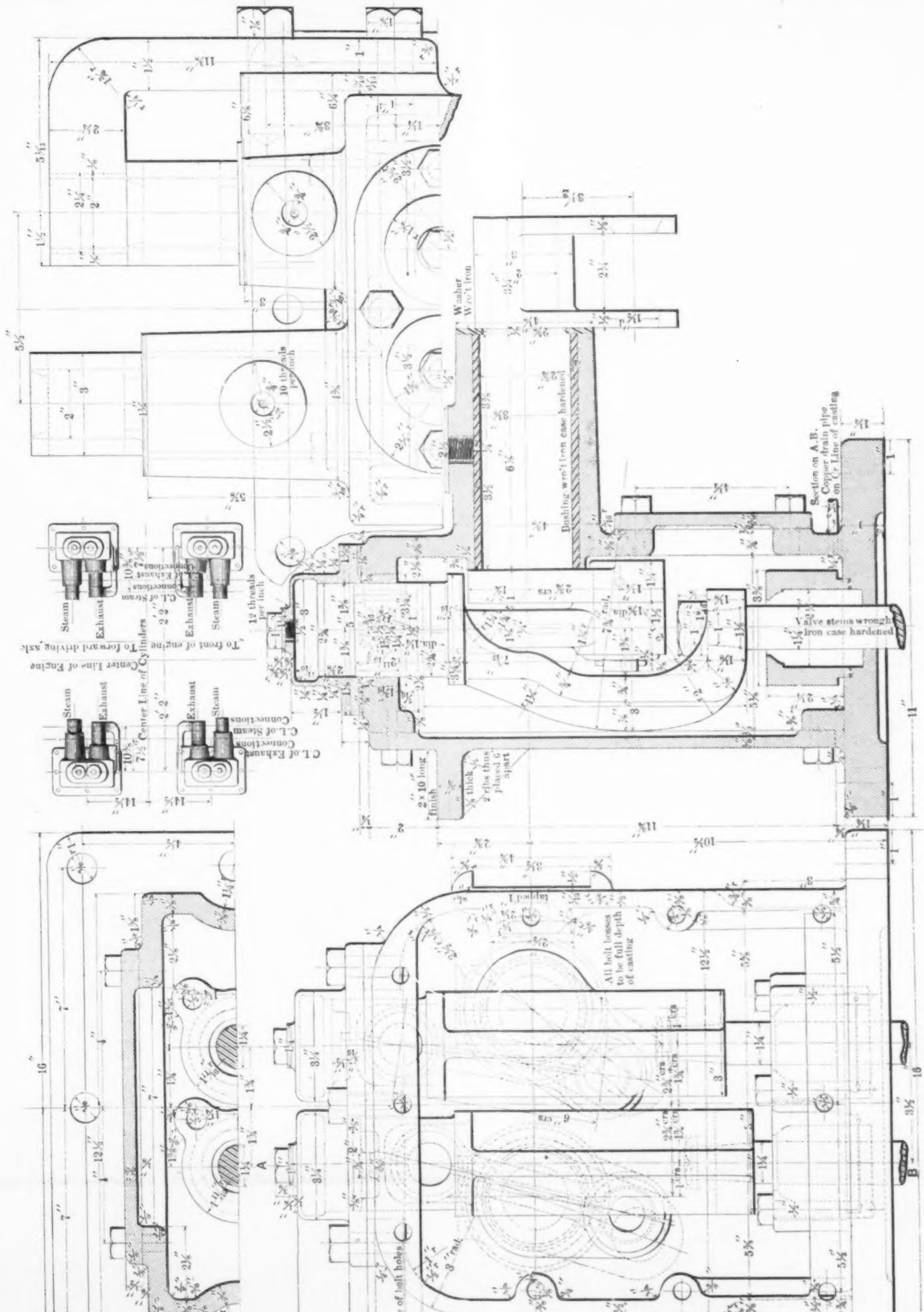
In the new locomotive the pre-release does not change with a change in the point of cut-off, and therefore in general with an increase of speed. In the ordinary locomotive earlier release with earlier cut-off is a fortunate property, in order that the exhaust can have time to escape through the contracted and tortuous passages, and thus enable the engine to attain a high speed; but with a valve gear, such as the new, which always opens the exhaust ports wide, this, as well as much pre-release, is unnecessary. With the new gear the steam is expanded to the last inch of stroke of the piston, notwithstanding which the pressure falls quickly, and in general the back pressure is some two-thirds or three-quarters that of the link-motion engine, or even less. The writer has known it to be but one-half that of a link motion engine of the same size as the Strong engine, No. 383, when working under almost identical conditions with the latter. These favorable results have not, how-



CYLINDER FOR EXPRE



XPRESS LOCOMOTIVE.



CYLINDER ROCKER BOX.

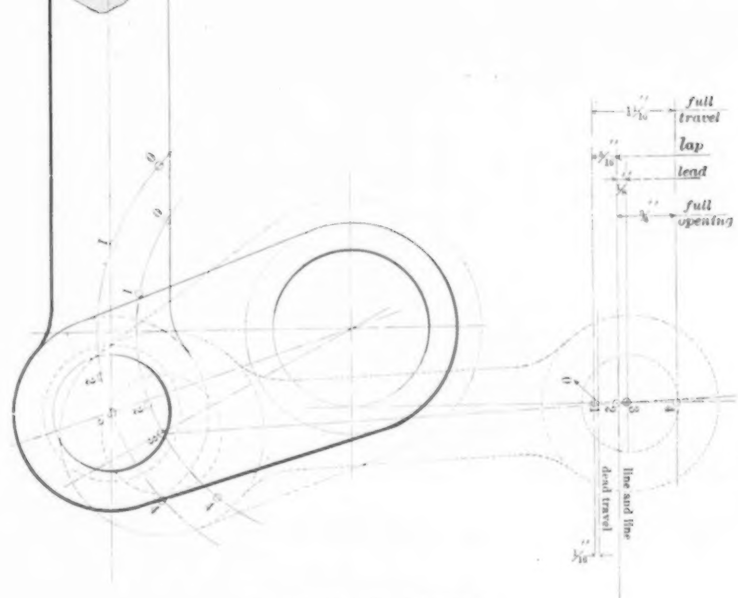
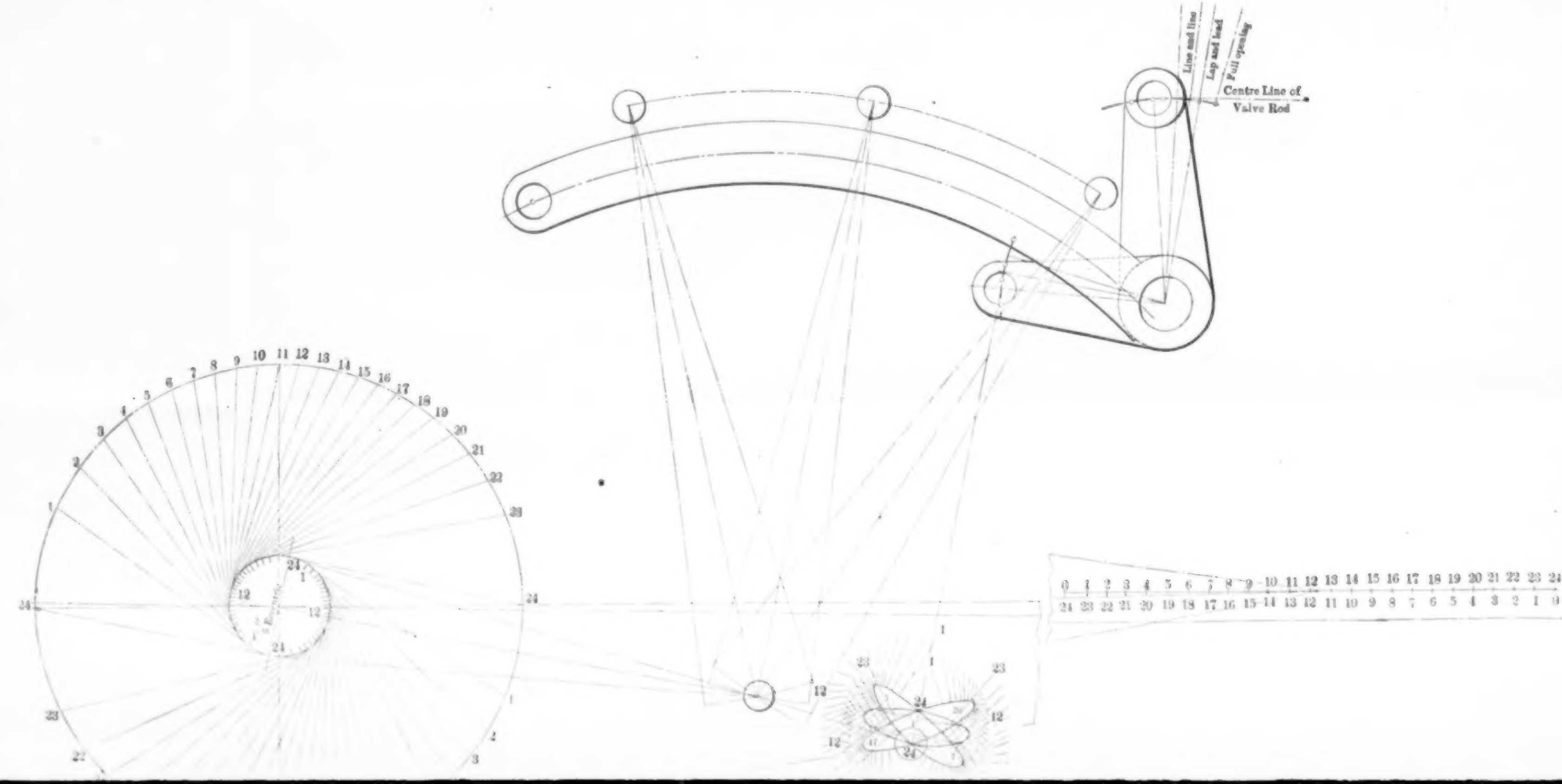


DIAGRAM OF ROCKER.



F. W.

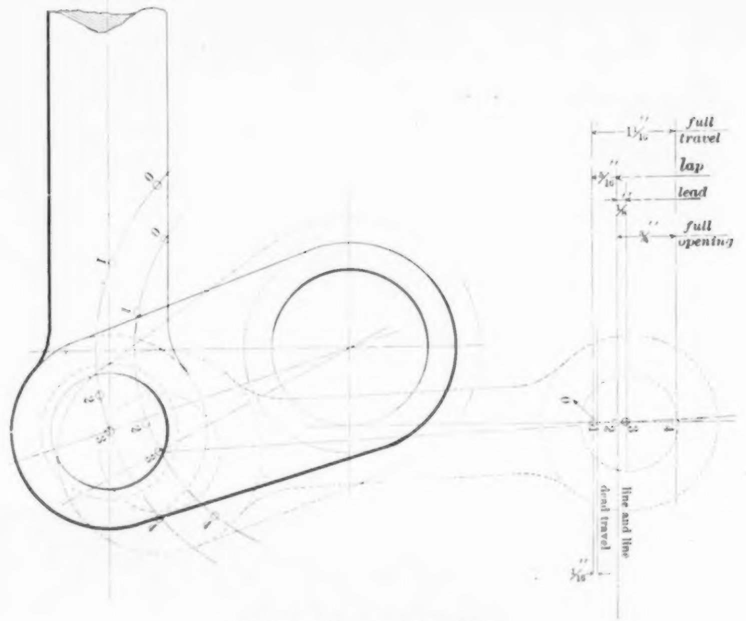


DIAGRAM OF ROCKER.

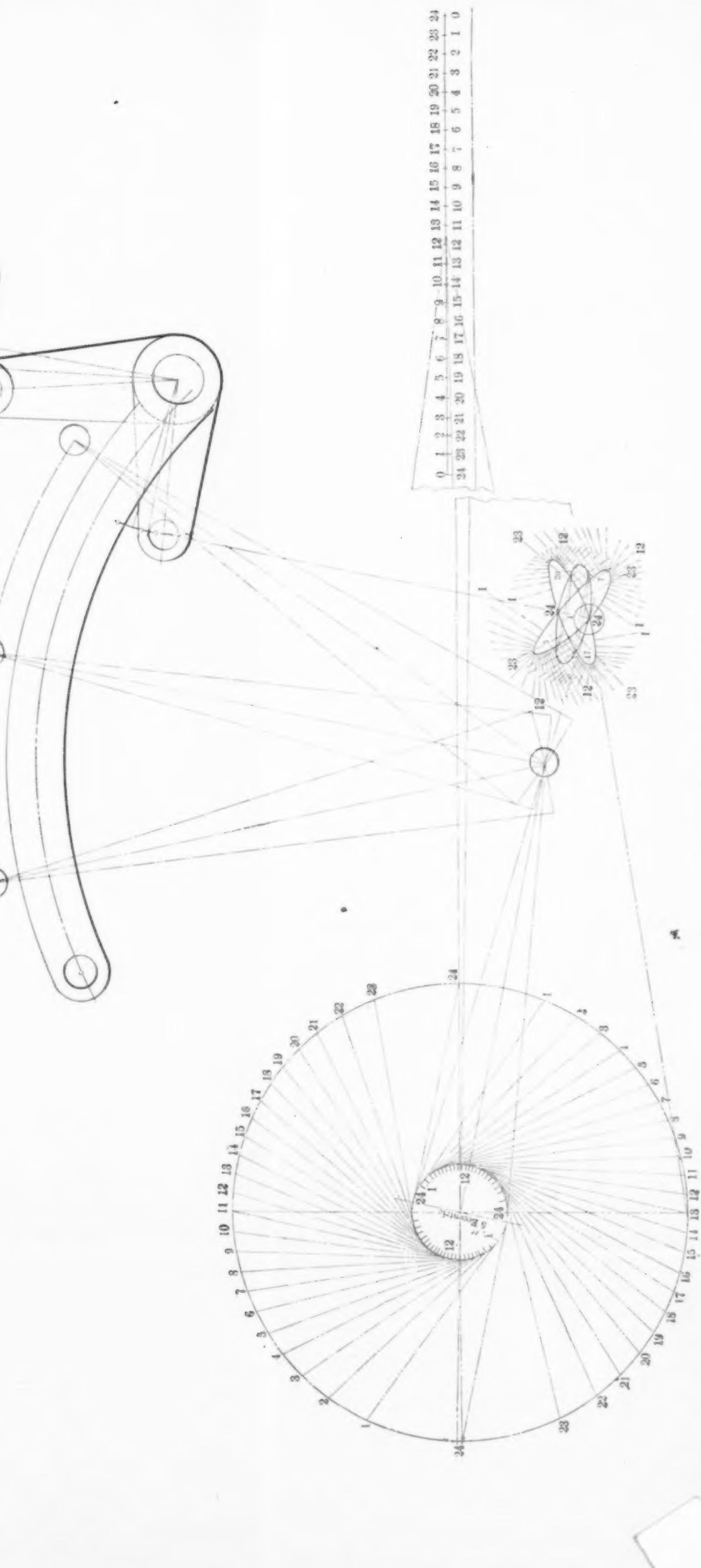
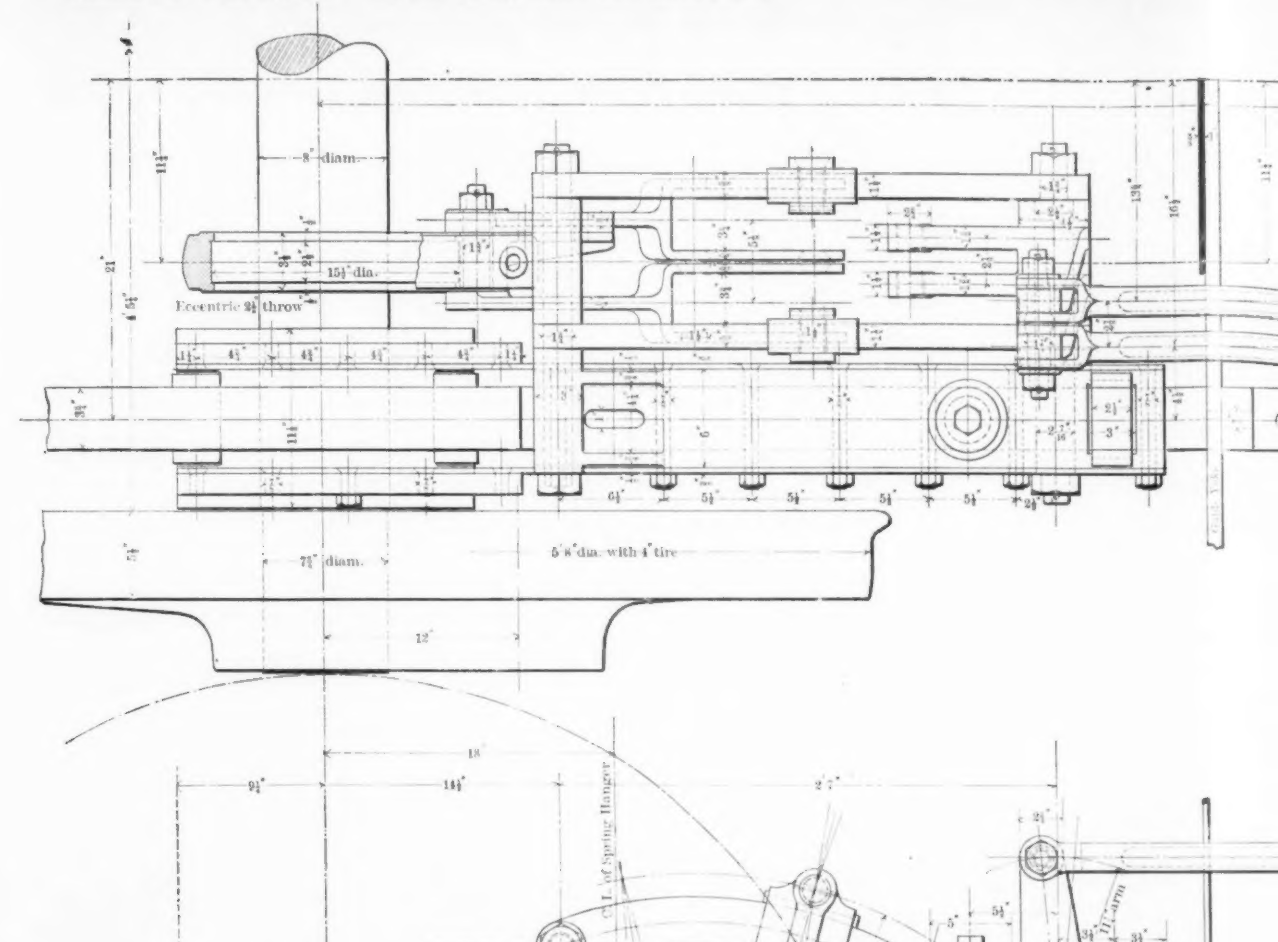
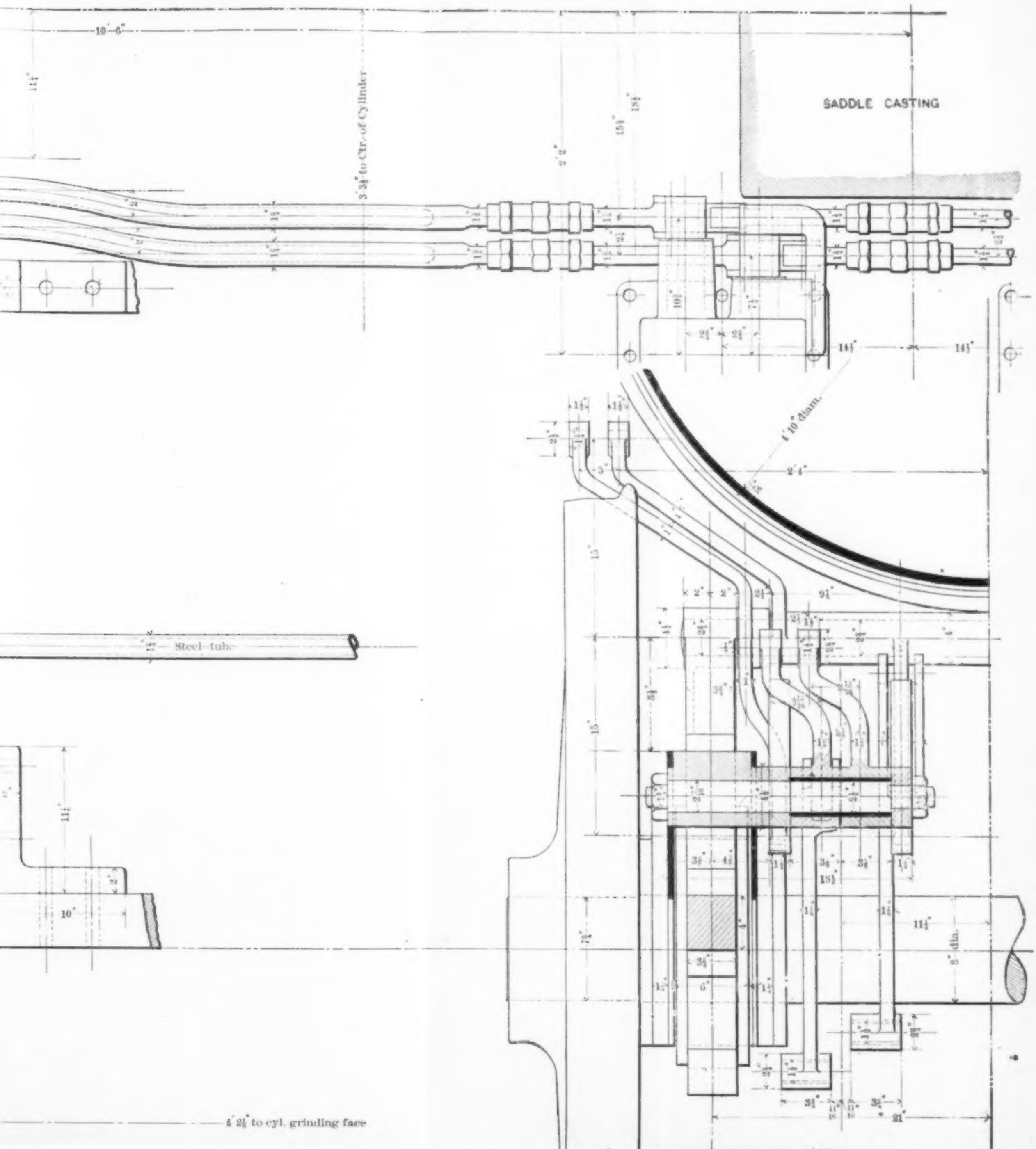


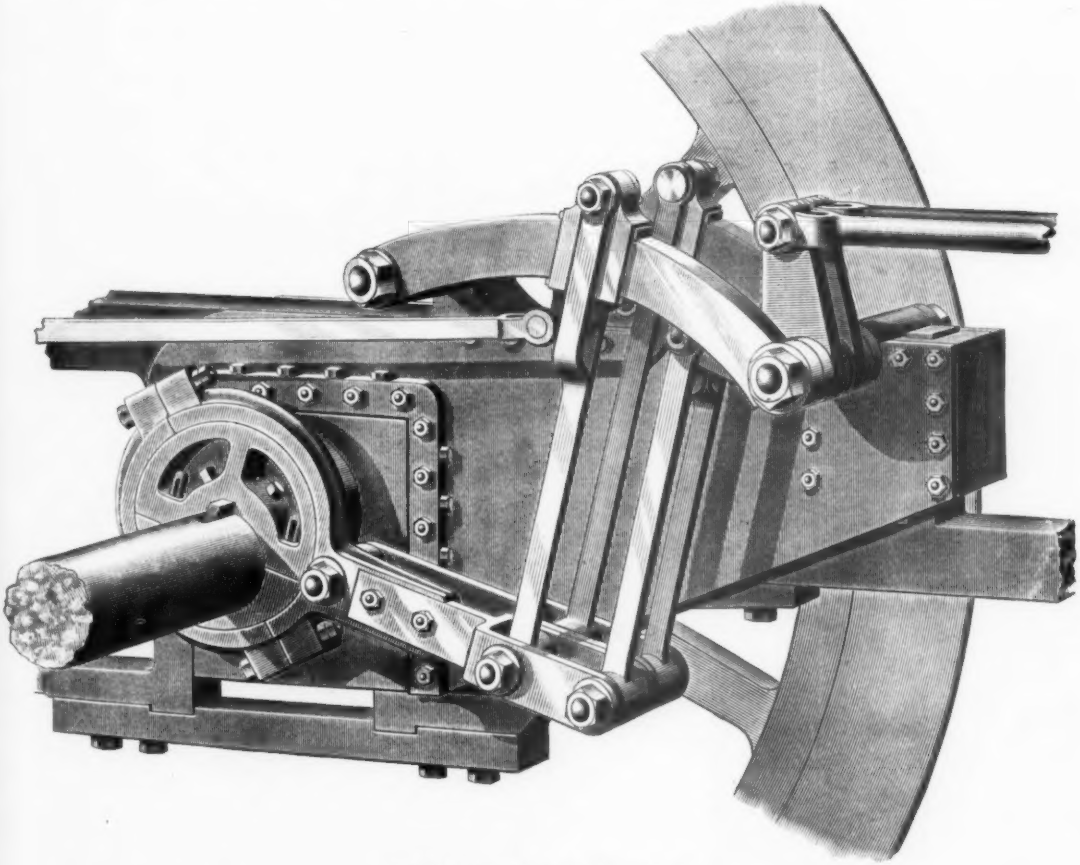
DIAGRAM OF VALVE GEAR.



VALVE GEAR FOR EXP



EXPRESS LOCOMOTIVE.



LOCOMOTIVE VALVE GEAR.

ever, been realized from engine No. 444, but those obtained thus far are superior when the speed and quantity of steam to be exhausted, or in other words when the work done, is considered. For example, locomotive No. 169, Central R. R. of N. J., when making 320 revolutions per minute, showed 20 lbs. of back pressure, while No. 444 at 326 revolutions showed a pound or two less. At these times No. 169 was cutting off at six inches, and showed an initial pressure of 19 lbs. below boiler pressure, while No. 444 was cutting off about two inches later, and showed an initial pressure some 4 lbs. only below boiler pressure, which itself was 30 lbs. above that of No. 169. This is a creditable showing. No. 169 has shown a back pressure of 23 lbs. when cutting off at 10 inches.

Again, locomotive No. 158, D., L. & W. R. R., made 181 revolutions, cut off at about 10 in., and showed 17 lbs. of back pressure, while the boiler pressure was 135 lbs., and the initial pressure 128 lbs., and the pre-release some 15 per cent. of the stroke. It is claimed by persons specially interested in locomotive No. 444 that its large back pressure was due to an unnecessarily small exhaust nozzle which could not be changed under the circumstances. It should not be forgotten that the nozzle was that used for mountain work, while the run in question was on a level, and very fast. Be this as it may, the engine needs no excuse from its friends.

Whoever looks for small back pressure from a locomotive when running fast and developing great power, is doomed to disappointment, no matter what type of valve gear is used, for back pressure must always be the means of creating draft. No slight amount of energy is needed for this purpose, when an engine is indicating from 600 to 1,810 horse power, the latter being the greatest power ever developed by a locomotive.

Back pressure automatically adjusts itself to the needs of the occasion in the most admirable manner. It may be reduced by greater perfection of details, but will never vanish until the engine is motionless.

In the new locomotive the point of exhaust closure does not change with a change in the point of cut-off. It is easy to show that comparatively slight compression at ordinary speeds is sufficient to arrest the motion of the piston, piston rod, crosshead, and connecting rod. When the crank pin of a 24-inch stroke engine is passing the dead point at 300 revolutions per minute, the normal acceleration of the pin is 986 ft. per second, and if the parts just enumerated should weigh 946 lbs. the force of inertia (centrifugal force) is

$986 \frac{W}{g} = 28{,}948$ lbs., which would be equaled by a pressure of 103 lbs. per square inch on a 19-inch piston. This can easily be realized by compression, and the computation shows how entirely unnecessary lead is, except for the reason previously stated, even at the highest speeds.

Excessive compression would probably never lift an unbalanced D valve from its seat, but a balanced valve might be so lifted, in which case steam would escape from the chest into the atmosphere. In the case of the new valve gear, in common with all 4-valve engines, the exhaust valves can never be forced from their seats, but are all the more firmly seated by compression. The inlet valves only could lift, in which case the steam would pass back to the boiler and be saved. This feature may be a source of considerable economy in the new locomotive.

As the valves of the new gear move vertically, they do not wear upon the seats when the engine is running without steam.

With a 19-inch Strong locomotive the port area is $31\frac{2}{10}$ square inches, or 11 per cent. of the piston area, while a 19-inch D valve engine ordinarily has about $7\frac{1}{2}$ per cent. of port area, or the former is nearly $1\frac{1}{2}$ times the latter. Notwithstanding this, the clearance volume of the new cylinder is only 87 per cent. of that of the old one, being respectively 6.38 and 7.35 per cent. of the piston displacements.

The port edges compare in length as $2\frac{1}{3}$ to 1 in favor of the new engine, which is very advantageous. The areas of rubbing surfaces of the valves are about as 2 to 1 in favor of the new device. We should consequently expect the gridiron valves to wear well and to keep tight longer than the D valve—a result which has been realized in stationary practice. The writer is familiar with gridiron valves in stationary engines which have been kept tight for so many years (16) without being faced, that he feels warranted in concluding that they will never leak. Being long and thin, they can bend over a valve seat which has been curved by expansion and contraction.

In the new engine the exhaust passages are much more direct to the chimney than in the common engine.

Many advantages of the new valve gear have been dwelt upon, but there is still remaining to be considered one of the greatest importance, viz.: the capacity of the Strong cylinder to perform work. The writer regrets that he has not in his possession many

cards from the old and new types taken under nearly identical conditions, but fortunately he has enough such to enable him to draw remarkable conclusions.

Cards X. (Fig. 173), are moderate speed cards, and their areas are, when reduced to the same scale, as follows: 2.57 and 1.98 or 1.3 to 1. The speeds and initial pressures are the same, and the points of cut-off, being at 9 in. for the Strong engine and at $10\frac{1}{3}$ in. for the link-motion engine, are more favorable for the latter. For cards at the faster speed of 44 miles per hour, the areas are as 1.92 to 1.50, being 1.3 to 1 in favor of the new engine. This is equivalent to saying that at 44 miles per hour a Strong cylinder $16\frac{1}{2}$ inches in diameter can do as much work as an ordinary cylinder of 19 inches in diameter. As, however, at this speed the initial cylinder pressure of the link-motion engine is usually some 10 or 12 pounds below boiler pressure, while that of the other is only some 4 pounds below, the disparity is even greater, and may be $1\frac{1}{2}$ to 1. At a speed of 50 miles an hour, the cards are like Fig. Z (Fig. 181), the ratio being 1.35 to 1. In this respect, viz., cylinder capacity to perform work, the advantage of the new engine is more apparent the higher the speed, and it would not be surprising if the ratio reached 1.7 to 1 at 60 to 65 miles per hour, which is equivalent to saying that in that case a 14-inch Strong cylinder would equal an 18-inch common cylinder, or an 18-inch Strong cylinder would equal a $23\frac{1}{2}$-inch common cylinder. The cards presented as evidence of the truth of these claims cannot be regarded as conclusive with reference to the *particular numerical* quantities given, because no cards can be found in the writer's collection of several hundreds, which were taken under identically the same conditions. It cannot be stated how much of the effect is due to size of throttle valve or steam pipe, and how much to the inherent qualities of the valve gear. Great pains were taken in selecting the cards for comparison, and in each case it is believed that the link-motion engine has received the more favorable treatment, either as to position of throttle, point of cut-off or speed, so that the claim is understated. With reference to the claim at 60 miles per hour, viz., 1.7 to 1, the writer has no cards from either class of engines, at present, taken at that speed. The claim is based upon a careful study of early cut-off cards at high speeds, at different times. Everybody knows that the typical link-motion card under these conditions is a thin crescent, while the Strong

card is never such. See card No. 32 (Fig. 202). This superiority of the new engine is not wholly due to the new valve gear *per se*, but to easy, large steam passages, the facility for making which the gear offers to the designer.

If these conclusions are correct, and they cannot be gainsaid, this feature alone, independent of economy in coal and water consumption, should place the Strong or some similar locomotive, even if provided with an ordinary boiler, if it could furnish the needed steam, far in advance of the common type of locomotive for conducting a heavy fast passenger or freight traffic; and the results are due, as already implied, to the high initial pressure, the fuller steam line, the late release, the low back pressure and the late compression.

Such qualities enabled engine No. 444 to draw on a level twelve cars weighing 740,000 pounds, or, with engine and tender, 952,000 pounds, from a start to a dead stop ten and eight-tenths miles ($10\frac{8}{10}$ miles) in eleven (11) minutes. (See the statement of Supt. M. C. Kimberly, Northern Pacific Railway.)

Having enumerated, in considerable detail, the advantages of the new valve gear over the common type, some results obtained in actual service will be noted.

Figs. 146 to 153 shows a number of cards taken (except No. 6) by the writer from engine No. 383 on the Lehigh Valley R. R. on October 28, 1885. Cards Nos. 1 to 5 inclusive were taken on the 12 M. train from Mauch Chunk to Wilkesbarre, with the throttle only four-sevenths open, and consequently the initial pressures are much below boiler pressures. They were taken with great care, and with the unusual arrangement of the indicator placed at one end of the cylinder, with the shortest possible connection to the cylinder, which the writer regards as the only means of really securing accurate results. The cards were fully as smooth as shown, and were almost entirely free from signs of a sticky indicator. They do not show the engine at her best, because the engineer could not be induced to run with a full throttle. The steam line is fairly full, considering the throttling, and the card is well filled out at the compression line, which is a valuable feature, because it points to great cylinder capacity. The back pressure does not rise above 7 pounds at its lowest point. The average consumption of dry saturated steam is $21\frac{6}{10}$ pounds per I.H.P. per hour. Figs. 151 to 153 are merely illustrative cards.

In Figs. 154 to 169 cards are shown, taken by the writer from

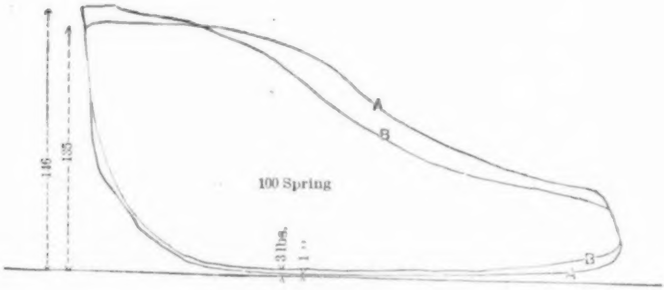


FIG. 146.

(1 *A*)

Steam, 155.
Revs. 86 = 17 m. per hour.
M. E. P., 91.2.
I. H. P., 539.

(1 *B*)

Steam, 155.
Revs., 112.
Miles per hour, 23.
M. E. P., 78.
I. H. P., 600.
Water by card, 21 lbs. per I. H. P. per hour.
Load, 9 cars = 463,000 lbs.
Grade, 9 feet per mile.
Spring, 100.

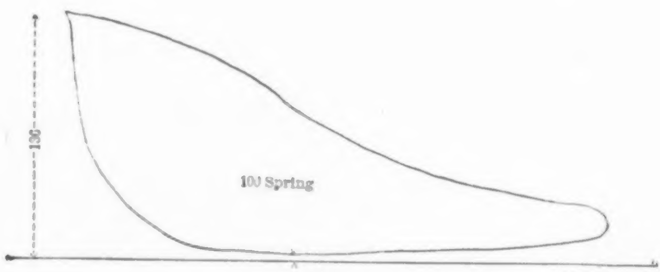


FIG. 147.

(2)

Steam, 145.
Revs., 115.
Miles per hour, 23.
M. E. P., 64.2.
I. H. P., 508.
Load, 9 cars = 453,000 lbs.
Water by card, $20\frac{1}{2}$ lbs. per I. H. P. per hour.
Grade, 9 feet per mile.
Cut off, 8 inches.
Throttle 4–7 open.
Spring, 100.
(One car switched off.)

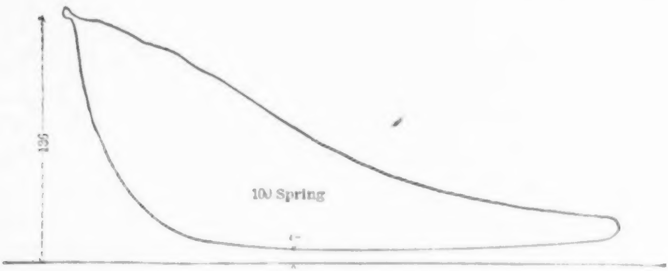


FIG. 148.

(3.)

Throttle 4-7 open.
Steam, 150.
Revs. per min., 200.
Miles per hour, 41.

M. E. P., 53.
I. H. P., 725.
Water by card, 22 lbs. per I. H. P. per hour.
Load, 8 cars = 410,000 lbs.
Grade, 34 ft. per mile.

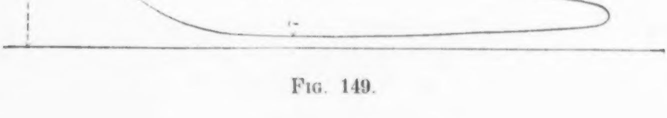


FIG. 149.

(4)

Throttle 4-7 open.
Steam, 145.
Revs., 175 per min.
Miles per hour, 36.
M. E. P., 53.3.

I. H. P., 633.
Water by card, 21 lbs. per I. H. P. per hour.
Load, 8 cars.
Grade, 34 ft. per mile.

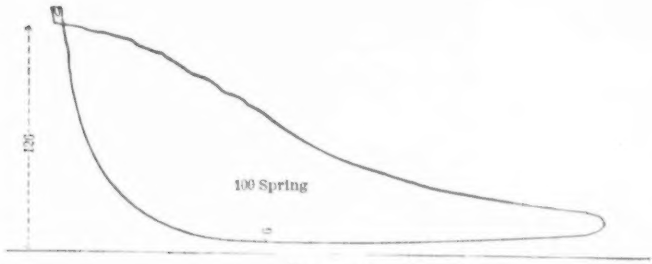


FIG. 150.

(5)

Steam, —.
Revs. per min., 175.
Miles per hour, 35.
M. E. P., $49\frac{1}{2}$.
I. H. P., 595.
Water by card, $22\frac{1}{2}$ lbs. per I. H. P. per hour.
Load, 8 cars.
Grade, 34 ft. per mile.
Throttle 4-7 open.
Spring, 100.

Conditions unknown. 80 spring.

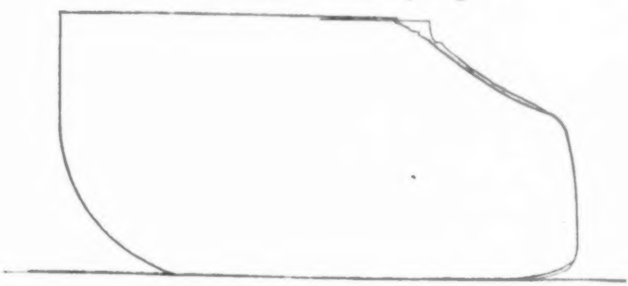


FIG. 151.

About 80 Revs. 100 spring.
Taken on siding near Round house, after trip.

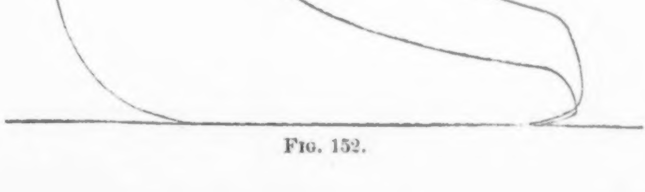


FIG. 152.

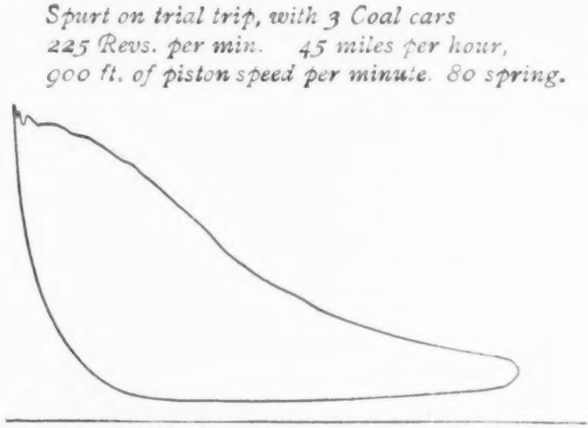


Fig. 153.

1
Time, 4.52 p.m.
Steam (?)
Revs., 86.
Throttle, 5.
I. H. P., 573.
Cut off, 12½ in.
Grade, 75.5 ft. per mile.
Water, 25 lbs.

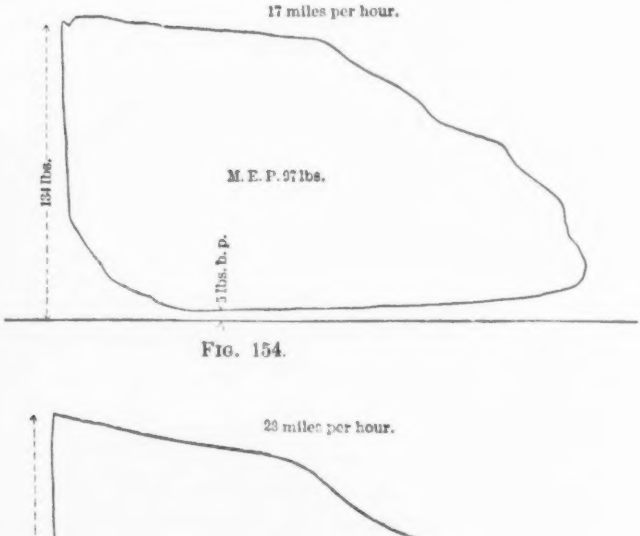


Fig. 154.

2
4.55 p.m.
Steam, 135.
Revs., 116.
Throttle, 8.
I. H. P., 666.
Cut off (?)
Grade, 75.5.
Water, 24.6 lbs.

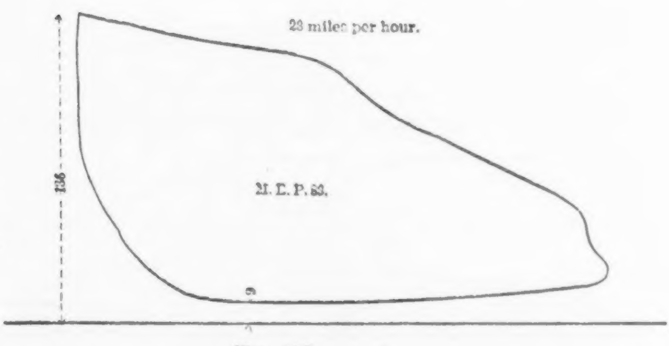


Fig. 155.

3
4.57 p.m.
Steam, 137.
Revs., 130.
Throttle, 8.
I. H. P., 739.
Cut off (?)
Grade, 75.5 ft.
Water, 25.1 lbs.

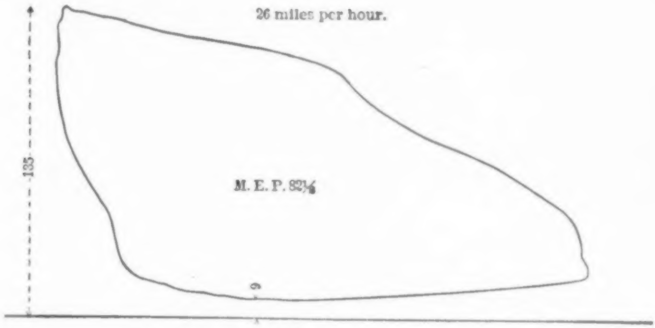


FIG. 156.

4
5 p.m.
Steam, 137.
Revs., 132.
Throttle, 8.
I. H. P., 692.
Cut off (?)
Grade, 75.5 ft.
Water, 26.2 lbs.

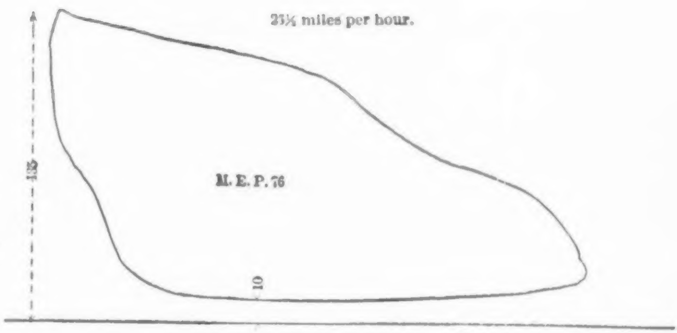


FIG. 157.

5
5.04 p.m.
Steam, 137.
Revs., 138.
Throttle, 8.
I. H. P., 746.
Cut off (?)
Grade, 53.3 ft.
Water, 25.5 lbs.

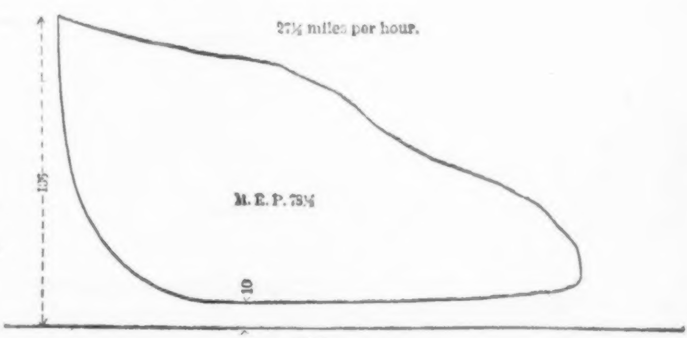


FIG. 158.

6
5.10 p.m.
Steam, 137.
Revs., 178.
Throttle, 8.
I. H. P., 824.
Cut off (?)
Grade, 53.3 ft.
Water, 24.8 lbs.

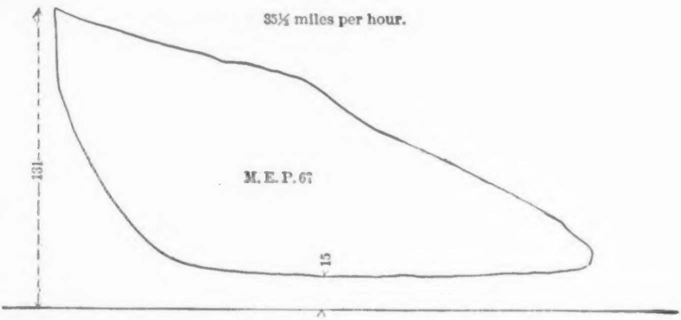


FIG. 159.

7
5.12 p.m.
Steam, 135.
Revs., 166.
Throttle, 8.
I. H. P., 739.
Cut-off (?)
Grade, 53.3 ft.
Water, 25 lbs.

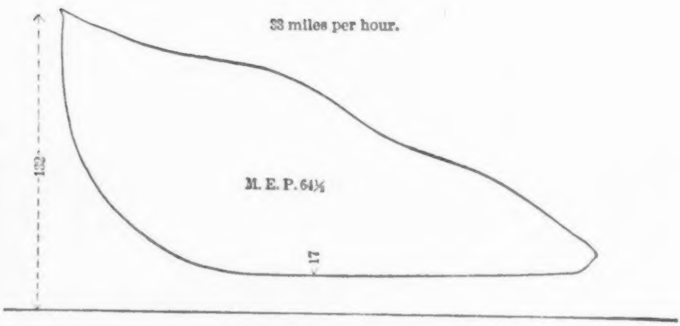


FIG. 160.

8
5.15 p.m.
Steam, 137.
Revs., 181.
Throttle, 8.
I, H. S., 786.
Cut off (?)
Grade, 53.3 ft.
Water, 25.2 lbs.

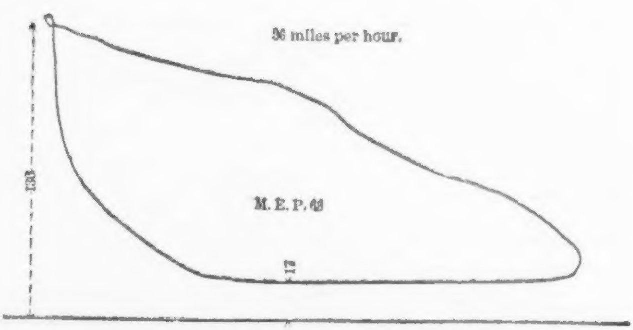


FIG. 161.

9
5.17 p.m.
Steam, 135.
Revs., 181.
Throttle, 8.
I. H. P., 799.
Cut off (?)
Grade, 53.3 ft.
Water, 24.5 lbs.

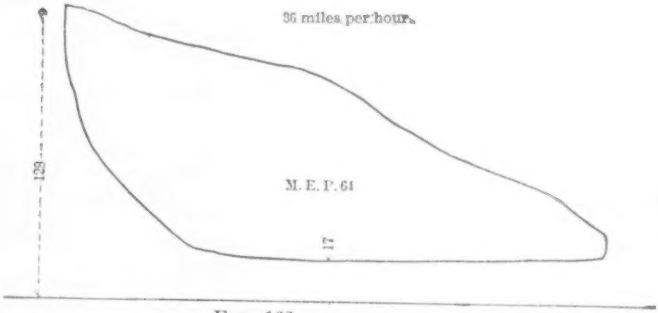


FIG. 162.

10
5.21 p.m.
Steam, 132.
Revs., 171 (?)
Throttle, 8.
I. H. P., 719.
Cut off (?)
Grade, 53.3 ft.
Water, 25.5. lbs

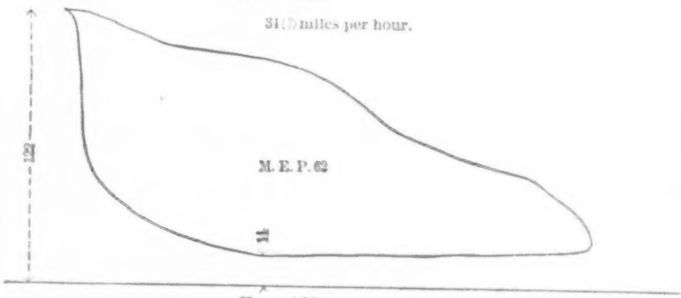


FIG. 163.

11
5.27 p.m.
Steam, 128.
Revs., 209.
Throttle, 8.
I. H. P., 678.
Cut off (?)
Grade, 13.7 ft.
Water, 28 lbs.

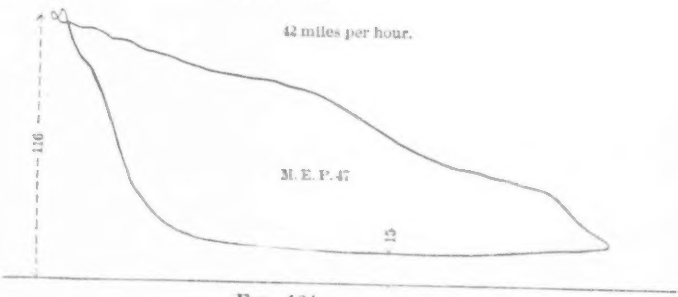


FIG. 164.

12
5.30 p.m.
Steam, 125.
Revs., 232.
Throttle, 8.
I. H. P., 656.
Cut off (?)
Grade, 25.3 ft.
Water, 24.5 lbs.

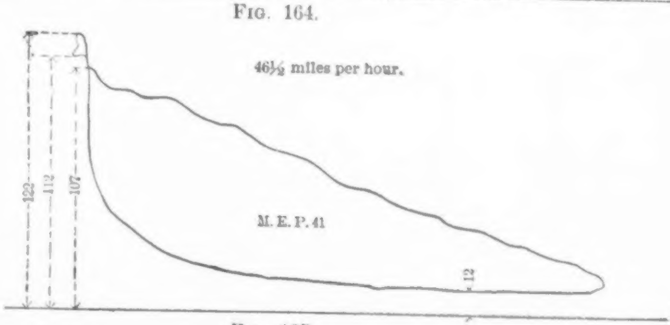


FIG. 165.

13
5.35 p.m.
Steam, 130.
Revs., 240.
Throttle, 8.
I. H. P., 662.
Cut off (?)
Grade, 24.6 ft.
Water, 26 lbs.

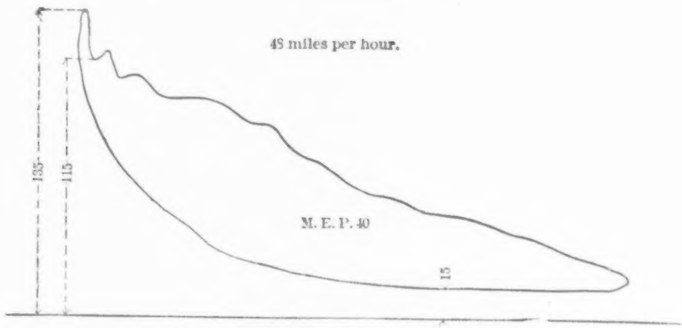


FIG. 166.

14
5.37 p.m.
Steam, 130.
Revs., 208.
Throttle, 8.
I. H. P., 603.
Cut off, 11 ins.
Grade, 24.6 ft. up.
Water, 23.2 lbs.

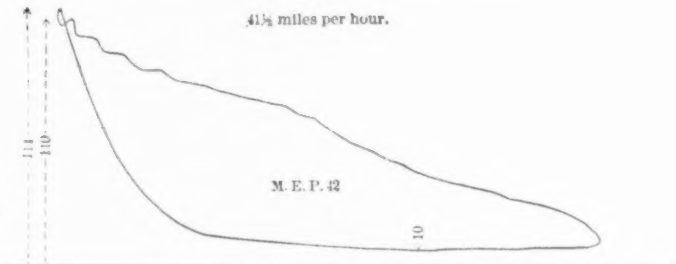


FIG. 167.

15
6.20 p.m. (?)
Steam, 135.
Revs. 263, or 53 miles pr. hour.
Throttle, 8 or 10.
I. H. P., 635.
Cut off (?)
Grade, 17.4 down.

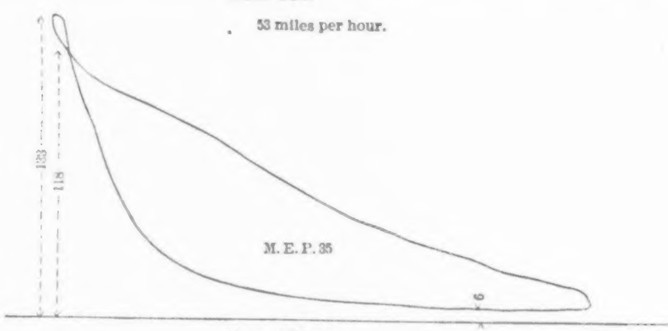


FIG. 168.

16
6.26 p.m. (?)
Steam, 135.
Revs. 275, (?) or 55 miles p. hour.
Throttle wide open.
I. H. P., 533.
Cut off (?)
Grade, 27.4 down.

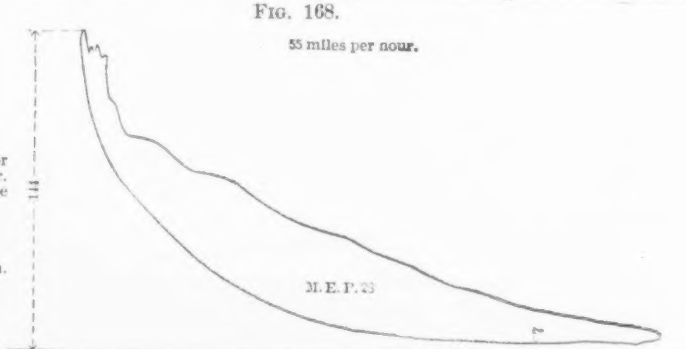
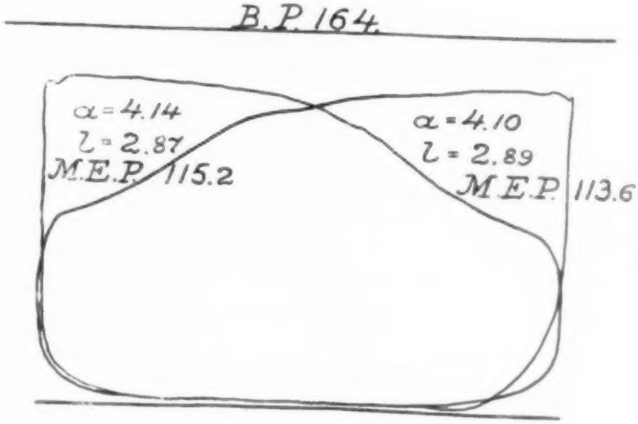
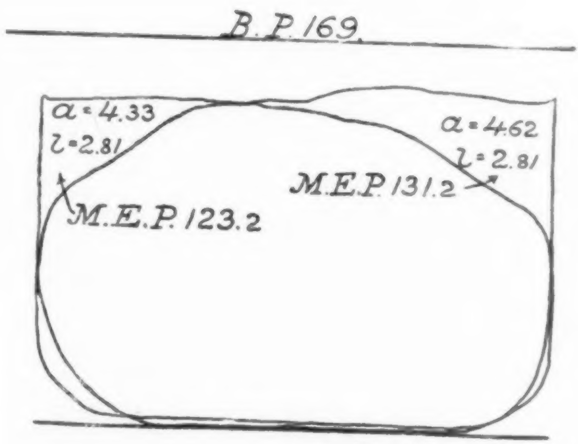


FIG. 169.



383
May 2, 1887.
12.
R. L., 4.
T. L., 9 = $\frac{9}{15}$ open

FIG. 170.



383.
May 3, 1887.
3.
R. L., 1.
T. L. 9 = $\frac{3}{5}$ open.

FIG. 171.

383
May 10, 1887.
23.
R. L., 8.
T. L., 12=$\frac{4}{5}$ open.
Speed about 30 miles per hour or 150 revs.

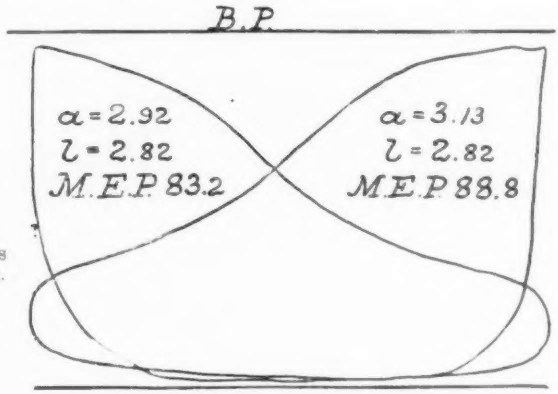


FIG. 172.

357
May 9, 1887.
27.
R. L., 8.
Throttle $\frac{9}{10}$ open.
Speed about 36 miles per hour or 180 revs.

383
May 3, 1887.
8.
R. L., 8.
T. L., 10=$\frac{2}{3}$ open.
Speed about 34 miles per hour or 170 revs.

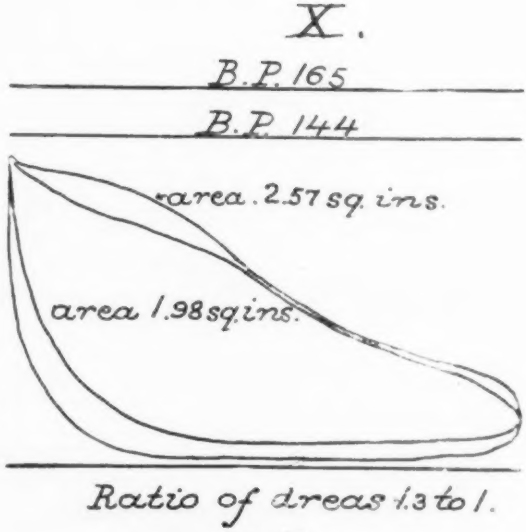
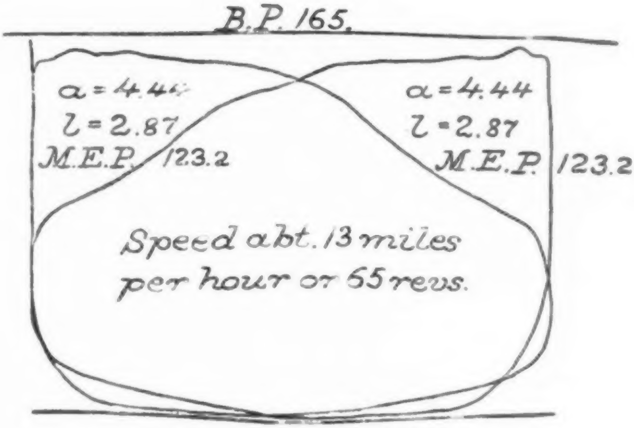
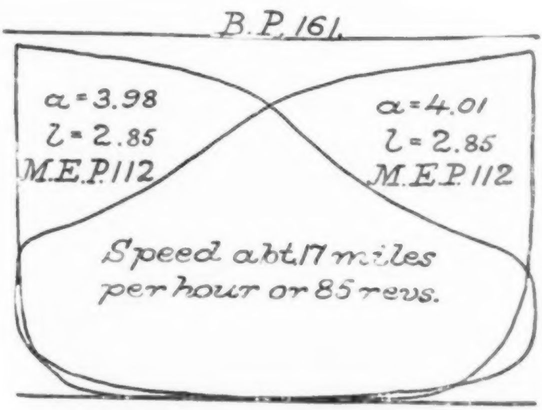


FIG. 173.



383½
May 2, 1887.
3.
R L., 4.
T. L., 15 = full.

FIG. 174.



383
May 10, 1887.
9.
R. L., 6.
T. L., 13 = $\frac{2}{3}$ open.

FIG. 175.

383
May 10, 1887.
31.
R. L., 10.
T. L., 5 = $\frac{1}{3}$ open.
Speed about 37 miles
per hour or 190 revs.

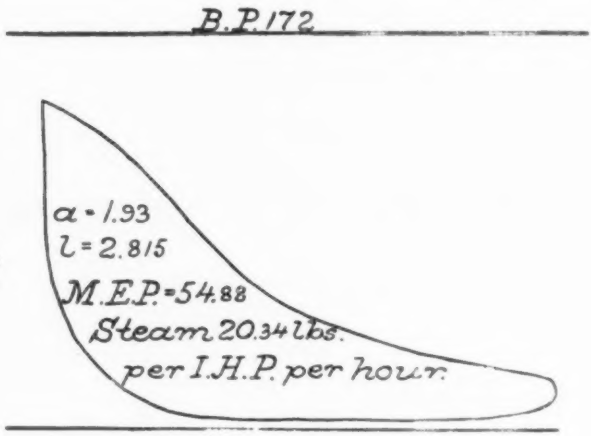


FIG. 176.

Cards from
383 and 357.
Throttle and B. P.
unknown.
Speed about 44 miles
per hour or 220 revs.

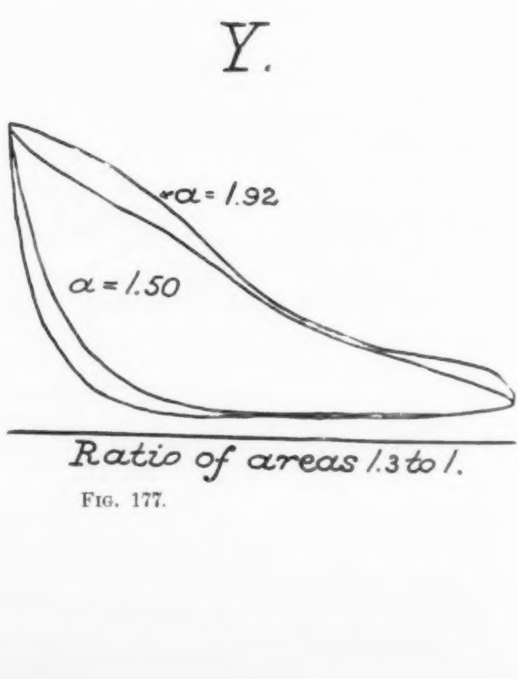


FIG. 177.

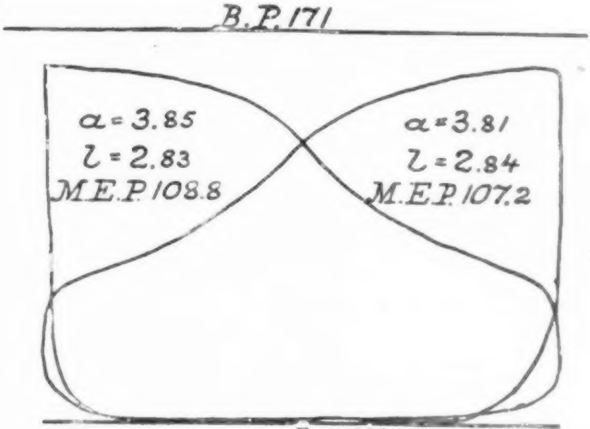


FIG. 178.

383
May 2, 1887.
44.
R. L., 10.
T. L., 8 = $\frac{3}{15}$ open.

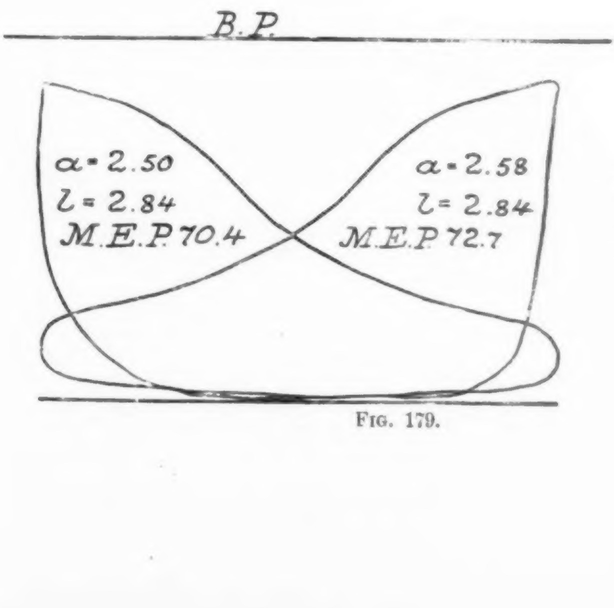


FIG. 179.

383
May 10, 1887.
39.
R. L., 10.
T. L., 5.

383
May 10, 1887.
R. L., 10.
T. L., 10=$\frac{3}{8}$ open.
Speed about 37 miles per hour, or 185 revs.

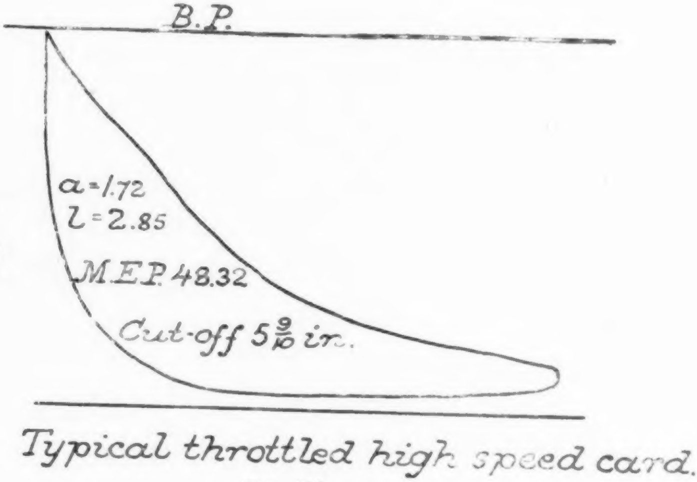


FIG. 180.

383
May 2, 1887.
31.
Throttle $\frac{5}{8}$ open.
Speed about 50 miles per hour, or 250 revs.

B. & A. R. R. Engine 129.
July 26, 1882.
Throttle unknown.
212 revs. per minute.

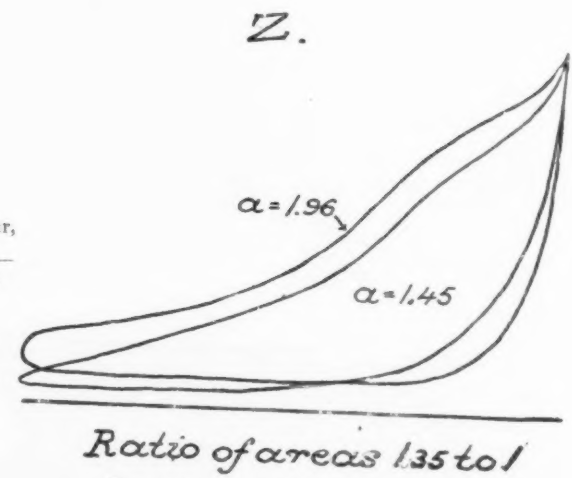
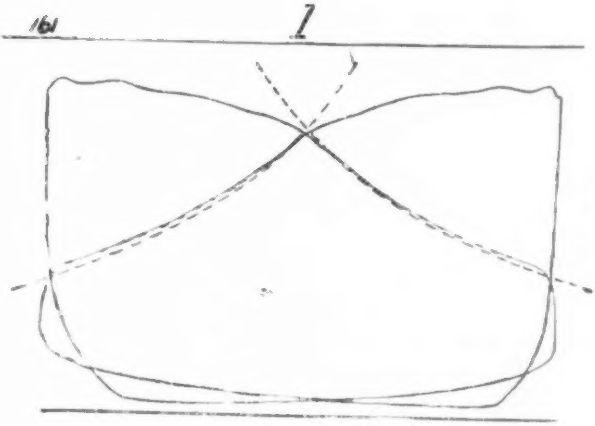
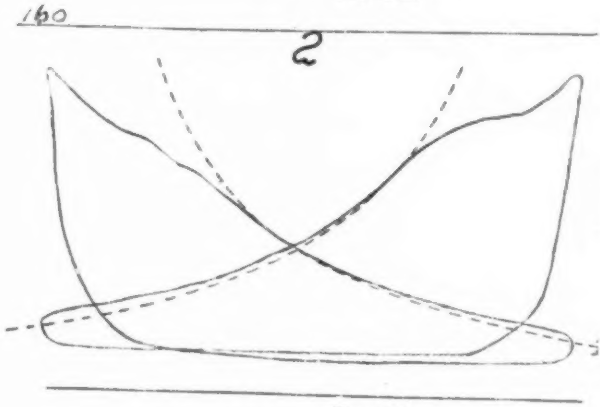


FIG. 181.



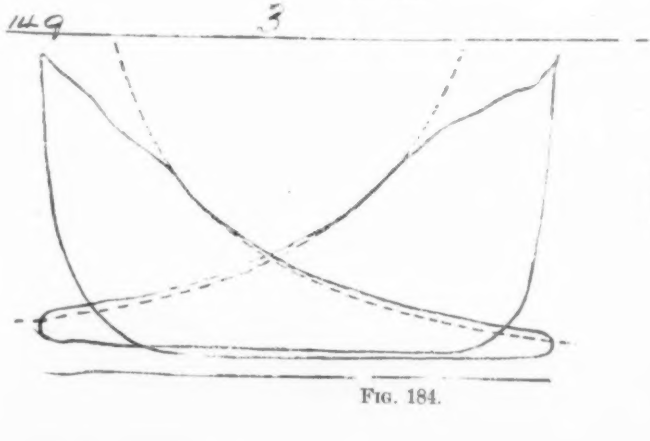
Boiler Press.... 161
M. E. P.......100.1
Speed.........14.52
Rev..............77.9
Total I. H. P. 594.4

Fig. 182.



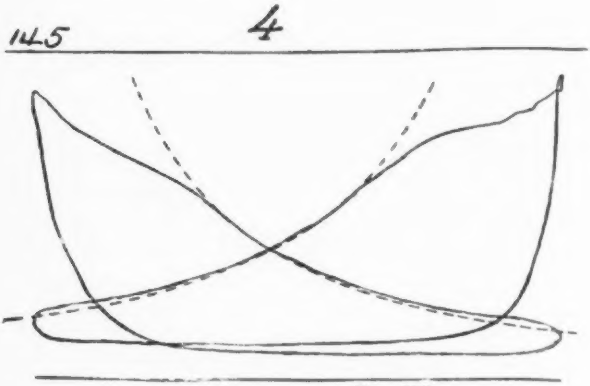
Boiler Press.... 160
M. E. P........51.4
Speed.........26.60
Rev............143.4
Total I. H. P. 561.6

Fig. 183.



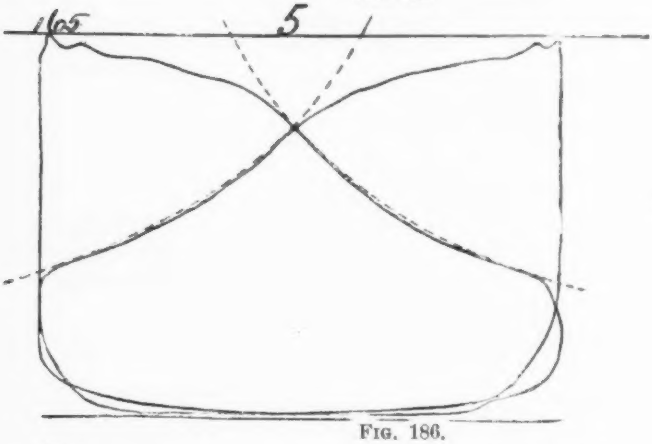
Boiler Press....149
M. E. P.......48.7
Speed.........21.95
Rev...........117.9
Total I. H. P...437

Fig. 184.



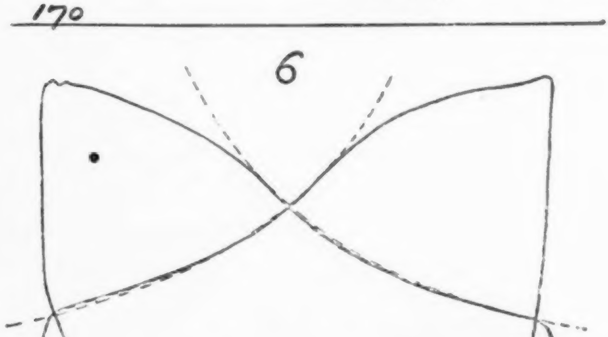
Boiler Press... 145
M. E. P.........45
Speed.......44.79
Rev..........240.5
Total I. H. P. 823.8

FIG. 185.



Boiler Press....165
M. E. P......111.6

FIG. 186.



Boiler Press....170
M. E. P.......84.1

FIG. 187.

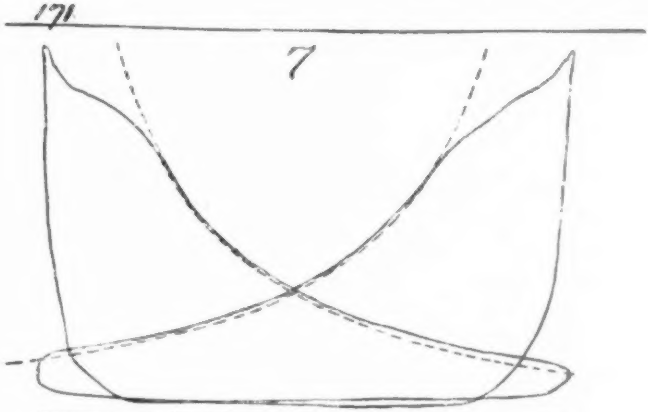


Fig. 188.

Boiler Press.... 171
M. E. P.......58.9
Speed....... 40.44
Rev..........206.2
Total I. H. P. 834.6

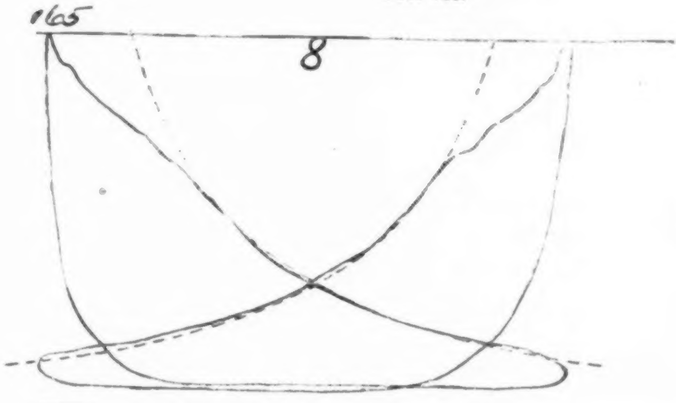


Fig. 189.

Boiler Press.... 165
M. E. P.......51.8

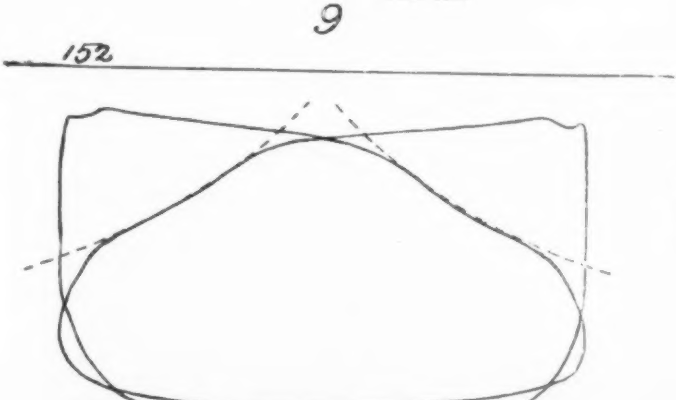
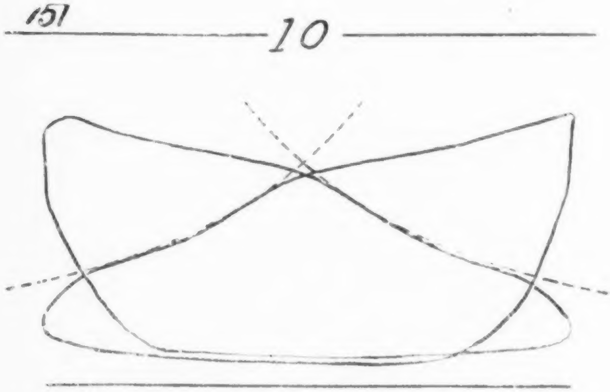


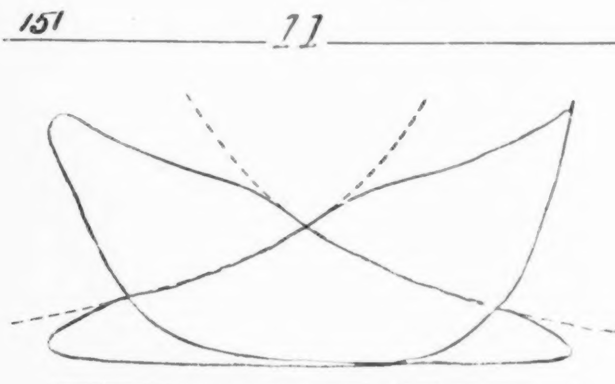
Fig. 190.

Boiler Press.... 152
M. E. P...... 101.2
Speed........19.46
Rev...........98.7
Total I. H. P. 762.4



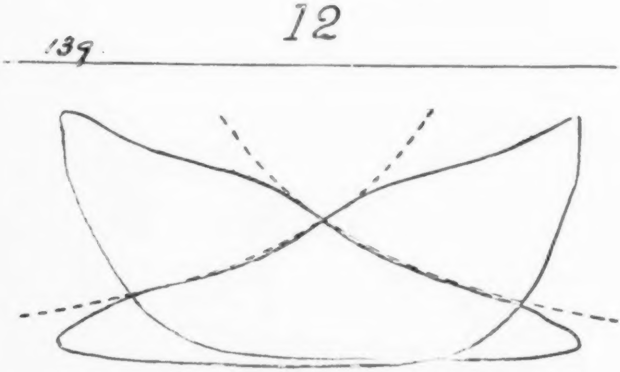
Boiler Press....151
M. E. P.......63.7
Speed........29.51
Rev 149.7
Total I. H. P.744.8

FIG. 191.



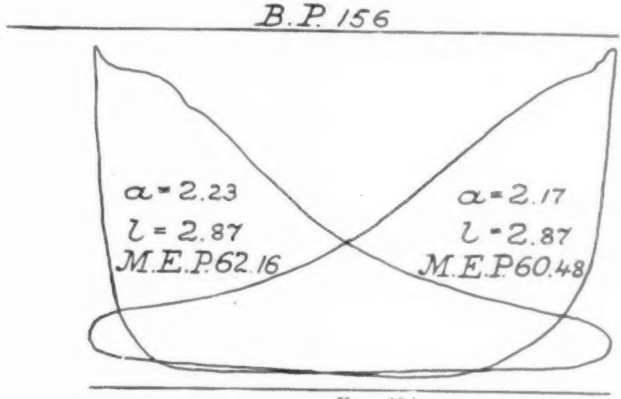
Boiler Press....151
M. E. P.......48.5
Speed........36.36
Rev 184.5
Total I. H. P.698.6

FIG. 192.



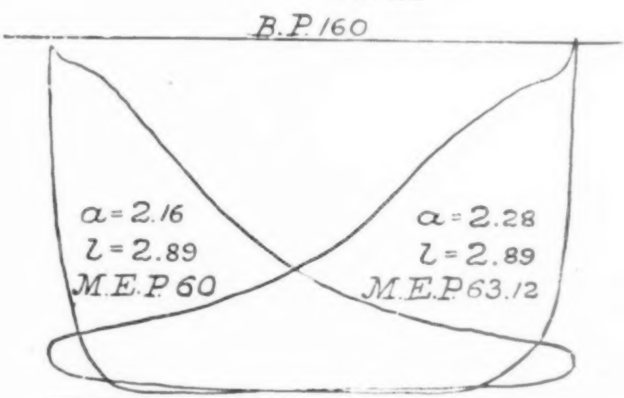
Boiler Press ... 139
M. E. P.......52.5

FIG. 193.



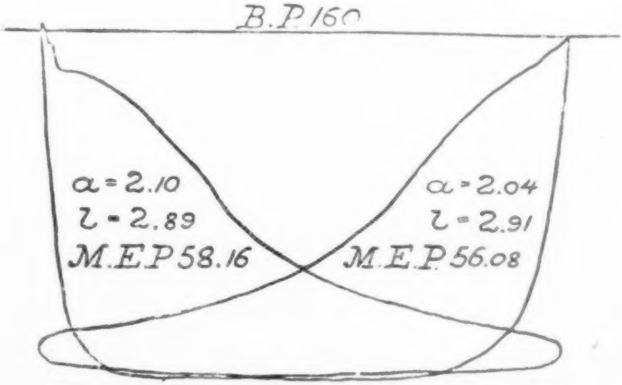
53
R. L., 8.
T. L., 10.

Fig. 194.



37
R. L., 10.
T L.

Fig. 195.



36
R. L., 10.
T. L., 10.

Fig. 196

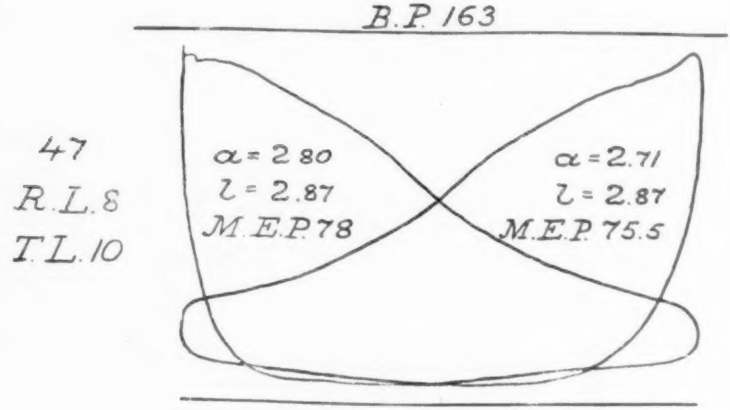


FIG. 197.

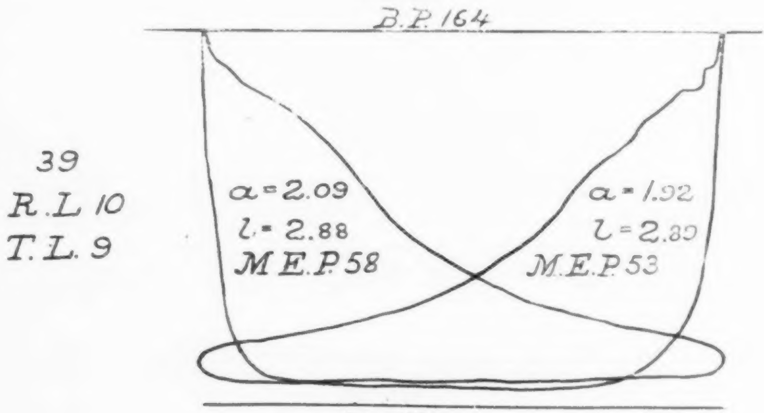


FIG. 198.

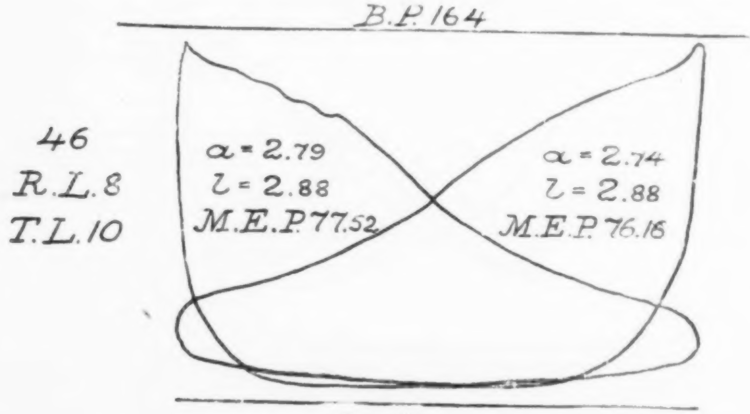


FIG. 199.

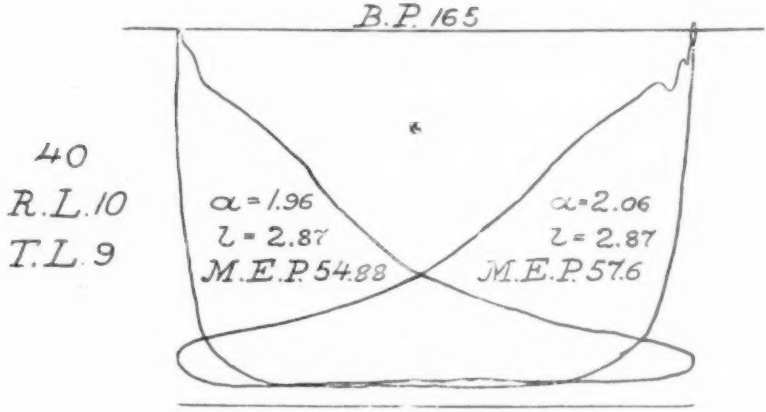


FIG. 200.

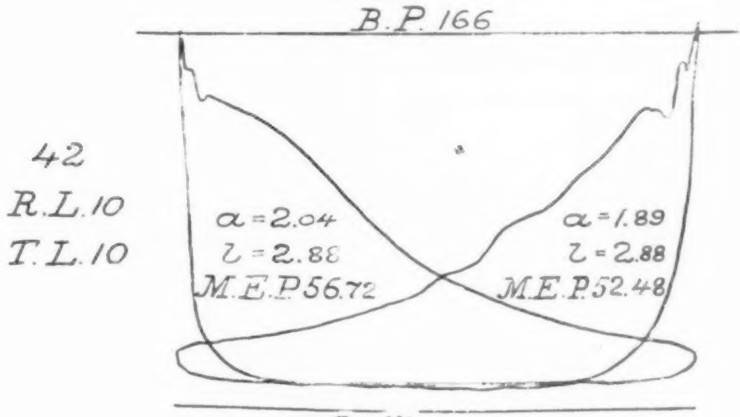


FIG. 201.

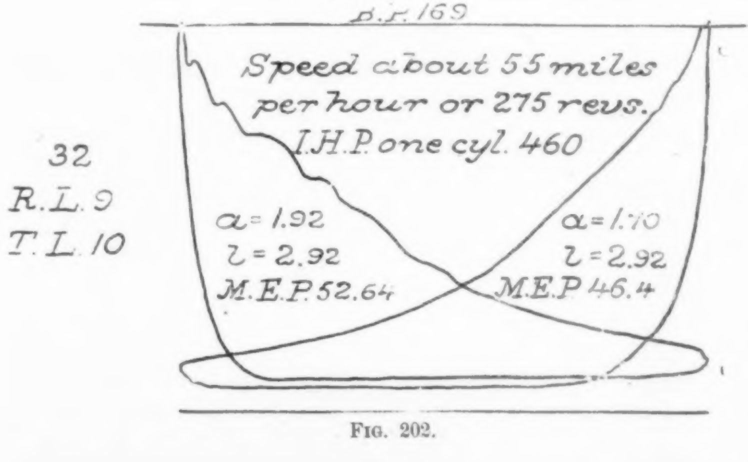


FIG. 202.

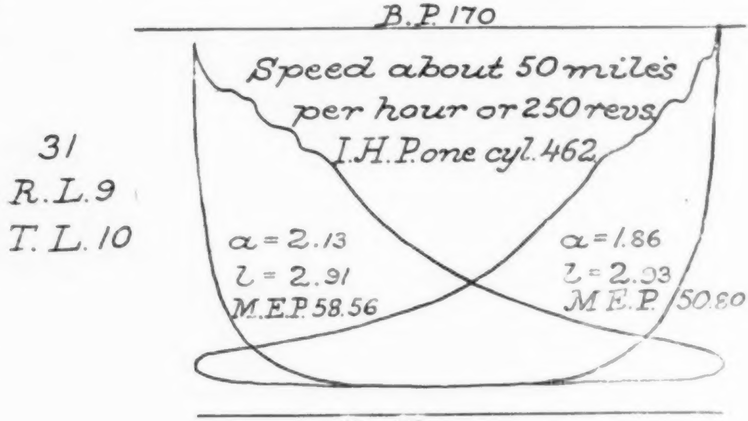


FIG. 203.

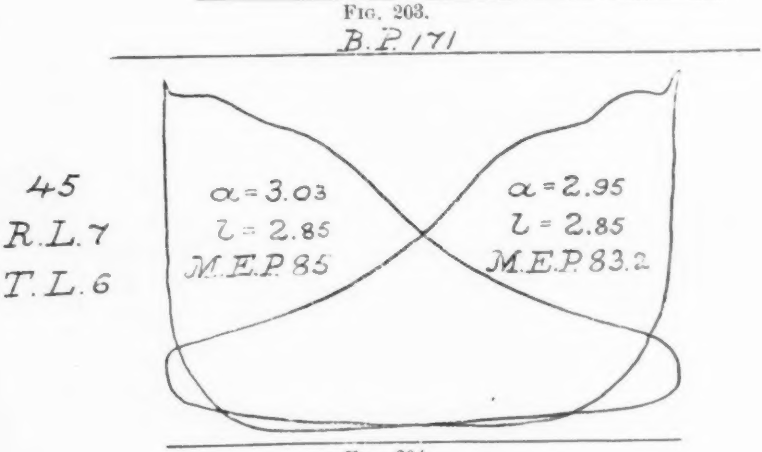


FIG. 204.

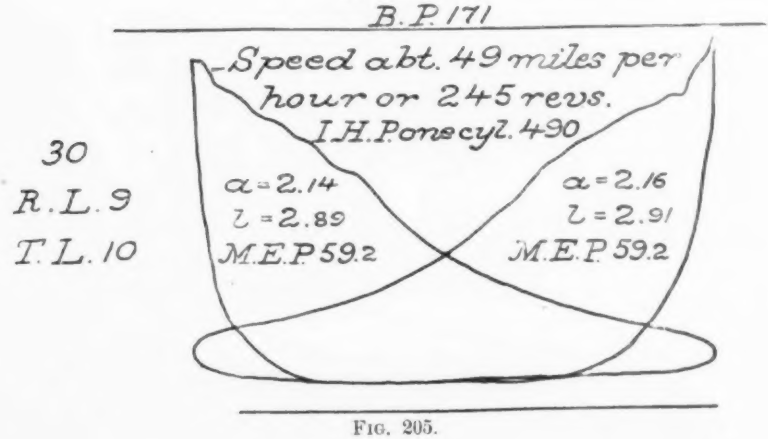


FIG. 205.

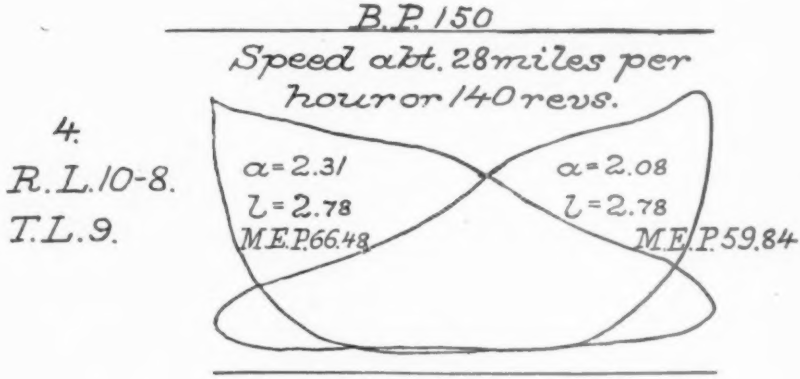


FIG. 206.

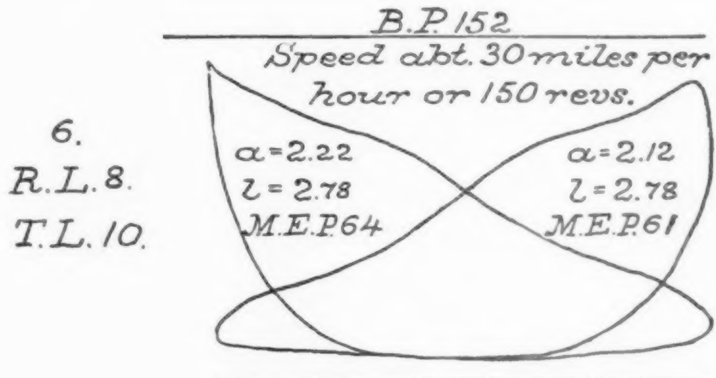


FIG. 207.

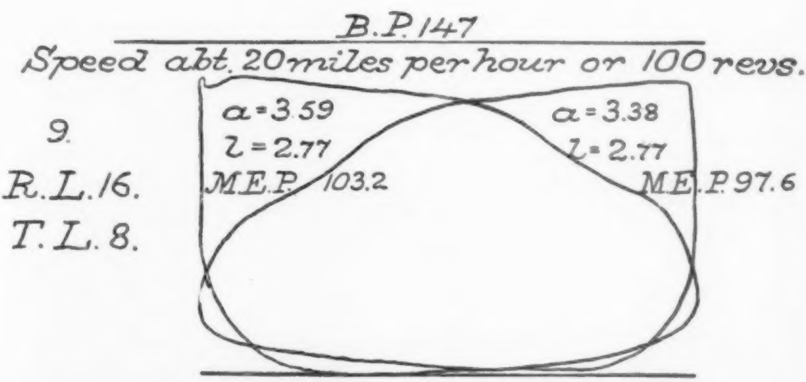


FIG. 208.

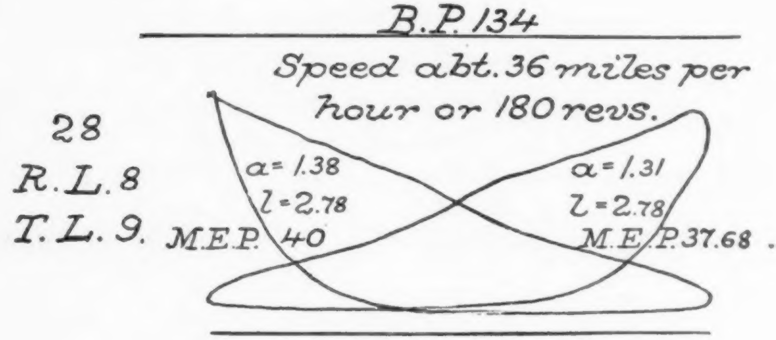


Fig. 209.

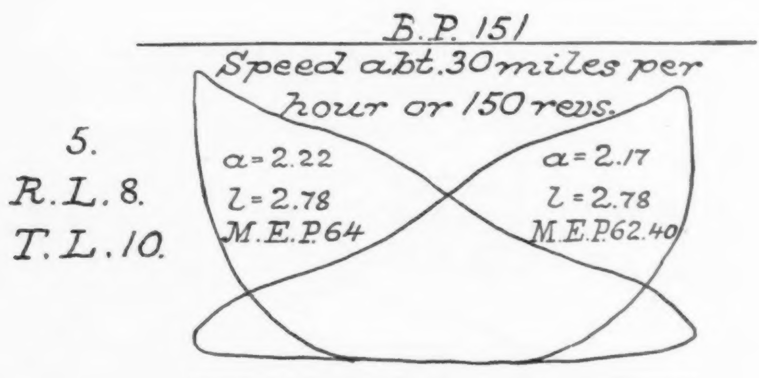


Fig. 210.

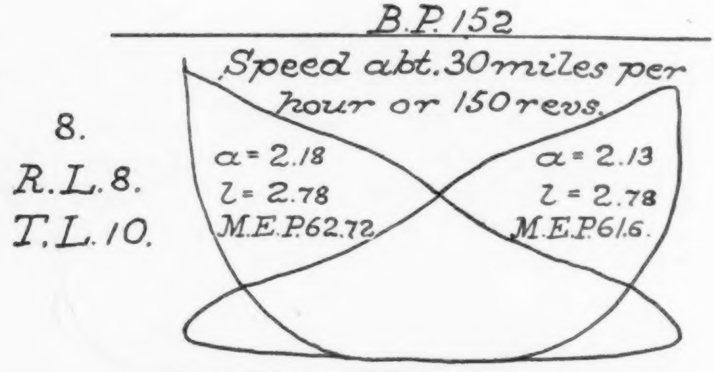


Fig. 211.

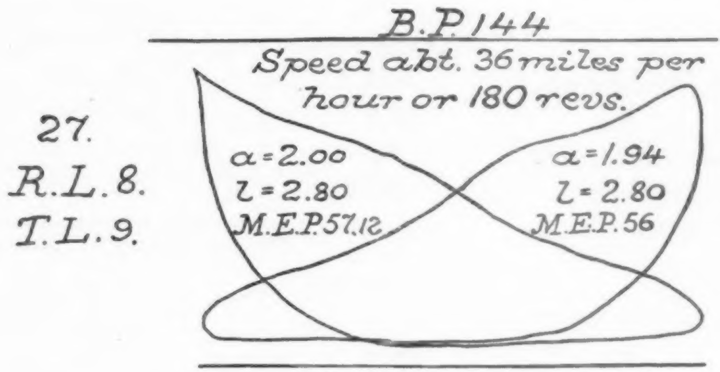


FIG. 212.

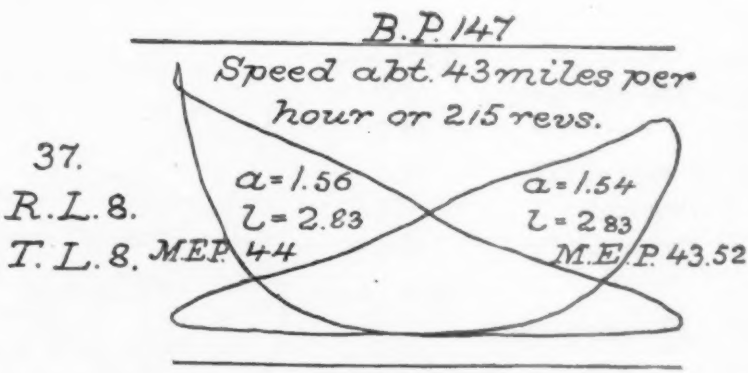
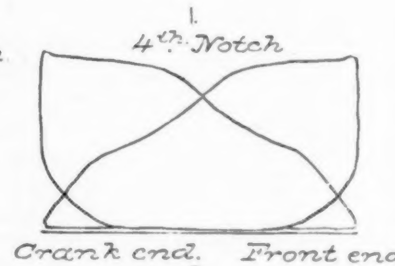


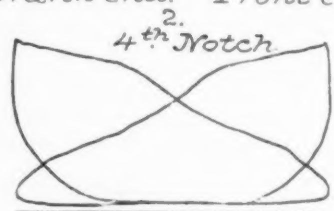
FIG. 213.

Scale 160

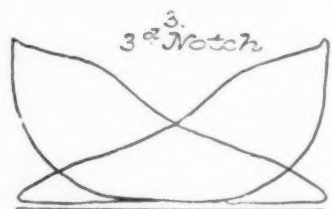
Time	3.22½ p.m.
Boiler Pressure	160
Chest "	153
Rev. per minute	70
M.E.P. front	97.8
M.E.P. crank	101.9



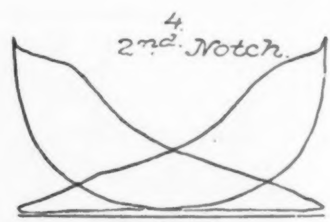
Time	3.27½ p.m.
Boiler Pressure	160
Chest "	148
Rev. per minute	140
M.E.P. front	79.0
M.E.P. crank	82.2



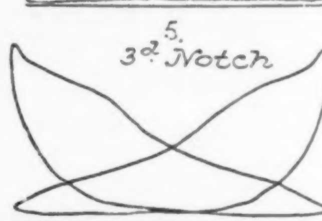
Time	3.30 p.m.
Boiler Pressure	160
Chest "	148
Rev. per minute	140
M.E.P. front	65.9
M.E.P. crank	66.8



Time	5.37½ p.m.
Boiler Pressure	160
Chest "	152
Rev. per minute	150
M.E.P. front	51.1
M.E.P. crank	52.4

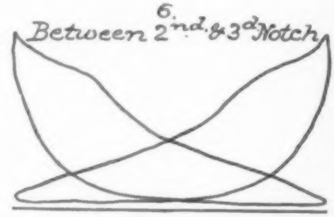


Time	12.05 p.m.
Boiler Pressure	160
Chest "	148
Rev. per minute	208
M.E.P. front	54.9
M.E.P. crank	55.2

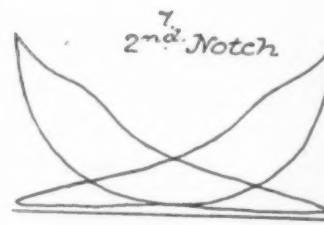


FIGS. 214–218.

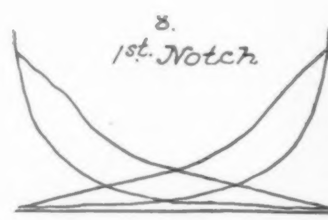
Time	12.27½ p.m.
Boiler Pressure	160
Chest "	146
Revs. per minute	192
M.E.P. front	49.9
M.E.P. crank	51.1



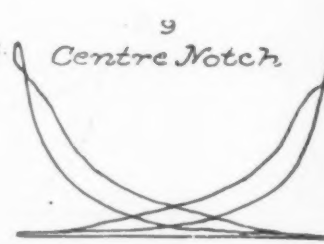
Time	2.31½ p.m.
Boiler Pressure	155
Chest "	145
Rev. per minute	212
M.E.P. front	40.3
M.E.P. crank	39.8



Time	1.15 p.m
Boiler Pressure	150?
Chest "	149?
Rev. per minute	228
M.E.P. front	26.3
M.E.P. crank	25.6

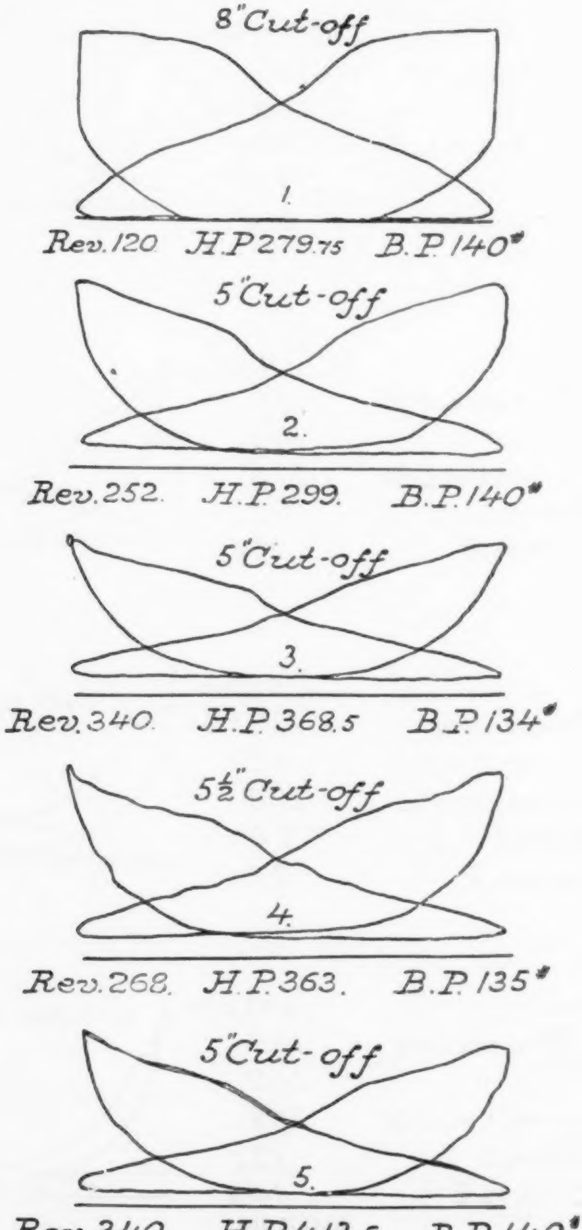


Time	5.45 p.m.
Boiler Pressure	155
Chest "	149
Rev. per minute	208
M.E.P. front	13.7
M.E.P. crank	14.9

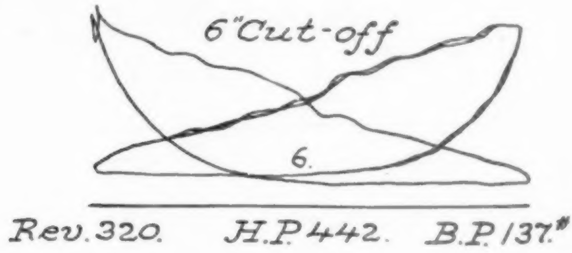


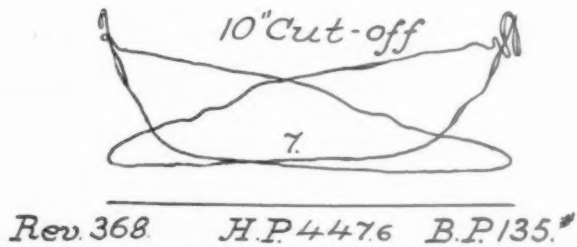
SCALE 160.

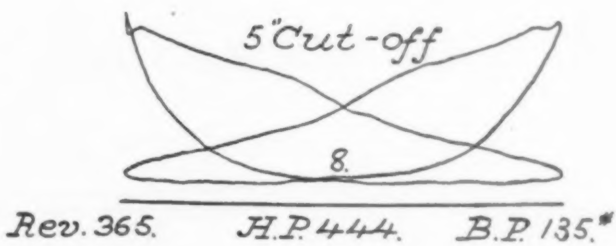
FIGS. 219-222.

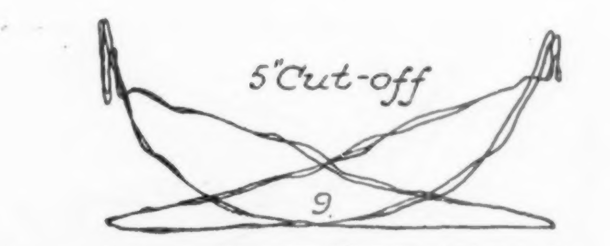

8" Cut-off
1.
Rev. 120 H.P 279.75 B.P 140#
5" Cut-off
2.
Rev. 252. H.P 299. B.P 140#
5" Cut-off
3.
Rev. 340. H.P 368.5 B.P 134#
5½" Cut-off
4.
Rev. 268. H.P 363. B.P 135#
5" Cut-off
5.
Rev. 340 H.P 413.5 B.P 140#

Figs. 223-227.


6"Cut-off
6.
Rev. 320. H.P 442. B.P 137.#


10"Cut-off
7.
Rev. 368 H.P 4476 B.P 135.#

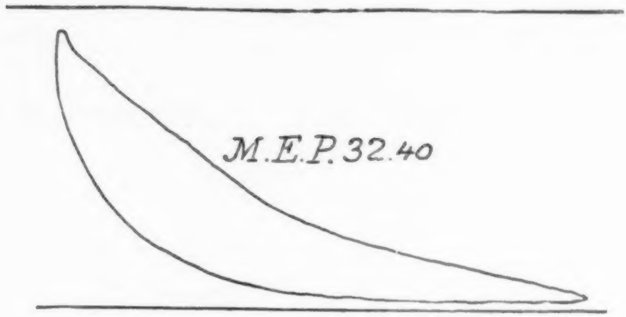

5"Cut-off
8.
Rev. 365. H.P 444. B.P 135.#


5"Cut-off
9.
Rev. 390. H.P 349. B.P 145

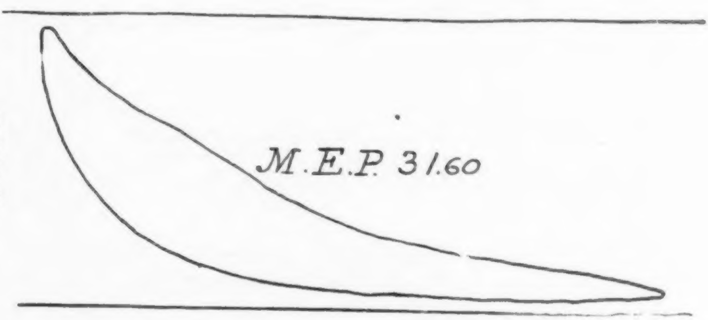
FIGS. 228–231.

Scale 80

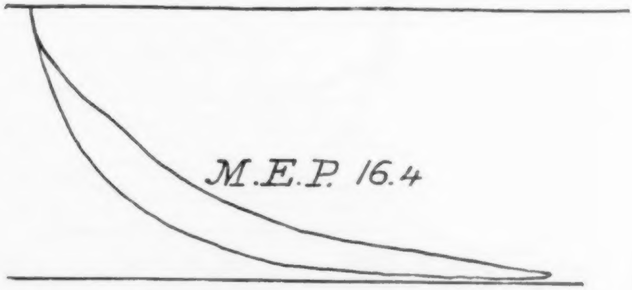
Steam Chest Pres...135
Speed............ 65
Revs. per minute.280.15
Cut off............27%
Gradient 1 in 264 down.
Total I. H. P.....615.79



Steam Chest Pres...130
Speed...............39
Revs. per minute.221.52
Cut off.............17%
Gradient 1 in 264 down.
Total I. H. P. 474.88



Steam Chest Pres...120
Speed........ 50
Revs. per minute..257.0
Cut off.... ....15%
Gradient 1 in 264 down.
Total I. H. P (?)



FIGS. 232–234.

Steam Chest Pres....140
Speed..................36
Revs. per minute....185
Cut off...............23%
Gradient....1 in 264 up.
Total I. H. P.......370.3

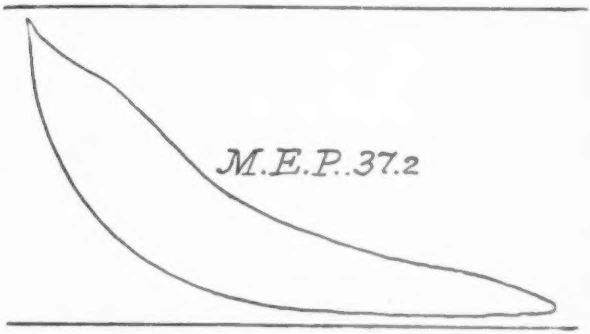


Fig. 235.

Steam Chest Pres...110
Speed................ 31
Revs. per minute.159.34
Cut off..............15%
Gradient,1 in 264 down.
Total I. H. P.....183.42

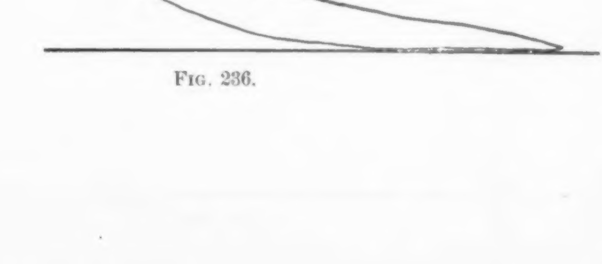


Fig. 236.

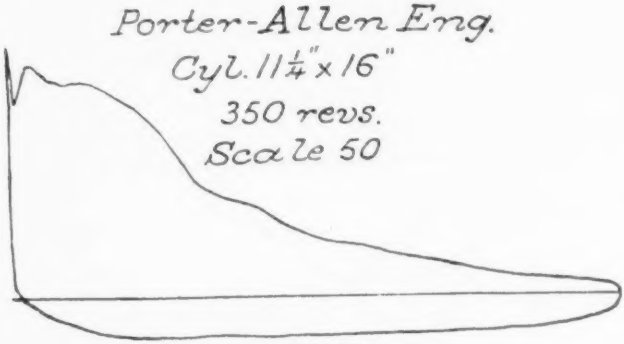


FIG. 237.

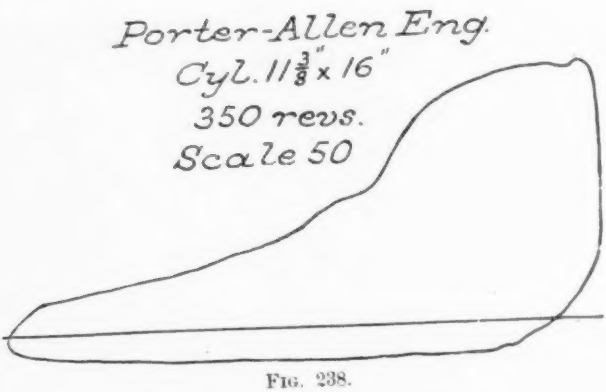


FIG. 238.

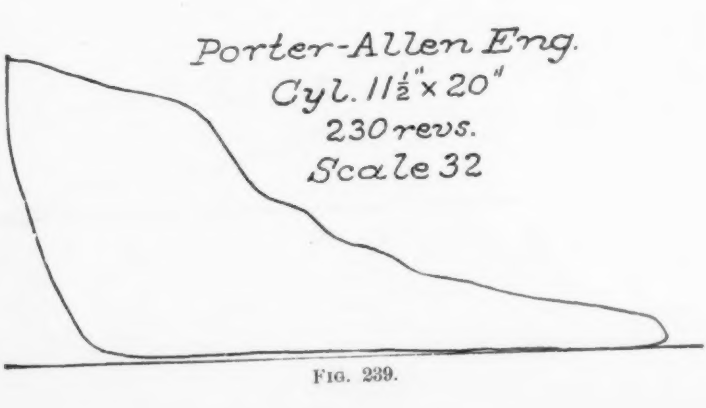


FIG. 239.

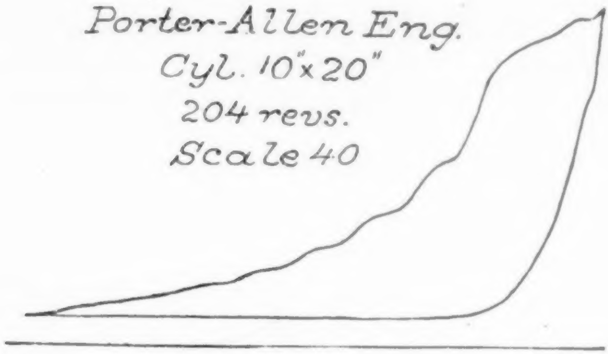


FIG. 240.

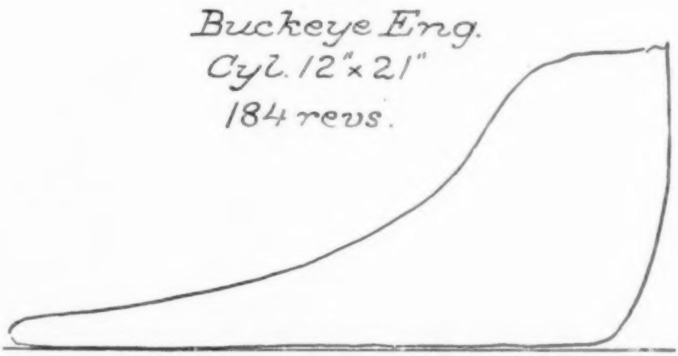


FIG. 241.

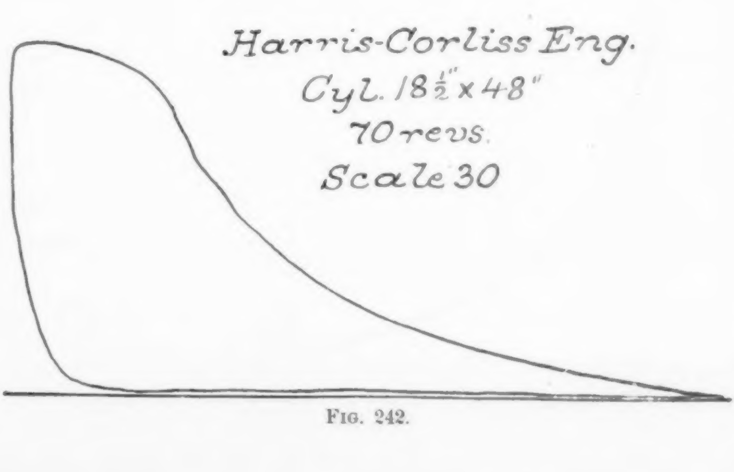


FIG. 242.

30" × 60" Cam. Valve Gear Eng.
58 revs.
Scale 60

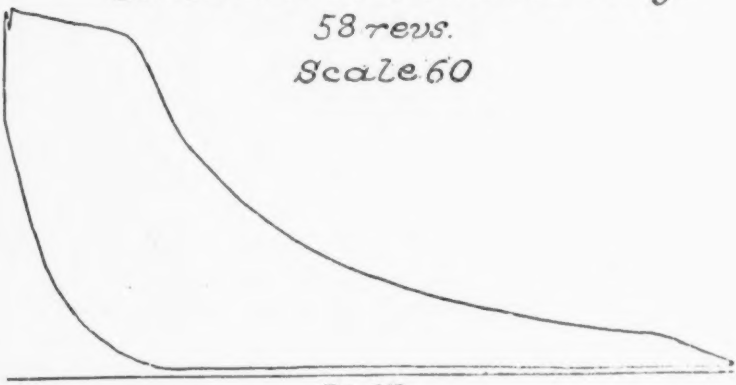


FIG. 243.

Boston Sewage Pumping Eng. H. P. Cyl.
Cyl. 25¼" × 9'.
13 revs.
Scale 40.

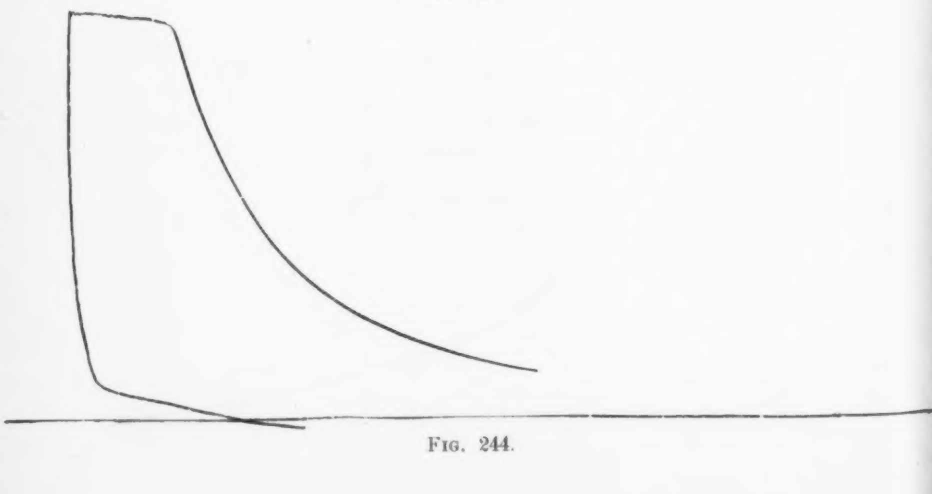
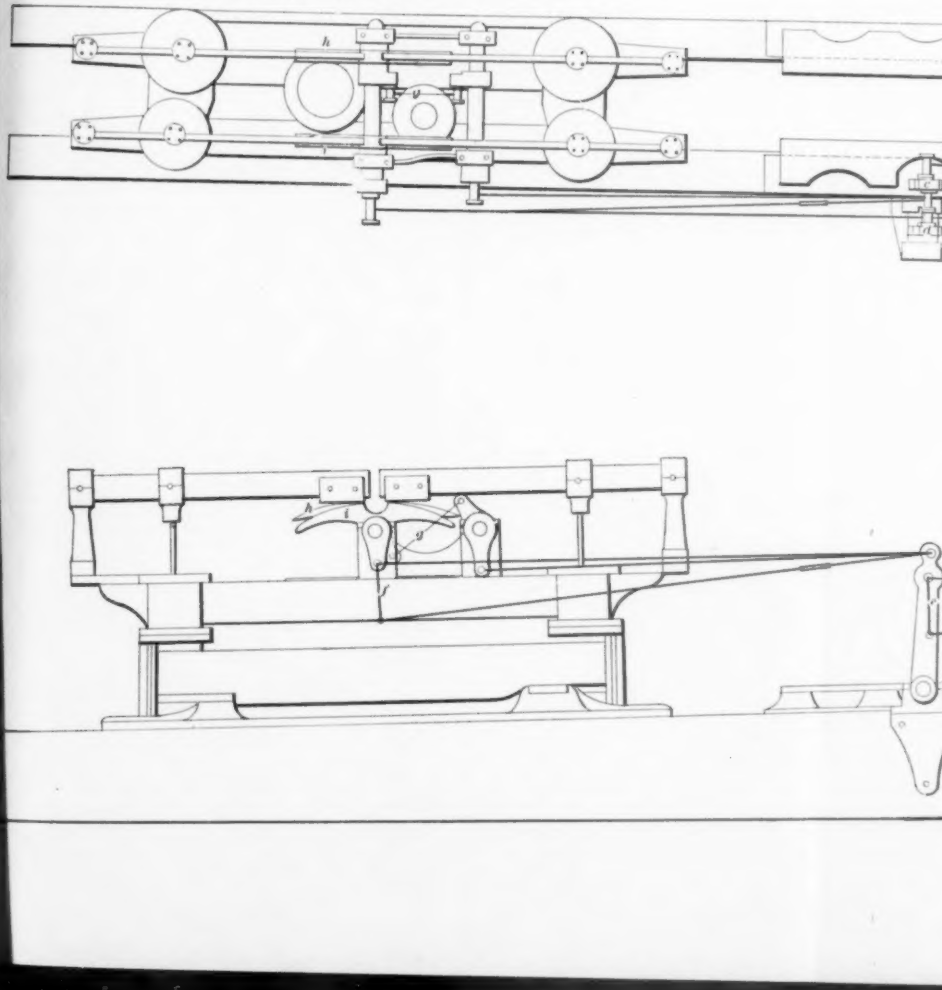


FIG. 244.



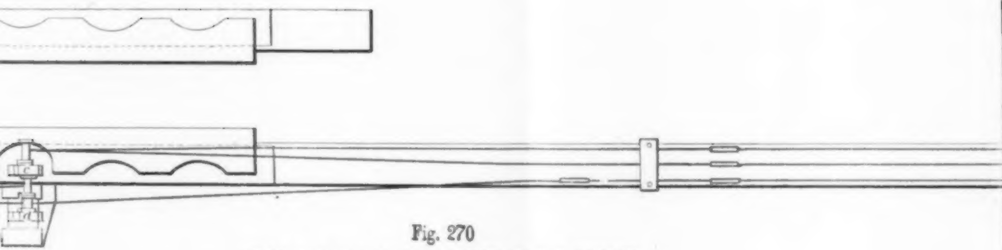
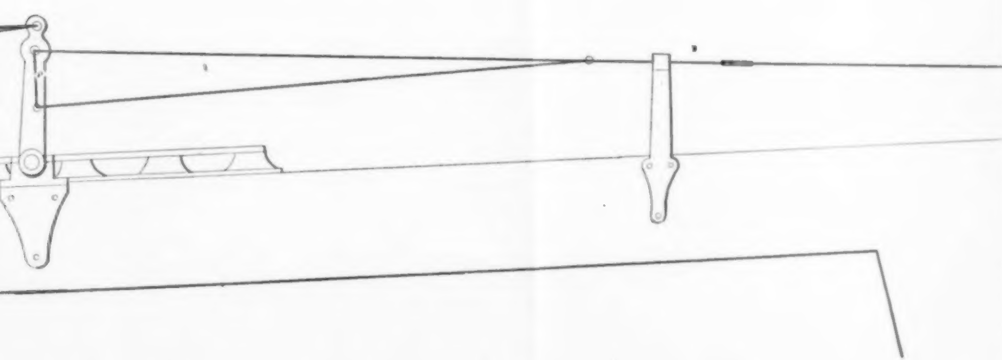


Fig. 270

PLAN AND ELFVATION OF PART OF ONE ENGINE
SHOWING ARRANGEMENT OF VALVE GEAR

a-a-Full Stroke Cams.
b-Cut off Cam.
c-d-Arms of Rocher *d*-loose & *c*-tight on shaft.
e-Double Hook on full stroke Rods.
f- " " connecting full stroke & cut off
g-Link connecting loose knocker.
h i-Exhaust & receiving knockers



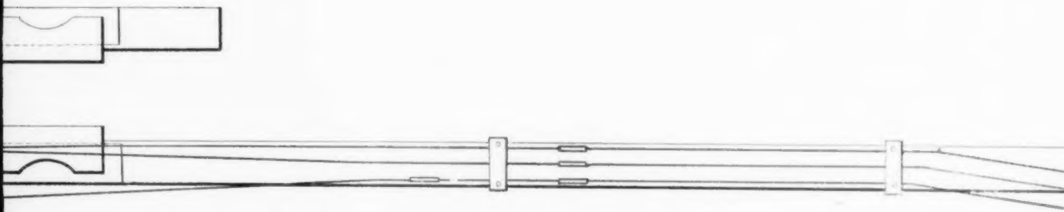
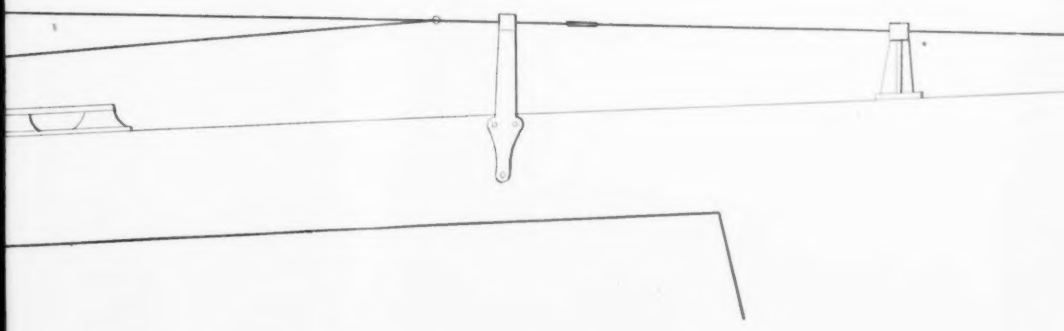


Fig. 270

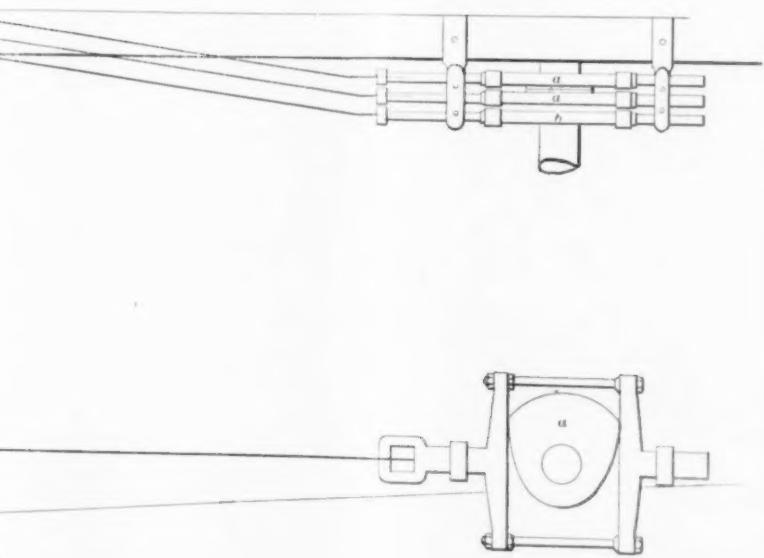
PLAN AND ELEVATION OF PART OF ONE ENGINE

SHOWING ARRANGEMENT OF VALVE GEAR

a-a-Full Stroke Cams.
b-Cut off Cam.
c-d-Arms of Rocher *d*-loose & *c* tight on shaft.
e-Double Hook on full stroke Rods.
f- " " connecting full stroke & cut off
g-Link connecting loose knocker.
h i-Exhaust & receiving knockers



JNO. M. SWEENEY.



engine No. 158 D., L. & W. R. R., on the 4.20 P.M. train from Scranton eastward, Sept. 21, 1885. The loads and conditions were similar to those under which the cards in figures 146 to 150 were taken, and are as follows: Cylinders $19\frac{1}{8} \times 24$ inches. Diameter driving wheels, 68 inches. Load, 7 cars, weighing 360,000 lbs., loaded. Weight of locomotive, 95,000 lbs. Weight of tender, 60,000 lbs. The horse powers (I. H. P.) given are the total for both cylinders on the supposition that all mean effective pressures are equal. The water given is in pounds per I. H. P. per hour. The cards (Thompson Indicator) are from right cylinder, front end only, using shortest pipe possible, well wrapped. Scale of spring, 80. The cut-off is apparently sharper than that of No. 383, but this is misleading, as it is evidently above a theoretical line drawn backward from the release point. This is a frequent difficulty with indicators, and is due to inability, under certain conditions, of the moving parts to be responsive to sudden changes of pressure. The point of cut-off is more defined on account of an 80 spring being used instead of a 100, as in the case of No. 383. The waves in the toes and heels of the cards are probably due to recoil of the indicator drum, which was worked by a string about 42 inches long. In this case, as before, the indicator was attached to the forward end of the cylinder, but the throttle was either wide open or nearly so. The same indicator was used in both cases.

The defects of these cards (those from No. 158) are such as to enlarge their areas, but in working them up their points of release and compression were carefully sought. The steam consumption deduced is therefore too small. Taking it, however, as given on the first ten cards, it averages $25\frac{14}{100}$ pounds per I.H.P. per hour, or $1\frac{16}{100}$ times that of No. 383, or, in other words, the consumption of steam by No. 383 is 86 per cent. of that of No. 158. Selecting a card of No. 158 whose steam consumption is equal to the mean, and correcting its area for imperfections, the consumption rises to $26\frac{2}{10}$ pounds, and the above percentage is reduced to $82\frac{1}{2}$. The most reliable card among those taken by Mr. Coon from No. 383 gives a steam consumption of $20\frac{1}{3}$ pounds, while the most reliable one taken by him from No. 357 gives $24\frac{58}{100}$ pounds, making the percentage $82\frac{7}{10}$.

Figs. 170 to 181 give cards from No. 383 at various cut-offs and speeds, as well as comparative cards between Nos. 383 and 357, and Nos. 383 and 129, of the Boston & Albany Rail-

road, the latter being taken from the *Railroad Gazette* * of November 10, 1882. The spring of the indicator is an 80-pound one.

Figs. 182 to 185 (cards 1 to 4) were taken from engine 444; cards 5 to 8 (Figs. 186 to 189) were taken from engine 383; and cards 9 to 12 (Figs. 190 to 193, were taken from No. 357. These cards are the ones which were used in connection with Tables I., II., III., IV. and V., 80-pound spring.

Figs. 194 to 205 show cards from No. 383, which were selected from those taken May 2, 1887, when the throttle was nearest wide open. The conditions were as follows: Weight of train including engine and tender, going south, 253,000 pounds; going north, 421,500 pounds; actual running time going south, 24 minutes 10 seconds, with a continuous up grade of 96 feet per mile; going north, 59 minutes 17 seconds. Maximum up grade going north, 69 feet per mile for $10\frac{8}{10}$ miles; distance south, $12\frac{1}{100}$ miles; north, $34\frac{2}{10}$ miles. Spring 80 pounds.

Figs. 206 to 213 show the same for No. 357, except that the time south was 25 minutes, and the time north 64 minutes and 27 seconds.

Figs. 214 to 222 shows cards from locomotive No. 129, B. & A. R. R., taken by Mr. George H. Barrus,* and reproduced from the *Railroad Gazette* of November 10, 1882; Figs. 223 to 231 on the scale of a 120-pound spring show cards from No. 169 C. R. R. of N. J., and reproduced from *Engineering* (Vol. XL., p. 283), and figs. 232 to 236 show cards with an 80 spring from the locomotive on the London, Brighton & So. Coast Railroad, reproduced from a paper by Mr. W. S. Stroudley, locomotive superintendent of that line, before the Institution of Civil Engineers.

Figs. 237 to 244 show cards from various well-known stationary engines, inserted as instances of well-taken cards, which show the difficulty, perhaps impossibility, of maintaining straight horizontal steam lines under any conditions.

Tables I., II., III., IV. and V. display the economical performances of engines Nos. 383, 444 and 357 when tested by Mr. Coon in May, 1887, as worked up in Mr. Leavitt's office. They show that No. 383 used $81\frac{1}{2}$ per cent. of as much dry steam per I. H. P. as No. 357, and that No. 444 used 95 per cent. of that used by No. 357. This last result is surprising, and is thus accounted for by persons interested, viz.: that the cast-steel valves with which

* See reproductions by Mr. Barrus of his originals in discussion of this paper. —SECRETARY.

the engine was provided did not wear well, soon became grooved and leaky. The theoretical [isothermal] lines plotted on the cards of No. 444, figs. 182 to 185 corroborate this explanation, as they fall considerably below the actual expansion line—much more so than those of cards from Nos. 383 and 357. Cast-iron valves, as in the case of No. 383, have been substituted.

It was not intended that the scope of this paper should include a description of the boiler with which the inventor proposes to equip his locomotives, and with which No. 444 is equipped. Therefore only a brief statement of the features and claims of advantage will be made.

TABLE I.

ENGINE NO. 357, L. V. R. R., FITTED WITH ORDINARY LOCOMOTIVE BOILER AND STEPHENSON LINK-VALVE GEAR. (THESE FIGURES ARE FOR ONE CYLINDER ONLY.)

No. of Card.	STEAM ACCOUNTED FOR AT RELEASE.						STEAM RETAINED AT EXHAUST CLOSURE.						Steam used per Rev., lbs.	Steam used per Hour, lbs.	Steam used per I. H. P. per Hour, lbs.	Mean Effective Press. per Rev., lbs.	Speed, Miles per Hour.	No. of Rev. per Minute.	Indicated Horse Power.
	Gauge Press. at Release.	Fraction of Stroke at Release.	Volume, cu. ft., including Clearance.	Weight, lbs. per cu. ft.	Weight, lbs. per Stroke.	Weight, lbs. per Rev.	Gauge Press. at Closure.	Fraction of Stroke after Closure.	Volume, cu. ft., including Clearance.	Weight, lbs. per cu. ft.	Weight, lbs. per Stroke.	Weight, lbs. per Rev.							
9...........	80	2.54/2.87	4.281	0.22247	0.9524	1.8692	29	0.10/2.87	0.485	0.1088	0.0528	0.1095	1.7597	10425	27.34	101.2	19.46	98.7	381.2
	75	2.57/2.87	4.328	.21183	.9168		31	0.11/2.87	.500	.1134	.0567								
12	42	2.46/2.86	4.170	.1388	.5788	1.1410	25	0.42/2.86	.985	.0994	.0979	0.2184	0.9226	10758	27.01	52.5	38.30 Approximate.	194.3	398.3
	40	2.47/2.86	4.186	.1343	.5622		31	0.47/2.86	1.063	.1134	.1205								
10............	51	2.56/2.89	4.285	.15921	.6822	1.3451	37	0.27/2.89	.746	.1274	.0950	0.1885	1.1566	10391	27.91	63.7	29.51	149.7	372.4
	49	2.56/2.89	4.285	.15471	.6629		35	0.28/2.89	762	.1227	.0935								
11............	41	2.35/2.88	3.974	.1366	.5428	1.0701	51	0.28/2.88	763	.15921	.1215	0.2567	0.8134	9003	25.78	48.5	36.36	184.5	349.3
	40	2.32/2.88	3.926	.1343	.5273		53	0.32/2.88	.826	.16372	.1252								
May 7, 1887. 22..........	33	2.53/2.88	4.252	.1181	.5022	0.9821	20	0.77/2.88	1.523	.0875	.1333	0.2757	0.7064	8709	25.63	42.4	40.5	205.5	399.8
	32	2.46/2.88	4.144	.1158	.4799		27	0.67/2.88	1.368	.1041	.1424								

TABLE II.

ENGINE NO. 383, L. V. R. R. FITTED WITH ORDINARY LOCOMOTIVE BOILER AND STRONG'S VALVE GEAR. (THESE FIGURES ARE FOR ONE CYLINDER ONLY.)

No. of Card.	Gauge Press. at Release.	STEAM ACCOUNTED FOR AT RELEASE.					Gauge Press. at Closure.	STEAM RETAINED AT EXHAUST CLOSURE					Steam Used per Rev., lbs.	Steam Used per Hour, lbs.	Steam Used per I. H. P. per Hour, lbs.	Mean Effective Press. per Rev., lbs.	Speed, Miles per Hour.	No. of Rev. per Minute.	Indicated Horse Power.
		Fraction of Stroke at Release.	Volume, cu. ft., including Clearance.	Weight, lbs. per cu. ft.	Weight, lbs. per Stroke.	Weight, lbs. per Rev.		Fraction of Stroke after Closure.	Volume, cu. ft., including Clearance.	Weight, lbs. per cu. ft.	Weight, lbs. per Stroke.	Weight, lbs. per Rev.							
6	48	$\frac{2.63}{2.80}$	3.938	0.15248	0.6035	1.1695	21	$\frac{0.15}{2.80}$	0.470	0.0899	0.0423	0.0957	1.0738	15979	22.29	84.1	48.65 Approximately	248	716.8
	44	$\frac{2.65}{2.83}$	3.947	.1434	.5660		23	$\frac{0.22}{2.83}$	.565	.0946	.0534								
7	29	$\frac{2.78}{2.91}$	4.021	.1088	.4375	0.8423	17	$\frac{0.38}{2.91}$	.773	.0803	.0621	0.1236	0.7187	8890	21.30	58.9	40.44	206.2	417.3
	26	$\frac{2.74}{2.90}$	3.980	.1017	.4048		43	$\frac{0.13}{2.90}$	.436	.1411	.0615								
8	25	$\frac{2.77}{2.87}$	4.060	.0994	.4036	0.8304	20	$\frac{0.60}{2.87}$	1.080	.0875	.0947	0.1721	0.6583	9536	22.19	51.8	47.36 Approximately	241.4	429.8
	29	$\frac{2.67}{2.87}$	3.923	.1088	.4268		31	$\frac{0.28}{2.87}$	.643	.1204	.0774								
5	67	$\frac{2.69}{2.85}$	3.976	.19454	.7735	1.5191	16	$\frac{0.26}{2.85}$	.618	.0779	.0481	0.0843	1.4348	20785	22.44	111.6	47.36 Approximately	241.4	926
	65	$\frac{2.65}{2.85}$	3.921	.19015	.7456		19	$\frac{0.12}{2.85}$	.425	.0851	.0362								
May 10, '87. 3	88	$\frac{2.71}{2.84}$	4.017	.23925	.9611	1.8939	9	$\frac{0.35}{2.84}$	.744	.0610	.0454	0.0812	1.8127	9686	24.10	131.3	17.47	89.1	401.9
	85	$\frac{2.70}{2.84}$	4.003	.23302	.9328		10	$\frac{0.22}{2.84}$	.564	.0634	.0358								
May 2, '87. 38	23	$\frac{2.79}{2.90}$	4.049	.0946	.3830	0.7987	15	$\frac{0.48}{2.90}$	.911	.0755	.0688	0.1161	0.6826	7669	21.71	54.9	36.73	187.2	353.3
	27	$\frac{2.74}{2.89}$	3.993	.1041	.4157		15	$\frac{0.27}{2.89}$	.627	.0755	.0473								

TABLE III.

ENGINE NO. 444, L. V. R. R. FITTED WITH STRONG'S BOILER AND STRONG'S VALVE GEAR. (THESE FIGURES ARE FOR ONE CYLINDER ONLY.)

No. of Card.	Gauge Press. at Release.	STEAM ACCOUNTED FOR AT RELEASE.					Gauge Pressure at Closure.	STEAM RETAINED AT EXH. CLOSURE.					Steam Used per Rev., lbs.	Steam Used per Hour, lbs.	Steam Used per I. H. P. per Hour, lbs.	Mean Effective Press. per Rev.	Speed, Miles per Hour.	No. of Rev. per Minute.	Indicated Horse Power.
		Fraction of Stroke at Release.	Volume, cu. ft., including Clearance.	Weight, lbs. per cu. ft.	Weight, lbs. per Stroke.	Weight, lbs. per Rev.		Fraction of Stroke after Closure.	Volume, cu. ft., including Clearance.	Weight, lbs. per cu. ft.	Weight, lbs. per Stroke.	Weight, lbs. per Rev.							
1	64	$\frac{2.71}{2.83}$	4.463	0.18797	0.8389	1.6928	15	$\frac{0.22}{2.83}$	0.618	0.0755	0.0466	0.0955	1.5933	7449	25.06	100.1	14.52	77.9	297.2
	66	$\frac{2.69}{2.82}$	4.440	.19232	.8539		11	$\frac{0.34}{2.82}$	.804	.0658	.0529								
2	35	$\frac{2.86}{2.93}$	4.537	.1227	.5567	1.0711	54	$\frac{0.17}{2.93}$	.581	.16596	.0881	0.1923	0.8788	7561	26.93	51.4	26.60	143.4	280.8
	31	$\frac{2.83}{2.90}$	4.536	.1134	.5144		49	$\frac{0.22}{2.90}$	.609	.15471	.0942								
3	28	$\frac{2.68}{2.81}$	4.439	.1064	.4723	0.8742	20	$\frac{0.30}{2.81}$	.744	.0875	.0651	0.1423	0.7319	5176	23.69	48.7	21.95	117.9	218.5
	21	$\frac{2.70}{2.81}$	4.470	.0899	.4019		22	$\frac{0.36}{2.81}$	.837	.0922	.0772								
4	30	$\frac{2.79}{2.87}$	4.519	.1111	.5021	0.9432	31	$\frac{0.28}{2.87}$	.704	.1134	.0798	0.1734	0.7698	11110	26.97	45	44.79	240.5	411.9
	26	$\frac{2.67}{2.87}$	4.337	.1017	.4411		31	$\frac{0.36}{2.87}$	.825	.1134	.0936								

TABLE OF EVAPORATIVE

	ENGINE No. 444.									
	MAY 5.			MAY 11.			MAY 18.			
	Sug. Notch to Fairview.	W. Haven to Glen St.	Entire Trip.	Sug. Notch to Fairview.	W. Haven to Glen St.	Entire Trip.	Sug. Notch to Fairview.	M. Church to P. H. Jc.	Entire Trip.	Sug. Notch to Fairview.
Running time, Minutes and Seconds	25.06	22.00		25.06	22.39		24.43	15.56		24.52
Running time, Hours	0.4183	0.3667		0.4183	0.3775		0.4120	0.2656		0.4144
Area of heating surface, sq. ft.					1,848					
" " grate " "					62					
Average indicated horse power	796.84	762.32		807.58	783.76		926.09	780.50		865.10
Temperature of feed water					64.3					
Average steam pressure			154			146			159	
Coal used per trip, pounds			6,511			6,948			7,530	
" " " hour, "	2,694	2,909		2,813	2,896		3,663	2,817		3,155
" " " sq. ft. of grate surface per hour, pounds	43	47		45	47		59	45		51
" " " I. H. P. per hour, pounds	3.38	3.82		3.48	3.70		3.69	3.61		3.65
Water used per trip, pounds	8,778	8,306	50,708	8.708	8,092	51,422	10,094	5,362	53,970	9,548
" " " hour, "	20,983	22,653		20,816	21,433		24,503	20,192		23,038
" " " sq. ft. of heating surface per hour, pounds	11.35	12.26		11.26	11.57		13.26	10.93		12.47
" " " " grate " " " "	338	365		336	346		395	326		372
" " " I. H. P. per hour, pounds	26.33	29.72		25.78	27.35		26.44	25.87	447	26.63
" evaporated per pound of coal, pounds			7.79			7.40			7.17	
Equivalent evaporation from and at 212°, pounds			9.37			8.90			8.63	

TABLE V.

TABLE OF EVAPORATIVE PERFORMANCE.

ENGINE No. 444.						
	MAY 18.			MAY 19.		
Entire Trip.	Sug. Notch to Fairview.	M. Church to P. H. Jc.	Entire Trip.	Sug. Notch to Fairview.	W. Haven to Glen St.	Entire Trip.
	24.43	15.56		24.52	19.8	
	0.4120	0.2656		0.4144	0.3189	
	926.09	780.50		865.10	990.40	
146			159			147
,948			7,530			7,154
	3,663	2,817		3,155	3,797	
	59	45		51	61	
	3.69	3.61		3.65	3.83	
1,422	10,094	5,362	53,970	9,548	8,840	52,234
	24,503	20,192		23,038	27,721	
	13.26	10.93		12.47	15.00	
	395	326		372		
	26.44	25.87	447	26.63	27.99	
7.40			7.17			7.30
8.90			8.63			8.78

ENGINE No. 383.							
MAY 2.			MAY 3.		MAY 10.		
Sug. Notch to Fairview.	W. Haven to Glen St.	Entire Trip.	Sug. Notch to Fairview.	Entire Trip.	Sug. Notch to Fairview.	W. Haven to Glen St.	Entire Trip.
24.10	22.53		23.13		22.39	20.19	
0.4028	0.3814		0.3869		0.3775	0.3386	
			1,385.9				
			37.12				
807.23	723.77		853.53		876.40	818	
			64.3				
		159		163			159
		8,311		7,666			7,217
3,744	3,337		3,693		3,232	3,197	
101	90		99		87	86	
4 64	4.61		4.33		3.69	3.91	
7,896	6,664	43,512	7,896	42,364	7,056	6,260	41,734
19,604	17,473		20,406		18,691	18,487	
14.15	12.61		14.72		13.49	13.34	
528	471		550		504	498	
24.29	24.14		23.91		21.33	22.60	
		5.24		5.53			5.78
		6.30		6.66			6 96

ENGINE No. 357.				
MAY 7.		MAY 9.		
W. Haven to Glen St.	Entire Trip.	Sug. Notch to Fairview.	W. Haven to Glen St.	Entire Trip.
23.15		25.00	22.19	
0.3875		0.4167	0.3719	
1,572.1				
39.2				
697.15		764.7	736.7	
64.3				
	147			147
	8,260			7,914
3,220		3,129	3,145	
82		80	80	
4.62		4.09	4.27	
7,224	47,824	8,092	7,260	49,112
18,643		19,418	19,519	
1,189		1,235	1,242	
476		495	498	
26.74		25.39	26.50	
	5.79			6.21
	6.96			7.46

TABLE IV.

TABLE OF CYLINDER PERFORMANCE.

	ENGINE NO. 444.				ENGINE NO. 383.				ENGINE NO. 357.			
Diameter of cylinders, inches	20				19				$20\frac{1}{4}$			
Stroke of piston, inches	24				24				24			
Piston displacement per stroke, cu. ft.	4.363				3.938				4.466			
Clearance, cu. ft.	0.278 = 6.38%				0.259 = 6.58%				0.329 = 7.35%			
Diameter of drivers, inches	$62\frac{13}{32}''$				$65\frac{15}{16}''$				$66\frac{1}{4}''$			
No. of revolutions per mile	322.21				305.87				304.43			
No. of indicator diagram	1	2	3	4	5	6	7	8	9	10	11	12
Date	May 20, 1887.	May 20, 1887.	May 5, 1887.	May 11, 1887.	May 10, 1887.	May 2, 1887.	May 2, 1887.	May 10, 1887.	May 9, 1887.	May 9, 1887.	May 9, 1887.	May 9, 1887.
Speed, miles per hour	14.52	26.60	21.95	44.79	....	....	40.44	....	19.46	29.51	36.36	....
No. of revolutions per minute	77.9	143.4	117.9	240.5	....	....	206.2	....	98.7	149.7	184.5	....
Boiler pressure	163	161	149	144	165	169	170	170	147	151	151	138
Mean effective pressure	100.1	51.4	48.7	45	111.6	84.1	58.9	51.8	101.2	63.7	48.5	52.5
Total indicated horse power, two cylinders	594.4	561.6	437	823.8	....	....	834.6	....	762.4	744.8	698.6	....
Steam accounted for at release, lbs.	1.6928	1.0711	0.8742	0.9432	1.5191	1.1695	0.8423	0.8304	1.8692	1.3451	1.0701	1.1410
Steam retained by exhaust closure, lbs.	0.0955	0.1923	0.1423	0.1734	0.0843	0.0957	0.1236	0.1721	0.1095	0.1885	0.2567	0.2184
Steam used per rev. by one cylinder, lbs.	1.5933	0.8788	0.7319	0.7698	1.4348	1.0738	0.7187	0.6583	1.7597	1.1566	0.8134	0.9226
Total steam used per hour, lbs.	14898	15122	10352	22220	....	....	17780	....	20850	20782	18006	....
Steam used per I. H. P. per hour, lbs.	25.06	26.93	23.69	26.97	22.44	22.29	21.30	22.19	27.34	27.91	25.78	27.01

There are two corrugated furnaces, which by means of a junction piece lead into a single corrugated combustion chamber, the latter terminating in the back tube sheet, from which the tubes proceed forward, as in the ordinary locomotive, to the smoke-box. While the ordinary soft coal burning boiler 52 inches in diameter has about 900 stay bolts, the Strong boiler has not one. There is not a rigid connection between the inner and outer parts of the boiler, and only two connections of any kind between the ends, the functions of which are to support the inner shell. Therefore there is nothing whatever to resist expansion and contraction, and thus hurtfully act upon the material. The corrugations doubtless contribute to freedom of movement, but even if they do not, the end plates of the outer shell have the usual opportunity to buckle. The crown sheet, being the upper half of a cylinder, easily parts with scale which may form upon it, and in this respect is in strong contrast with the common flat horizontal crown sheets covered with bolts and crown bars, which are efficient means of anchoring all scale which forms upon the sheet, and equally efficient means of preventing inspection and cleaning. The crown sheet of the new boiler, if such it may be called, is accessible from end to end. An inspector can crawl all over it, feel of it, see it, and clean it—qualities which are invaluable in the West, where the water is bad. The circulation of the water is entirely unimpeded. There is water under the fire-box which is free to rise, and its place can be occupied by other water which comes without obstruction. In this respect it is far superior to the ordinary locomotive boiler.

The inner shell has no joint which is in contact with the fire, except that connecting the back tube plate and combustion chamber, which does not differ from common practice.

It is unnecessary here to speak of corrugated furnaces. They have made high pressures possible at sea, and no modern steamship is without them.

On the whole this boiler is far in advance of the ordinary locomotive boiler, and deserves a hearty reception from the railroad master mechanics.

COMMENTS ON THE INDICATOR CARDS.

The distinguishing characteristics of the Strong cards are the steam line, which is convex upward, the consequent rounding form at the cut-off, the late release, the (in general) comparatively small back pressure, and the fullness at the compression line, all of

which contribute to a high mean effective pressure for any given cut-off. The convex [upward] steam line is found at ordinary speeds, and with a fairly full throttle at fairly high speeds, while a link-motion card is concave at the steam line at all but the very slowest speeds, when it generally is a sloping straight line. This concavity of the steam line gives to the card the appearance of a sharper cut-off than is secured with the Strong gear, which is misleading. A given radius of curvature corresponding to a given rate of cut-off is apparently sharper if joined to a concave line than if joined to a convex one.

The mean effective pressures at the opposite ends of the Strong cylinder differ considerably, but there is nothing inherent in the valve gear, that the writer has discovered, which should produce this. The mean effective pressures of No. 357 differ at the two ends, but this might have been overcome by adjusting the gear. The remarkable equality of the mean effective pressures of the B. and A. R. R.'s engine No. 129, figs. 214 to 222 was thus secured.

The cut-off of No. 129's cards, figs. 214 to 222, are in several instances too sharp, for they are evidently outside of an isothermal line drawn backward from a point of the lower part of the expansion line. In this respect the cards from No. 169, Central R. R. of N. J., are interesting. These cards are all but worthless. Many of them show a very sharp cut-off at about half stroke when the record gives the cut-off at five inches. The indicator was not responsive. The areas are all too large, and the horse powers based upon them are erroneous.

The back pressures of the B. and A. cards are, in general, much smaller than the others. But in comparing this feature with that of the other engines, it must be remembered that the B. and A. engine is a soft coal burner, and that all of the others burn anthracite [except the L., B. & S. C. engine], which makes a small nozzle necessary. Moreover the L.V. R. R. and D. L. and W. R. R. engines are built and adjusted for heavy fast mountain work, where the steaming qualities of the engines are taxed to the utmost, and great powers are developed. The maximum grade on the L. V. R. R. southward is 96 feet per mile for 12 miles without a break, and 69 feet per mile for 10.8 miles northward. On the D., L. and W. R. R. it is 53 feet to 75 feet per mile eastward for 19.3 miles continuously, and 77 feet to 83 feet westward for fifteen miles continuously. Both roads have a constant succession of sharp curves. The B. and A. R. R. maximum grade is 60 feet

per mile between Boston and Springfield, on which division the cards were taken, and curves are comparatively few. The length of the grade is not great. B. and A. cards No. 5 give the maximum horse power, viz.: 640, while the back pressure is 8 lbs. approximately, the speed being 208 revolutions. Cards No. 30 of No. 383, Plate V., give 980 horse power with $13\frac{1}{2}$ lbs. back pressure, the speed being 245 revolutions. The piston speeds of the two engines are 763 feet for No. 129, and 980 for No. 383. Engine No. 158 of the D., L. and W. R. R. shows 17 lbs. back pressure when developing 800 horse power at 181 revolutions. No. 383, however, when developing 725 horse power at 200 revolutions, showed only 7 lbs. back pressure, or 1 lb. less than No. 129 when developing 640 horse power at 208 revolutions. (See Fig. 148.)

Fig. 188 shows a back pressure of 8 lbs. when No. 383 was indicating 835 horse power at 206 revolutions. Such instances can be duplicated indefinitely.

REMARKS ON THE TABLES OF WATER AND COAL CONSUMPTION.

The "Coal used per hour," the "Coal used per square foot of grate surface," and the "Coal used per I.H.P., per hour" have been calculated by using the average evaporation for the entire trip, and the water used per hour between stations. The average actual consumption of water for No. 383 was $23\frac{1}{4}$ lbs. per I.H.P. per hour, and that of No. 357, $26\frac{21}{1000}$, or No. 383 consumed 88 per cent. of that of No. 357. The average actual water consumption of No. 444 was 27.01 lbs., or more than that of No. 357. This is inconsistent with the consumption of steam as deduced from the diagrams from the indicator, and is therefore to be credited to the boiler and the personal equations of the men in charge.

Notwithstanding that the boiler was obliged to evaporate so much water, it did it with an average evaporation of 7.41 lbs. of water per pound of coal, while the boiler of No. 357, which was a 54-inch wagon-top boiler, evaporated on the average 6 lbs. per pound of coal, or the evaporation of No. 357 was only 81 per cent. of that of No. 444. The average evaporation of No. 383, which has a 55-inch straight-top boiler, was 5.52 lbs. of water per pound of coal, or only $74\frac{1}{2}$ per cent. of that of No. 444. These comparisons show the great efficiency of the Strong boiler over the common type. The efficiency of the Strong boiler, while the water consumption was very high, brought the coal consumption down to an average of 3.64 lbs. per I. H. P. per hour, while those of

Nos. 357 and 383 were respectively 4.33 and 4.24. That is to say, whether working upon water or steam, 444 used 84 per cent. of the coal required by 357 in developing one horse power, and 86 per cent. of that required by 383.

This shows the economy of No. 383 to be due to the valve gear, and that of No. 444 to the boiler.

The great water consumption of No. 444 was due to excessive blowing off, and to priming, caused by a timid engineer's carrying the water too high.*

The true test of the valve gear is to be found in the last line of Table IV., which shows results from the cards, to which reference has already been made [see p. 11] which are independent of the boiler performances. These results show the consumption of dry saturated steam by No. 383 to be—as before stated—$81\frac{6}{10}$ per cent. of that of No. 357, and the consumption of steam by No. 444 to be 95 per cent. of that of No. 357. There is no apparent reason why No. 444 with her new cast-iron valves should not equal No. 383, because the cylinders and valve gears of the engines are identical, except that No. 444's cylinder is one inch larger in diameter than No. 383's.

The question will be raised by all thoughtful persons, whether they are in sympathy with the Strong locomotive or not, as to the cost and durability of the new engine. Of these features of its existence the writer can only express an opinion. He sees no reason why, with special machinery—with which all locomotives are now built—and with care in the design, the great economy of the engine will not bring a handsome return upon the investment. As to the durability of the valve gear, he has the greatest faith. He sees complicated stationary engines running at all speeds from ten to twenty-four hours per day, six days in the week, while the locomotives run from four to eight hours only. There is no reason why men cannot acquire the skill necessary to properly handle and care for a high-class four-valve locomotive, as well as for a Corliss engine. Experience shows on the Lehigh Valley line that, to use the late A. L. Holley's humorous allusion to the Corliss locomotives, the farmers along our railroads will not be kept from their legitimate occupations by picking up scrap iron thrown over their fields by passing engines.

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* At a future meeting the writer hopes to present the results of an inquiry into the behavior of the steam in the cylinders of No. 444.

The writer takes pleasure in stating that he does not anticipate a permanent decline in American agriculture in consequence of the advent of the Strong locomotive.

LEADING PARTICULARS OF THE L. V. R. R. LOCOMOTIVES.

	ENGINE 444.	ENGINE 383.	ENGINE 357.
CYLINDERS, ETC.			
Cylinder, diameter and stroke	20″ by 24″	19″ by 24″	$20\frac{1}{4}$″ by 24″
Diameter of piston rod	$3\frac{1}{4}$″	$3\frac{1}{4}$″	$2\frac{3}{32}$ and $2\frac{31}{32}$″
Length of connecting rod, centers	8′ 3″	....	....
Transverse distance between cylinder centers	7′	....	....
Distance from center of main drivers to center of cylinders	12′ 10″	....	....
No. of valves per cylinder	4, 2 steam, 2 exh.	4, 2 steam, 2 exh.	One
Type of valves	Gridiron	Gridiron	Balanced slide
No. of ports per valve	10	10	....
Size of ports	$4\frac{5}{8}$″ by $\frac{3}{4}$″	....	17″ x $1\frac{1}{4}$″ & 17″ x $2\frac{1}{2}$″
Full travel of valves	$1\frac{1}{16}$″	$1\frac{1}{16}$″	$4\frac{9}{16}$″
Lap of valves	$\frac{5}{16}$″	....	$\frac{3}{4}$
Inside lap	....	....	$\frac{1}{16}$″
Width of bridge	....	....	$1\frac{1}{8}$″
Lead of valves, steam	$\frac{1}{8}$″ constant	$\frac{1}{8}$″ constant	$\frac{3}{32}$″
Lead of valves, exhaust	$\frac{5}{16}$″ constant	$\frac{5}{16}$″ constant	....
Throw of eccentrics	$2\frac{1}{2}$″	....	....
Tractive force per lb. of mean effective pressure on piston	154.8 lbs.	131.3 lbs.	149.11 lbs.
Cylinder clearance in cu. in.	481	448	568
Cylinder clearance in per ct. of piston displacement	6.38%	6.58%	7.35%
Range of cut-off	4″ to $19\frac{1}{2}$″	4″ to $19\frac{1}{2}$″	....
Release begins at	23″	23″	....
Compression begins at	2″ to 4″ ordinarily	2″ to 4″ ordinarily	....
WHEELS AND JOURNALS.			
Driving and truck wheel centers (all tires of steel)	Wrought iron	Cast iron	Cast iron
Front truck	4-wheel swing beam.	4-wheel swing beam	4-wheel swing beam
Rear truck	2-wheel swing and radius bar	None	None
Nominal diameter of driving wheels	62″	66″	68″
Calipered diameter of driving wheels	$62\frac{19}{32}$″	$65\frac{15}{16}$″	$66\frac{1}{4}$″
Diameter of front truck wheels	30″	....	....
Diameter of rear truck wheels	42″	....	....
Total wheel-base of engine	30′ 2″	22′ 9″	22′, 1″
Rigid " " " "	5′ 7″	7′ 9″	7′
Driving axle journals (diam. and length)	$7\frac{1}{2}$″ by 10″	....	....
Front truck axle journals (diam. and length)	6″ by 11″	....	....
Rear truck axle journals (diam. and length)	7″ by 9″	....	....

	ENGINE 444.	ENGINE 383.	ENGINE 357.
Main crank pin journals (diam. and length)	5" by 6"	....	....
Coupling-rod journals (diam. and length)	$4\frac{1}{4}$" by 4"	....	....
WEIGHTS, ETC.			
Weight on first pair drivers in working order	30,000 lbs.	....	....
Weight on second pair drivers, in working order	30,000 lbs.	....	....
Weight on third pair drivers, in working order	30,000 lbs.	None	None
Total on drivers, in working order	90,000 lbs.	74,640 lbs.	63,280 lbs.
Weight on front truck, in working order	27,000 lbs.	24,880 lbs.	27,440 lbs.
Weight on rear truck, in working order	21,000 lbs.	None	None.
Total weight of engine, in working order	138,000 lbs.	99,520 lbs.	90,720 lbs.
BOILERS, ETC.			
Height of boiler center above rail	7' 3"	....	....
Kind of boiler	Twin furnace	Straight shell	Wagon top
Material for boiler plate	O. H. steel	O. H. steel	O. H. steel
Diameter of barrel inside smallest ring	58"	55"	54"
Diameter of fire boxes and combustion chambers, corrugated	$38\frac{1}{4}$" inside $42\frac{1}{2}$" outside	....	....
Length of grates	9'	11'	11'
Width of grates	$40\frac{1}{2}$"	3' $4\frac{1}{2}$"	3' $6\frac{3}{4}$
Number of tubes, all iron	306	229	248
Diameter of tubes, outside	$1\frac{3}{4}$"	2"	2"
Length of tubes	11' 5"	11' $4\frac{1}{2}$"	12' 2"
Grate area	62 sq. ft.	37.12 sq. ft.	39.2 sq. ft.
Heating surface, fire box, sq. ft	155	151.6	142.3
Heating surface, combustion chamber, sq. ft	93	....	....
Heating surface, tubes, sq. ft	1,600	1234.3	1429.8
Heating surface, total, sq. ft	1,848	1385.9	1572.1
Ratio of heating surface to grate area	29.8 to 1	37.3 to 1	40.1 to 1
Smallest inside diameter of smoke stack	16"	....	....
Height of top of smoke stack above rail	14' 3"	....	....
Working steam pressure per sq. in	160 lbs.	160 lbs.	140 lbs.
TENDER.*			
8-wheeled double trucks, diameter of wheels	33"	....	....
Capacity of tender (gallons)	3,000	....	....
Capacity of tender (coal)	10,000 lbs.	....	....
Weight of tender loaded	70,000 lbs.	....	....

DISCUSSION.

Mr. Geo. H. Barrus.—Mr. Dean has submitted a very interesting paper, giving an analysis of the indicator diagrams taken from the Strong locomotive, and comparing them with diagrams

* The same tender was used on all tests.

from locomotives fitted with the ordinary Stephenson valve gear; and this paper forms a valuable addition to the data published in connection with Mr. Leavitt's report on the same subject. This report gave incomplete data regarding the performance of the engine apart from the boiler, and Mr. Dean's paper supplies the deficiency.

The comparison made between the action of the Strong valve gear and that of the engines of the old-fashioned type, shows, according to Mr. Dean's method, that the new valve gear enables an engine to produce a much larger amount of power than the old gear, and also to secure greater economy in the consumption of steam and fuel. Several comparisons are made giving the superiority in the case of power some 30%, and in the case of economy some 20%.

Mr. Dean has kindly referred to some cards taken by myself from the Boston and Albany locomotive No. 129, and for this reason I may be permitted to criticise his conclusions so far as they relate to the cards in question.

It cannot be doubted that an engine with four valves, of which the Strong locomotive is a type, should secure not only greater capacity but increased economy over an engine with a single valve like those in ordinary use. The criticism which I would offer consists in raising the question as to the amount of this increase. It seems to me that the whole tenor of Mr. Dean's position in this matter is toward over-estimation, and I base my ideas on the subject upon the performance of the Boston and Albany locomotive, No. 129, which I tested, and upon a similar locomotive on the same road, which was tested at the same time.

I would call attention, first, to the column of diagrams taken from this engine, No. 129, Figs. 214 to 222 of Mr. Dean's paper, which are reproductions of those given in the published account of these tests. Suppose a glance is taken at these diagrams, and then reference is made to Figs. 182 to 205, giving diagrams from engine No. 383, which is fitted with the Strong valve gear. Neither set of diagrams corresponds very closely to the ideal perfect diagram. In both instances there is wire drawing before the point of cut-off, early release, early compression and a high back pressure, although the Strong diagrams, except in the matter of wire-drawing, come the nearest to the ideal. Taking, however, the general appearance of the two sets of diagrams, I submit that the Boston and Albany diagrams have a more pleasing effect to the eye than those taken

from the other engine. This comparison, however, does not count for much. It is only a sort of object lesson.

As to the matter of relative capacity shown by the cards from the Strong engine, it is difficult to make a comparison owing to the difficulty of obtaining like conditions of speed, boiler pressure and cut-off. It seems to me that the comparison which has been given at Fig. 181, between the Strong engine and the Boston and Albany engine, wherein it appears that the ratio of area is 1.35 to 1, is quite unfair to the Boston and Albany engine. There is a wide difference in the point of cut-off on the two cards, the Boston and Albany card being the shorter. Had the cut-offs been equal, the areas of the diagrams would doubtless have been substantially the same. Take, for example, this very diagram from the Strong locomotive, and compare it with the one which is numbered 5 in the set from the Boston and Albany engine. The cut-offs are substantially the same. So are the pressures at cut-off and the back pressures. I make the mean effective pressure of the Strong card 53.3 lbs., while that of the Boston and Albany card is 55 lbs.

The proper comparison to make is one in which the cut-offs, the boiler pressures and the speeds are all alike. In other words, diagrams should be selected in which the expansion lines, at some stipulated points of the stroke, show the same pressure, and in which the other conditions were alike. It must be admitted that it is difficult to make such a comparison; but whatever the difficulty, it is certain that this kind of comparison is the one which ought to be made.

Now, as to the question of economy, Mr. Dean's table No. 4, column 7, gives for the steam accounted for by the indicator at release, 21.3 lbs. per indicated horse-power per hour, which is his best result. This is at a pressure of 170 lbs., and a mean effective pressure of 58.9 lbs. On the Boston and Albany locomotive, the steam accounted for at release was 20.3 lbs. per indicated horse power per hour, the boiler pressure being 156 lbs., and the mean effective pressure 54.4 lbs. It appears in the matter of economy, therefore, figured from the diagram, that the Strong engine was inferior to the Boston and Albany engine.

There seems to be some inconsistency in the figures given here, for in Table No. 5 one test gives the feed water consumption as 21.3 lbs. per indicated horse power per hour, whereas the consumption figured from the diagram, with about the same indicated horse power, is the very same figure, namely, 21.3. Taking the average

results of all the feed water tests, which is a consumption of 23.25 lbs. of feed water per indicated horse-power per hour, and taking the average of all the quantities giving the steam accounted for by the diagram, which is 22.05 lbs. per indicated horse-power per hour, we have a ratio of steam accounted for to actual consumption of .95. It seems, then, that the loss from cylinder condensation and leakage, measured at the release in the Strong engine, was only 5 per cent. I submit that this is an unheard-of performance for an engine which must be cutting off at about one-quarter stroke. The steam accounted for at the release in the Boston and Albany engine was .79 of the actual consumption, this being at a cut-off of about one-third. This corresponds to authentic results obtained from automatic cut-off stationary engines—but .95 is altogether out of the question.

Taking the actual consumption of coal of the engine with the Strong valve gear, No. 383, which was an average of 4.24 lbs. per indicated horse power per hour, and comparing it with the coal consumption on the Boston and Albany engine, which was 3.92 lbs. per indicated horse power per hour, the economy of the two types of engines, even here, appears to favor the old-fashioned engine. But a comparison of this kind is not a proper one, for the reason, as stated in Mr. Dean's paper, that one engine used anthracite coal and the other a high grade of bituminous coal. The comparison, however, is suggestive.

In making these criticisms I recognize the fact, which is prominently brought out by Mr. Dean, that the increased port area which the Strong valve gear secures, adapts the engine for high speed, and that at extremely high speeds, the new valve gear should produce decidedly the best results.

In this connection I would refer to a statement given by Mr. Dean in regard to the objectionable features of balanced valves. He offers the opinion that all balancing devices are accompanied by steady or at least intermittent leaking. I do not know how true this is in the majority of cases where balanced valves are used, but I do know that in the case of the valves used on three different Boston and Albany engines which I tested, and which were fitted with a simple balancing device, the valves did not permit a breath of steam to pass when set at mid-stroke and subjected to full boiler pressure. I have every reason to believe, from the statements made to me by the shop hands who had charge of repairs on these engines, that they continued in this

condition under ordinary usage, for several months' time without repair.

The report published in the *Railroad Gazette* related to tests made on three locomotives, two of which, No. 129 and No. 169, were at that time of special design, and one, No. 150, was of the type in common use. They all had the old-fashioned front with petticoat pipe and straight stack. The object of the tests was to compare the economy of the first two-named locomotives with each other, one of these, No. 169, being fitted with an Allen valve having a port $\frac{3}{8}$ inch wide, and the other with a common D valve; and furthermore to compare the economy of these with that of No. 150, the former having increased boiler capacity and carrying 160 lbs. pressure, while the latter carried 130 lbs. in accordance with the then prevailing practice. They were all fitted with the Richardson balanced valve, and each valve was tight under a full pressure at mid throw.

The tests made subsequent to July 14 were conducted on the express passenger train which leaves Boston at 11.00 A. M. and arrives in Springfield at 1.48 P. M.; and on the return trip leaves Springfield at 3.22 P. M. and arrives in Boston at 6.10 P. M. The observations covered the whole round trip.

The coal was taken on at Boston and that unconsumed at the end of the return trip was weighed back. The test was started with a new fire and the fire was burned out at the end of the trip. All the wood and coal required in getting up steam was included in that measured. Water was taken at Boston, Worcester, Springfield, and Worcester, and the quantity used was measured by observations on a glass tube attached to the tender. These observations were taken after filling and before refilling, and at each intermediate stopping point. The effect of changes of level, due to variations of grade in the track whereon the measurements were taken, was allowed for.

A set of indicator diagrams was taken from each cylinder at intervals of two and one-half minutes during the whole trip, on all the tests subsequent to July 14, whenever the throttle valve was open. Three-quarter-inch indicator pipes were used, connecting by easy bends made in the pipes, to a central tee, where the indicator, suitably braced, was located. The rig consisted of a pendulum hung to the running board, with sector for the cord and a connecting rod at the lower end leading to the cross-head.

The tests preceding July 20 were made on the 4.30 P. M. train,

and no diagrams were taken on the return trip from Springfield. On these tests the speed was controlled to some extent by variations in the opening of the throttle valve. On the remaining tests, the throttle, when opened at all, was kept continuously wide open.

For the basis of comparison, which has been referred to, the performance of the engines has been figured for that portion of the return trips which extends from Springfield to East Brookfield, when the conditions were most nearly identical.

The dimensions of the engines and the data and results of the tests are given in the following tables, Nos. 1–3. Table No. 4 embraces the results of additional tests which were made on locomotive No. 169 in September, 1882. These results give information as to the temperature of the escaping gases in the smoke arch and the amount of the draught suction, which were not obtained on the previous tests.

The conclusions of the writer in regard to the results of the various comparisons were detailed fully in the report. They were, briefly, that the Allen valve increased the economy about 2 per cent. The large boiler of No. 129 was about 14 per cent. more economical than that of No. 150; and the high boiler pressure (160 lbs.) of No. 129 secured about 7½ per cent. less consumption of steam per horse power than the lower pressure (130 lbs.) of No. 150.

Appended are copies of representative diagrams, taken in each case from the right-hand cylinder. (Figs. 310 to 340.) They are given with the reversing lever in various notches, both with slow and medium speeds. A full set is given for each day's test of Engine No. 150. The valve on this engine, previous to the first test, had been "set by sound," according to the usual practice. After the first day's test, the valve stems were lengthened about $\frac{3}{64}$ of an inch.

B. P 160
Chest P 153
R. P. M 70
M. E. P. front . . 97.8
M. E. P. crank . 101.9

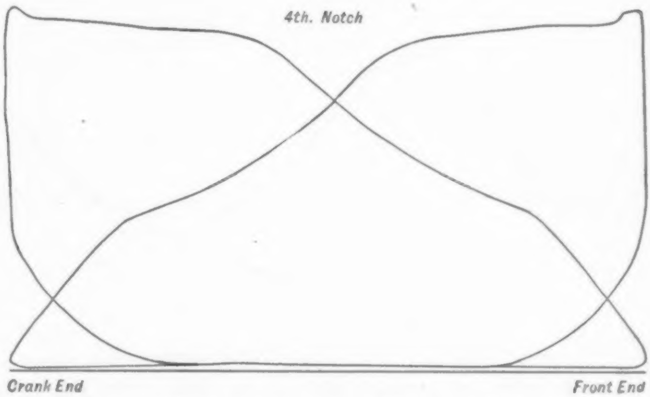

FIG. 310.—No. 1, Loco. 129, July 26. Time, 3.22½ P.M. Scale, 80.

B. P 160
Chest P 148
R. P. M 140
M. E. P. front . . 79.0
M. E. P. crank . 82.2

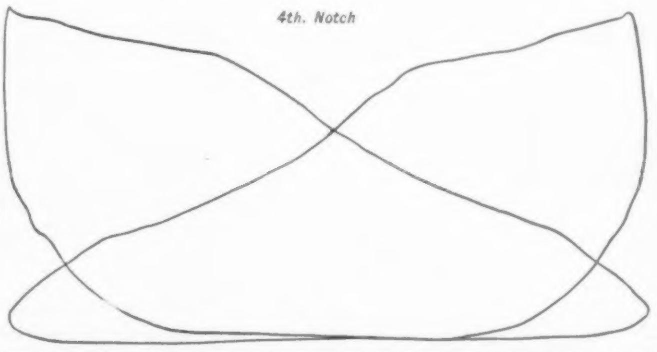

FIG. 311.—No. 2, Loco. 129, July 26. Time, 3.27½ P.M. Scale, 80.

B. P 160
Chest P 148
R. P. M 140
M. E. P. front . . 65.9
M. E. P. crank . 66.8

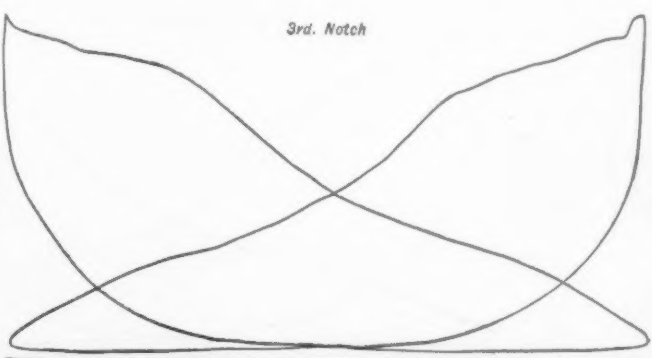

FIG. 312.—No. 3, Loco. 129, July 26. Time, 3.30 P.M. Scale, 80.

B. P............160
Chest P........152
R. P. M.........150
M. E. P. front..51.1
M. E. P. crank .52.4

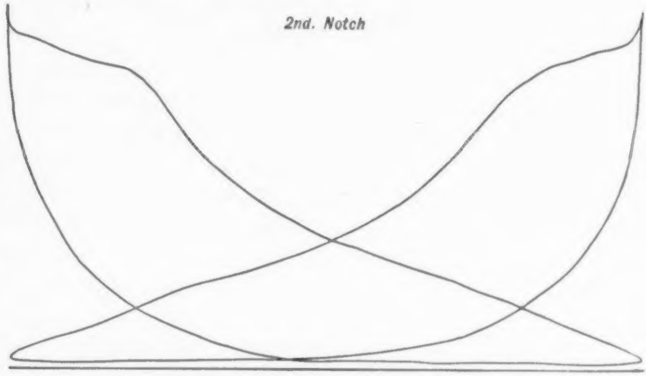

FIG. 313.—No. 4, Loco. 129, July 26. Time, 5.37½ P.M. Scale, 80.

B. P............160
Chest P.........148
R. P. M.........208
M. E. P. front .54.9
M. E. P. crank .55.2

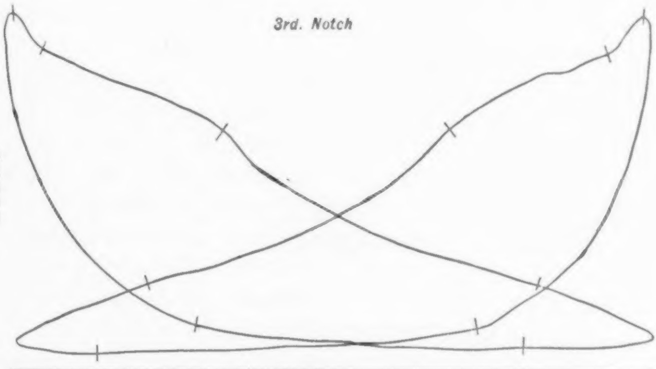

FIG. 314.—No. 5, Loco. 129, July 26. Time, 12.05 P.M. Scale, 80.

B. P............160
Chest P.........146
R. P. M........192
M. E. P. front..49.9
M. E. P. crank .51.1

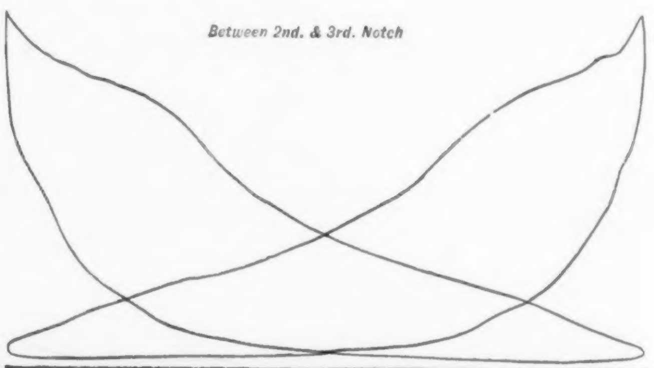

FIG. 315.—No. 6, Loco. 129, July 26. Time, 12.27½ P.M. Scale, 80.

B. P 155
Chest P. 145
R. P. M 212
M. E. P. front . . 40.3
M. E. P. crank 39.8

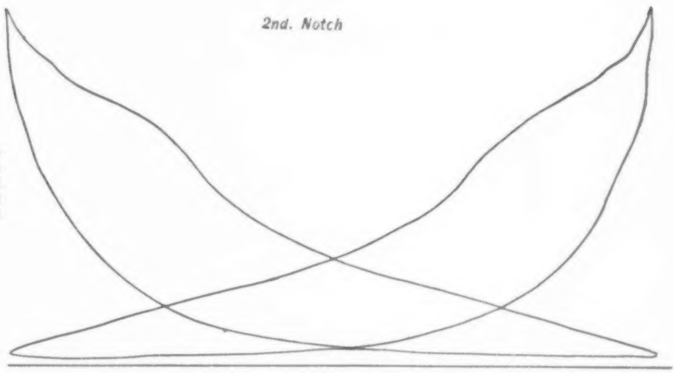

FIG. 316.—No. 7, Loco. 129, July 26. Time, 3.32½ P.M. Scale, 80.

B. P. . . . . 150 (?)
Chest P. . . . . . 149 (?)
R. P. M 228
M. E. P. front . . 26.3
M. E. P. crank . 25.6

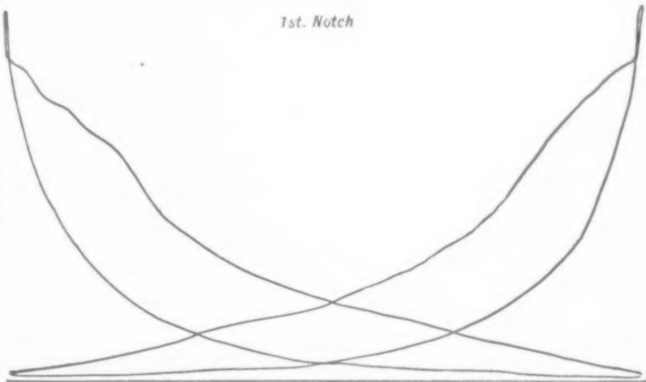

FIG. 317.—No. 8, Loco. 129, July 26. Time, 1.15 P.M. Scale, 80.

B. P 155
Chest P. 149
R. P. M 208
M. E. P. front . . 13.7
M. E. P. crank . 14.9

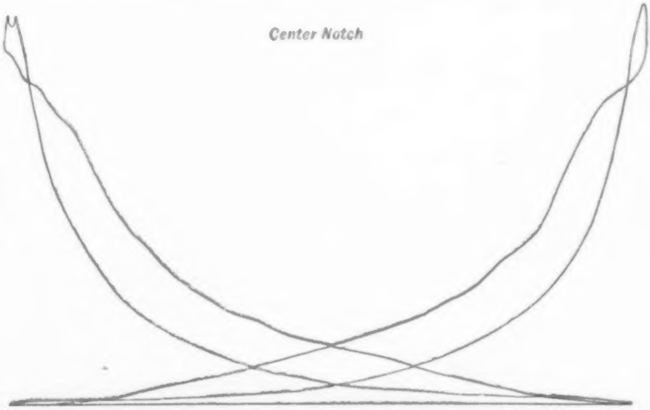

FIG. 318.—No. 9, Loco. 129, July 26. Time, 5.45 P.M. Scale, 80.

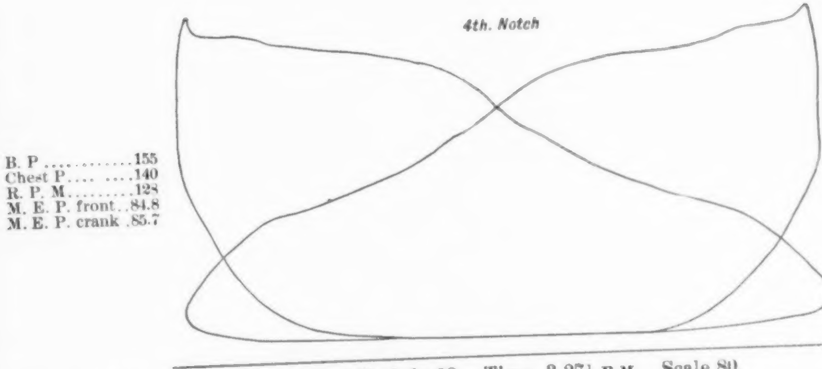

FIG. 319.—No. 10, Loco. 169, July 28. Time, 3.27½ P.M. Scale 80.

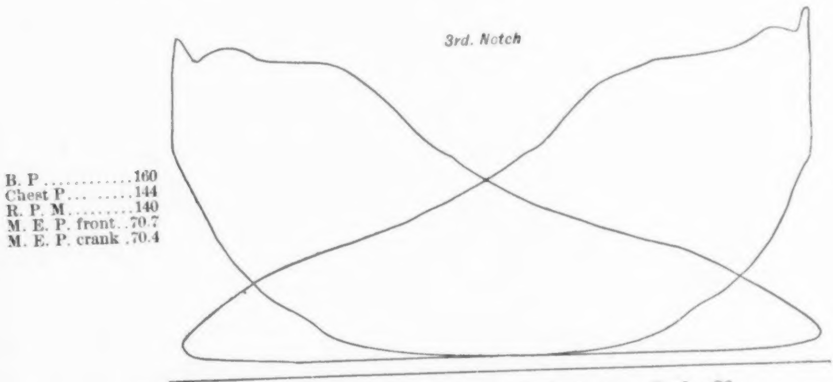

FIG. 320.—No. 11, Loco. 169, July 28. Time, 3.30 P.M. Scale, 80.

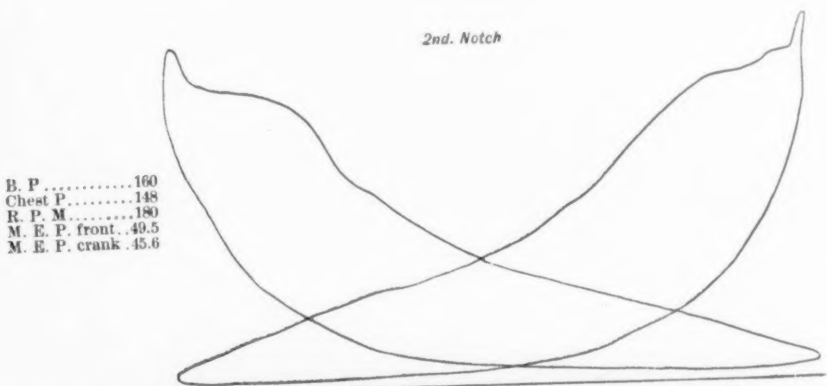

FIG 321.—No. 12, Loco. 169, July 28. Time, 12.17½ P.M. Scale, 80.

B. P 155
Chest P. 140
R. P. M. 216
M. E. P. front. .47.2
M. E. P. crank .45.6

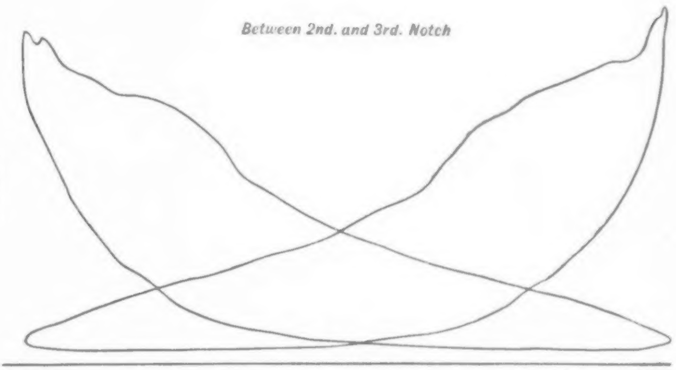

FIG. 322.—No. 13, Loco. 169, July 28. Time, 5.42½ P.M. Scale, 80.

B. P 160
Chest P. 144
R. P. M. 218
M. E. P. front. .39.2
M. E. P. crank .37.4

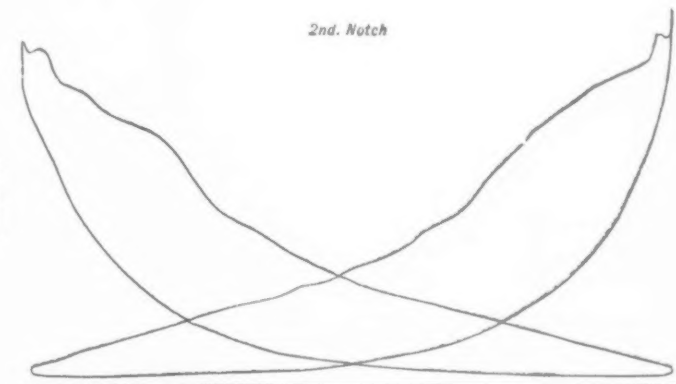

FIG. 323.—No. 14, Loco. 169, July 28. Time, 11.42½ A.M. Scale, 80.

B. P 160
Chest P. 142
R. P. M 192
M. E. P. front. .32.6
M. E. P. crank .32.4

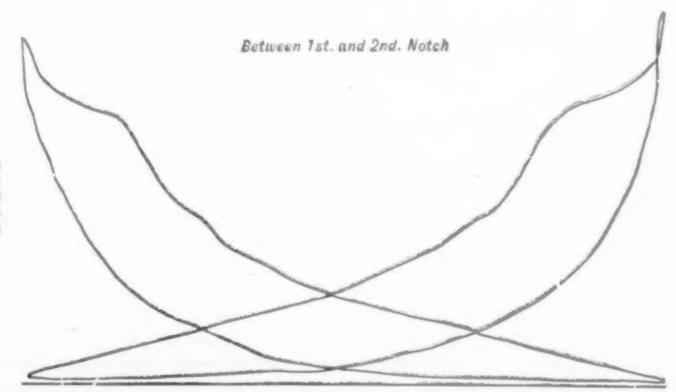

FIG. 324.—No. 15, Loco. 169, July 28. Time, 11.15 A.M. Scale, 80.

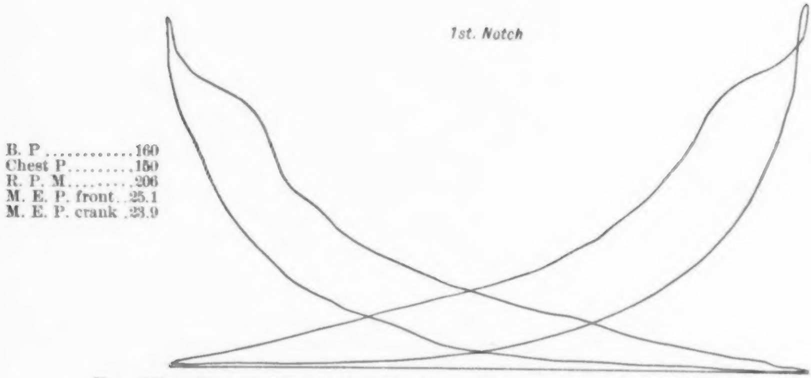

FIG. 325.—No. 16, Loco. 169, July 28. Time, 5.22½ P.M. Scale, 80.

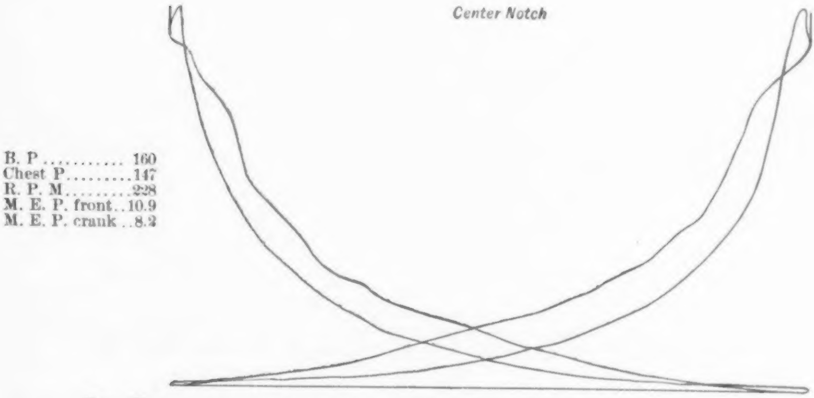

FIG. 326.—No. 17, Loco. 169, July 28. Time, 5.15 P.M. Scale, 80.

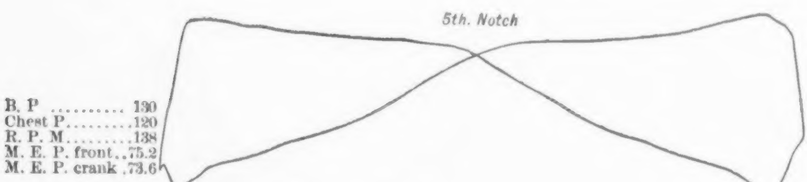

FIG. 327.—No. 18, Loco. 150, July 20. Time, 3.27½ P.M. Scale, 80

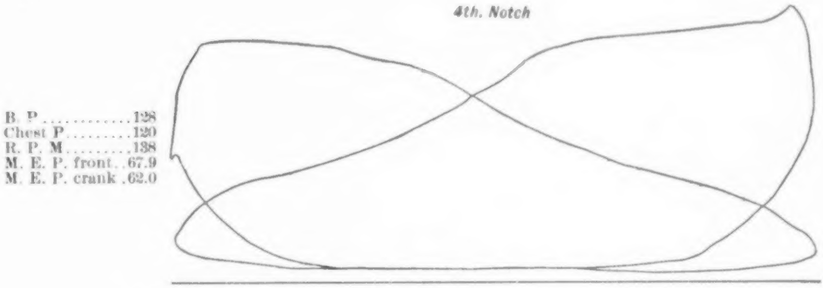

FIG. 328.—No. 19, Loco. 150, July 20. Time, 3.30 P.M. Scale, 80.

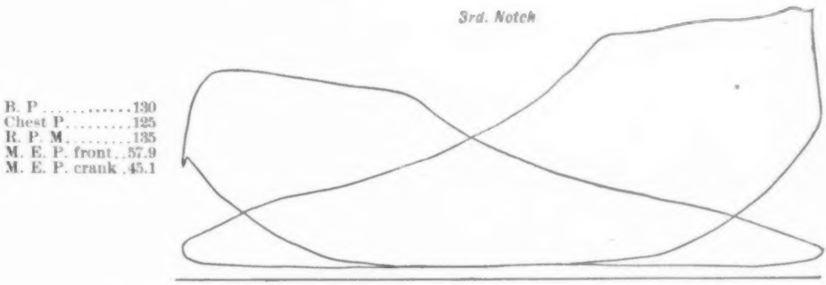

FIG. 329.—No. 20, Loco. 150, July 20. Time, 11.36 A.M. Scale, 80.

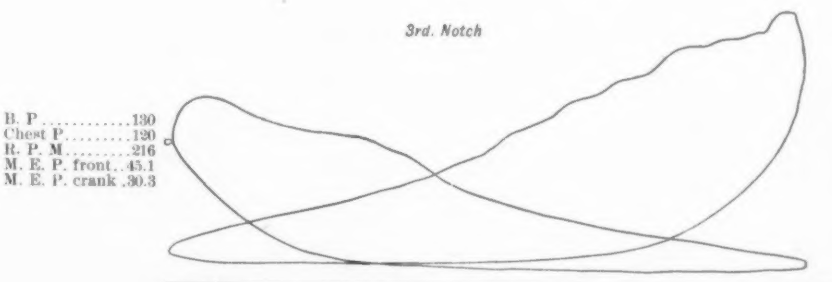

FIG. 330.—No. 21, Loco. 150, July 20. Time, 11.20 A.M. Scale, 80.

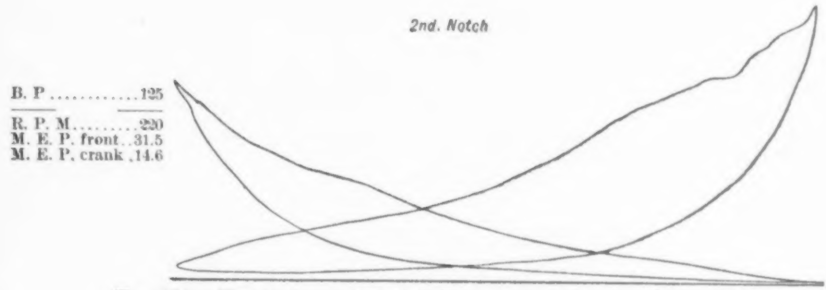

FIG. 331.—No. 22, Loco. 150, July 20. Time, 5.55 P.M. Scale, 80.

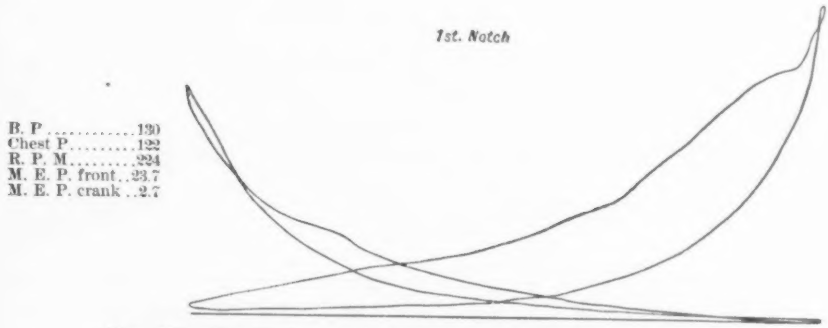

FIG. 332.—No. 23, Loco. 150, July 20. Time, 5.10 P.M. Scale, 80.

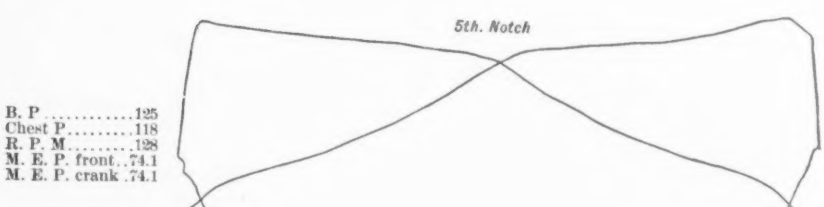

FIG. 333.—No. 24, Loco. 150, July 21. Time, 4.32½ P.M. Scale, 80.

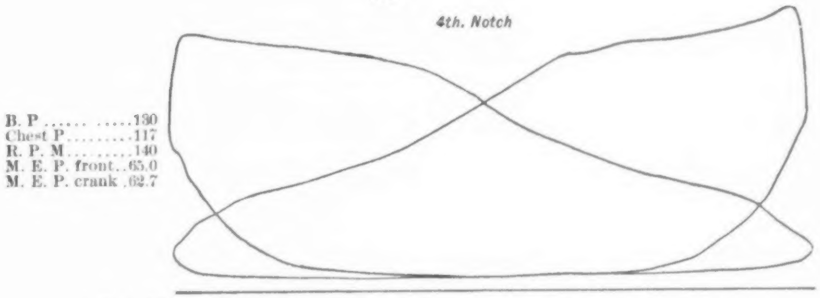

FIG. 334.—No. 25, Loco. 150, July 21. Time, 4.30 P.M. Scale, 80.

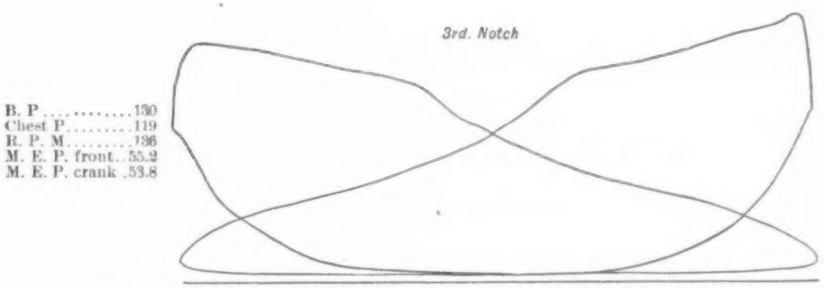

FIG. 335.—No. 26, Loco. 150, July 21. Time, 12.37½ P.M. Scale, 80.

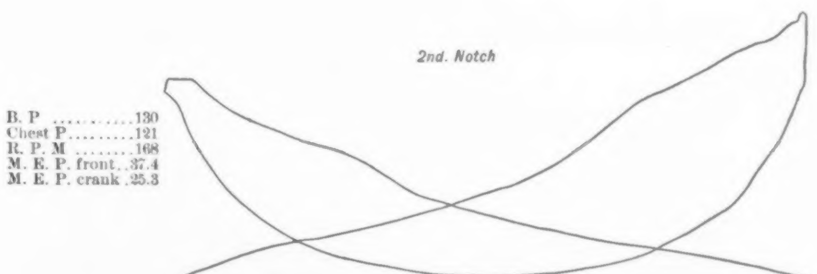

FIG. 336.—No. 27, Loco. 150, July 21. Time, 11.52½ A.M. Scale, 80.

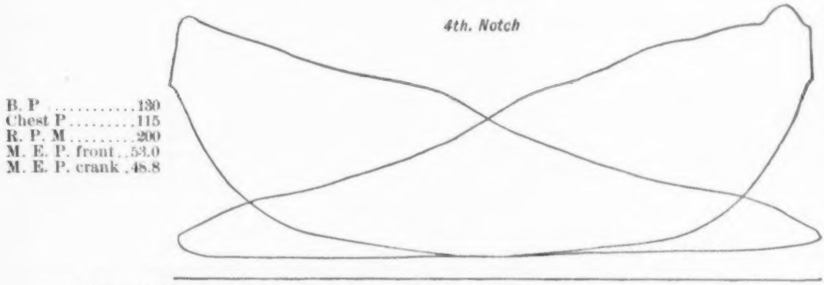

FIG. 337.--No. 28, Loco. 150, July 21. Time, 12.05 P.M. Scale, 80.

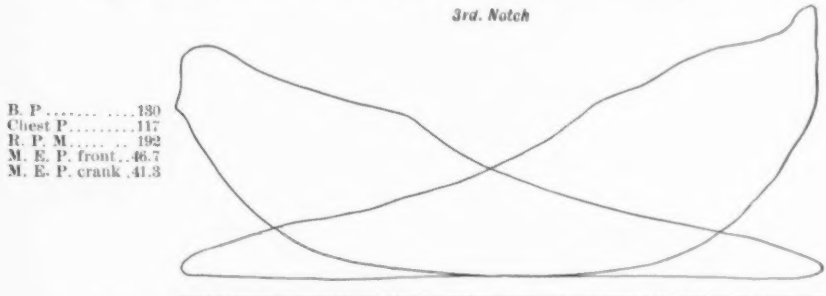

FIG. 338.—No. 29, Loco. 150, July 21. Time, 11.20 A.M. Scale, 80.

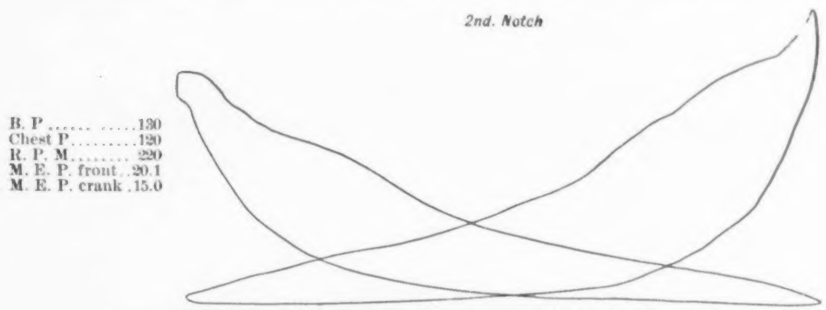

FIG. 339.—No. 30, Loco. 150, July 21. Time, 11.30 A.M. Scale, 80.

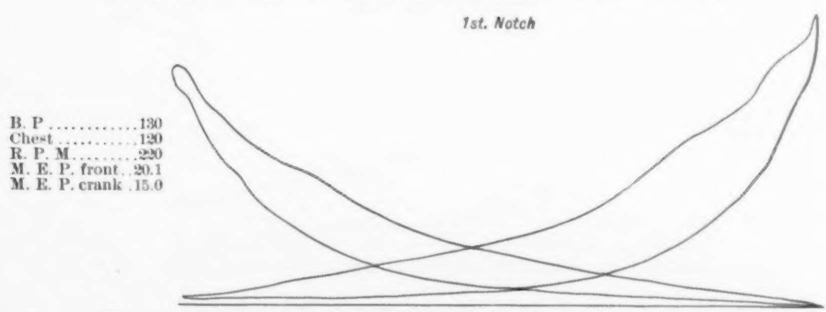

FIG. 340.--No. 31, Loco. 150, July 21. Time, 11.30 A.M. Scale, 80.

TABLE I.

DIMENSIONS OF BOILERS.

Name of Locomotive.	No. 150.	No. 129 and No. 169.
Diameter of shell.....in.	50	52
Length between tube sheets.....	$11'\ 1''$	$10'\ 11\frac{1}{4}''$
Length of fire-box.....in.	$65\frac{1}{4}$	$71\frac{1}{8}$
Width of fire-box at bottom.....in.	$35\frac{1}{4}$	$35\frac{1}{4}$
Width of fire-box at crown, front.....in.	$45''$	$46\frac{1}{4}$
" " " back.....in.	$39\frac{1}{2}$	$39\frac{3}{4}$
Depth of fire-box.....in.	$63\frac{1}{2}$	$70\frac{1}{2}$
Number of 2-inch tubes.....	175	221
Length of tubes.....	$11'—1''$	$10'—11\frac{1}{4}''$
Inside diameter of tubes.....	$1\frac{7}{8}$	1
Diameter of stack.....in.	$14\frac{3}{4}$	$14\frac{3}{4}$
Area of heating surface (exposed to products of combustion).....sq. ft.	1012.0	1245.0
Area of grate surface.....sq. ft.	15.99	17.41
Area for draught through tubes.....sq. ft.	2.92	3.69
Area for draught through stack.....sq. ft.	1.19	1.19
Ratio of heating surface to grate surface.....	63.3 to 1	71.5 to 1
Ratio of grate surface to area through tubes.....	5.48 to 1	4.71 to 1
Ratio of grate surface to area through stack.....	13.4 to 1	14.6 to 1
Number of sq. ft. of heating surface per horse power when engine indicates 500 H. P.....	2.02	2.49
Kind of grates.....	Tupper	Tupper

DIMENSIONS OF THE ENGINES.

	NAME OF ENGINE.		
	No. 129.	No. 169.	No. 150.
	Inches.	Inches.	Inches.
Diameter of cylinder.....	18	18	18
Stroke of piston.....	22	22	22
Diameter of piston rod.....	$2\frac{11}{16}$	$2\frac{5}{8}$	$2\frac{5}{8}$
Clearance, expressed in fractions of the piston displacement (completed).....	.07	.07	.07
Size of steam ports.....	$1 \times 14\frac{3}{4}$	$1\frac{7}{32} \times 14\frac{3}{4}$	$1 \times 14\frac{3}{4}$
Size of exhaust port.....	$2\frac{3}{4} \times 14\frac{3}{4}$	$2\frac{3}{4} \times 14\frac{3}{4}$	$2\frac{3}{4} \times 14\frac{3}{4}$
Width of bar.....	1	1	1
Travel of the valve.....	$4\frac{3}{4}$	$4\frac{1}{4}$	$4\frac{3}{4}$
Outside lap.....	$\frac{7}{8}$	$\frac{27}{32}$	$\frac{15}{16}$
Inside lap.....	$\frac{1}{32}$ blind	line to line	line to line
Lead in full gear.....	$\frac{1}{32}$	$\frac{1}{32}$	none
Lead in mid gear.....	$\frac{1}{4}$	$\frac{1}{4}$	$\frac{3}{16}$
Diameter of dry pipe.....	5	5	5
Diameter of steam pipe each cylinder.....	4	4	4
Diameter of each exhaust tip.....	$3\frac{3}{16}$	$3\frac{1}{8}$	3
Diameter of petticoat pipe.....	10	10	9
Packing of piston.....	Wheelock	Wheelock	Wheelock
Outside diameter of driving wheels.....	$5'\ 8\frac{3}{4}''$	$5'\ 8\frac{3}{4}''$	$5'\ 8\frac{3}{4}''$

TABLE II.

DATA AND RESULTS OF THE EVAPORATIVE PERFORMANCE.

	Name of Locomotive.	No. 129.		No. 169.		No. 150.		No. 129.		No. 169.	
	Date of Test. 1882.	July 11.	July 12.	July 13.	July 14.	July 20.	July 21.	July 25.	July 26.	July 28.	July 29.
1	Duration of test ... hrs.	4.17	4.17	4.17	4.17	4.50	4.50	4.50	4.50	4.50	4.50
2	Weight of wood for lighting fire ... lbs.	420	420	387	415	350	350	350	350	350	350
3	Equivalent value of wood = wt. × $\frac{4}{10}$ ... lbs.	168	168	155	166	140	140	140	140	140	140
4	Weight of coal fired ... lbs.	8,707	8,770	8,284	7,594	9,260	8,174	7,377	7,033	7,214	6,656
5	Unconsumed coke in cinders and refuse ... lbs.	60	64	56	43	40	42	94	47	50	38
6	Weight of coal consumed ... lbs.	8,647	8,706	8,228	7,551	9,220	8,132	7,283	6,986	7,164	6,618
7	Weight of fuel consumed (includes wood) ... lbs.	8,815	8,874	8,383	7,717	9,360	8,272	7,423	7,126	7,304	6,758
8	Weight of fuel consumed per hour ... lbs.	2,114	2,128	2,010	1,851	2,080	1,838	1,650	1,584	1,623	1,502
9	Weight of fuel per hour per sq. ft. grate ... lbs.	121.5	122.2	115.5	106.4	130.0	114.9	91.8	91.0	93.3	86.3
10	Weight of cinders and ashes ... lbs.	...	505	502	406	354	336	269	264	272	202
11	Percentage of cinders and ash to total consumption ... %	...	5.7	6.0	5.3	3.8	4.1	3.6	3.7	3.7	3.0
12	Weight of combustible consumed ... lbs.	...	8,369	7,881	7,311	9,006	7,936	7,154	6,862	7,032	6,555
13	Number of inches of water fed ... in.	90.4	92.9	93.3	90.8	99.9	91.7	90.1	90.2	91.5	90.0
14	Number of pounds of water fed ... lbs.	47,805	49,127	49,322	48,001	52,817	48,482	47,636	47,689	48,376	47,583
15	Number of pounds fed per hour ... lbs.	11,464	11,781	11,828	11,511	11,737	10,774	10,586	10,598	10,750	10,574
16	Weight fed per hour per sq. ft. heating surface, lbs.	9.21	9.46	9.50	9.25	11.60	10.65	8.50	8.51	8.63	8.49
17	Horse power on basis of 30 lbs. fed per H. P. per hour ... H. P.	382.1	392.7	394.3	383.7	391.2	359.1	352.9	353.3	358.3	352.5
18	Average boiler pressure ... lbs.	155	155	155	155	126	129	154	155	156	157
19	Approximate temperature of feed water ... deg.	72	72	72	72	72	72	72	72	72	72
20	Water evaporated * per pound of fuel ... lbs.	5.42	5.54	5.88	6.22	5.64	5.86	6.42	6.69	6.62	7.04
21	Water evaporated per pound of combustible ... lbs.	...	5.87	6.26	6.57	5.86	6.11	6.66	6.95	6.87	7.26
22	Equivalent evaporation per lb. fuel with feed 212° ... lbs.	6.17	6.30	6.69	7.08	6.42	6.67	7.31	7.61	7.53	8.01
23	Equivalent evaporation per lb. combustible from and at 212° ... lbs	...	7.01	7.48	7.85	6.97	7.27	7.92	8.27	8.17	8.64
24	Average indicated horse power ... H. P.	...	...	...	...	...	428.4	...	431.7	...	439.4
25	Cars hauled from Boston to Springfield ...	...	...	...	...	6 N. Y.	6 B. & A.	6 N. Y.	6 B. & A.	6 B. & A.	6 N. Y. 1 B. & A.
26	Cars hauled from Springfield to Boston ...	...	...	...	...	7 B. & A. 2 Fram.†	7 N. Y. 1 Fram.	7 B. & A. 1 Fram.	7 N. Y. 1 Fram.	7 N. Y. 1 Fram.	7 B. & A. 1 Fram.
72	Time of leaving Boston ...	4.30 p.m.	4.30 p.m.	4.30 p.m.	4.30 p.m.	11.00 a.m.	11.00 a.m.	11.00 a.m.	11.00 a.m.	11.00 a.m.	11.00 a.m.

* This in reality is water *fed*. Except for the waste by the injector overflow, it is actual evaporation.

† The term "Fram." refers to cars token on at So. Framingham.

TABLE III.

DATA AND RESULTS OF THE EVAPORATIVE PERFORMANCE IN THE CYLINDERS.

Name of Engine.	No. 129.	No. 169.	No. 150.
Date of Test.	July 26.	July 28.	July 21.
1. Portion of the route	Spr. to E. Br.	Spr. to E. Br.	Spr. to E. Br
2. Duration of the test..........hrs.	0.842	0.867	0.871
3. Number of inches of feed water consumed ....in.	23.34	23.04	24.44
4. Number of pounds of feed water consumed..................lbs.	12,340	12,181	12,921
5. Consumption of feed water per hour ....lbs.	14,632	14,050	14,829
6. Average boiler pressure........lbs.	156	156	129
7. Average pressure in right hand chest ....lbs.	146	144	119
8. Average initial pressure in cylinder above atmosphere ....lbs.	150.0	151.6	120.0
9. Average normal initial pressure in cylinder above atmosphere.. lbs.	131.0	133.1	107.5
10. Average pressure at cut-off above atmosphere................lbs.	98.2	103.9	80.4
11. Average pressure at cut-off above zero ....lbs.	112.9	118.6	95.1
12. Average pressure at release above zero ....lbs.	53.5	54.5	48.3
13. Pressure at release measured on a hyperbolic curve passing through point of cut-off .... lbs.	54.5	54.7	49.3
14. Average pressure at compression point above zero ....lbs.	29.6	31.3	30.1
15. Average back pressure at lowest point above atmosphere .....lbs.	5.5	6.9	7.4
16. Proportion of direct stroke completed at cut-off.................	.329	.317	.416
17. Actual cut-off (clearance 7%).......	.373	.362	.454
18. Proportion of direct stroke completed at release	.756	.771	.867
19. Proportion of return stroke uncompleted at compression...........	.276	.303	.221
20. Av. mean effective pressure....lbs.	54.43	53.86	51.58
21. Average speed in revolutions per minute	189.3	190.0	190.0
22. Av. indicated horse power... H. P.	570.5	573.2	549.5
23. Consumption of feed water per I. H. P. per hour ....lbs.	25.65	24.51	27.00
24. Steam accounted for by diagram at cut-off per I. H. P. per hour.lbs.	19.63	19.39	22.71
25. Steam ac. for by diagram at release per I. H. P. per hour...lbs.	20.30	20.59	23.27
26. Proportion of feed ac. for by diagram at cut-off.................	.765	.791	.841
27. Proportion of feed acct. for by diagram at release.................	.791	.810	.861
28. Cars..............................	7 N. Y	7 N. Y.	7 N. Y.

NOTE.—The points where the various pressures given above are measured are represented upon diagram No. 5.

TABLE IV.

DATA AND RESULTS OF TEST OF LOCOMOTIVE NO. 169, SEPT. 29, 1888.

	Round Trip.	Springfield to Worcester.
TOTAL QUANTITIES.		
1. Duration equals length of time throttle valve was open ....hrs.	4 15	1.12
2. Total consumption of Clearfield bituminous coal..lbs	8,533	....
3. Ashes ....lbs.	359	....
4. Total weight of feed water consumed ....lbs.	49,381	16,850
5. Average indicated horse power developed .....H. P.	502 9	631.6
6. Average boiler pressure ....lbs.	155.7	157
7. Average back pressure at lowest point........lbs.	4.9	....
8. Average mean effective pressure ....lbs.	47.36	63.34
9. Average number revolutions per minute	190	178.5
10. Temperature of feed water in tank........deg. F.	65	....
11. Average indication of draught gauge........in.	3.26	....
12. Maximum " " "........in.	7.5	....
13. Minimum " " "........in.	0.5	....
14. Average indication of pyrometer in smoke arch deg. F.	689	....
15. Maximum indication of pyrometer in smoke arch deg. F.	850	....
16. Minimum indication of pyrometer in smoke arch. deg. F.	520	....
HOURLY QUANTITIES.		
17. Coal per hour ....lbs.	2,056	....
18. " " " per sq. ft. of grate ....lbs.	118.2	....
19. " " " per indicated horse power........lbs.	4.9	....
20. Feed water per hour ....lbs.	11,899	15,044
21. " " " " per I. H. P. ....lbs.	23.66	23.82
22. Steam accounted for by sample diagram at cut-off. lbs.	....	19.80
23. Water evaporated per pound of coal........lbs.	5.79	....

Mr. J. S. Coon.—Mr. Barrus has called attention to the fact that the coal used on the Boston and Albany Railroad was a very high quality of bituminous coal, whereas the coal used on No. 383 on the Lehigh Valley was anthracite of ordinary grade, and he further states that the true test of economy of the two engines should be based on the quantity of feed water, just as the economy of the stationary engine should be based on the steam which it uses. We had an excellent opportunity in those tests on the Lehigh Valley to test that fact. We ran from Sugar Notch to Glen Summit for about twelve miles on a grade averaging about 96 feet to the mile, and these engines drew on the the several tests exactly the same train, consisting of five cars, the weight of which, with the passengers, baggage, etc., was about 421,000 pounds. Taking an average of the feed water

consumed by No. 383, fitted with the Strong valve gear, and the other, fitted with an ordinary Stephenson link and balanced valve, the difference in the quantity of water consumed—the average—was $20\frac{3}{10}$ per cent. in favor of the Strong valve gear. No. 444 consumed more water than No. 383. That was fitted not only with the Strong valve gear, but the Strong boiler. The engineer was afraid to carry the water between the first and second gauge cock. He almost always had water in the third gauge cock. I think that the standard of comparison should be made between the two types simply on the consumption of feed water while the types of boiler were practically the same.

Mr. Barrus.—Mr. Coon states in a printed report that engine No. 444 had trouble from water priming over into the cylinders, and that the steel admission valves of the engine leaked. I think it is stated in the same report that the boiler frequently blew off during the tests. Now the same kind of comparison which I made on No. 383 shows that the steam accounted for by the indicator was 97 per cent. of the feed water consumption. This allows a margin of only three per cent. for cylinder condensation, leakage, priming and blowing off. I think there is some discrepancy here.

Mr. Coon.—It must be distinctly borne in mind that the figure given by Mr. Barrus is his figure, not mine.

Mr. Barrus.—I based my computation on the published figures given in Mr. Dean's paper. I do not know what else we have to go by. I should like to have some explanation of that discrepancy.

Mr. Coon.—I shall also have to decline to be responsible for Mr. Dean's figures.

Mr. Barrus.—Mr. Coon does not feel disposed to answer this matter, and so I will assist him, if he will allow me. I understand that the method employed in conducting these tests—that is, in working up the amount of power developed by the engine—was this: It appears that some of the runs—in fact, on a good many of the runs—the time was about three minutes, in some cases two minutes, in some cases four or five minutes, and in some cases a longer time, between the stops at the stations. Some of these runs are reported as of twenty minutes total duration, and in that time there were three or four stops. Now, in working up the cards I understand that no cards were figured that were taken before the engine had attained its full working speed, and that the whole time of the run was taken from the time the engine started until the time the engine stopped, no allowance being made for the time that the

throttle valve was shut. The power developed from the time the engine started until the time the engine reached its normal speed was thrown out, and the assumption is made that the power developed during that time is the average developed while the engine was in normal working, and also that the power developed during the remaining time, after the throttle valve was shut until the engine was brought to rest, was also that same average. In reality, during this last period there was no power developed. I think the difficulty is that the duration of the various periods of the test has been taken too short. For this reason the quantity of water consumed per hour comes out too small. Probably also, for the reason given, the average indicated power is too large. I think that this explains the discrepancy.

Mr. Coon.—I agree with Mr. Barrus that the time during which the tests were conducted was too short to get a very valuable record of the indicated power of the engines, and if Mr. Barrus will remember how the report is written—my report to Mr. Leavitt—he will recall that no attempt has been made to account for the quantity of feed water in the cards, from the fact that I did not myself consider that the runs were of sufficient length to warrant that kind of investigation. The object of the tests was simply and only to get a comparison between the Strong type and the other type, and no matter what system was adopted, we could get a comparative record of the types. I will not say that it is proper to take the records we obtained and compare them with the records obtained by Mr. Barrus, which might have been conducted under more favorable circumstances.

Mr. W. F. Mattes.—I did not understand that Mr. Coon's test of the comparative merits of the Strong and ordinary valve gear on the runs stated, from Sugar Notch to Glen Summit, was intended to be a scientific test. I understood it was intended to be a practical comparison between these two types of gear under as nearly as practicable identical conditions. While the test may not give us scientific facts, it seems to me that there is food for thought for engineers interested in these various types of gear, in the fact that there was so large a difference developed in the quantity of water consumed by these two engines on that identical run. When a difference so large as twenty per cent. in the quantity of feed water consumed in the run is shown, there must be something radical about that gear to produce it. We can eliminate a great deal for errors of observation, etc., and still have left a large figure. Every

engineer familiar with the subject knows that a large locomotive of eighteen or nineteen inches cylinder, by say twenty-four inch stroke, pulling a heavy passenger train at the highest possible speed for something like twelve or thirteen miles against a ninety-six foot grade, will use a very large quantity of water in that time. The quantities consumed are large enough, taken in connection with a difference of twenty per cent. to give us something that for practical purposes is really valuable.

Mr. Snell.—I would like to ask the question, During these trials was the direction and force of the wind the same? Going the speed at which the engine goes, it might be a quarter or half an ounce pressure on each square inch.

Mr. Coon.—The weather during the tests which were made during the month of May was every day just like it is to-day, or just like the weather has been this week. There was every day blowing a slight wind up the mountain, not enough to affect the results very much. There was one day when the wind blew very hard; then the consumption of feed water and coal was very much increased.

Mr. Mattes has called attention to a point about the large quantity of feed water consumed, and I will give those figures. The mean consumption of water from Mauch Chunk to Glen Summit on four runs, for 383, fitted with the Strong gear, was 17,942 pounds. For 357, fitted with the link motion it was 21,585 pounds. These figures go in conjunction with the loads I gave.

The President.—I would like to ask Mr. Coon if the locomotive which was fitted with the old type of gear was overhauled before the trial and the valves reset?

Mr. Coon.—Mr. Mitchell was asked to give us the best locomotive he had fitted with the Stephenson link motion, and he gave us this one. It had been out of the shop about two months—may be not so long as that; I would not state positively.

The President.—That is a very important matter in a test of that kind. Both engines and apparatus should be in equally good condition. Was any examination made of that gear?

Mr. Coon.—No, sir. We did not examine the valves at a close of the trial on any of the engines.*

Before we pass to the next subject, Mr. Baldwin has suggested that I call attention to the fact, which every engineer will at once

* Eng. 444, fitted with the Strong gear, had cast-steel valves and seats, which had cut badly and were leaking during the tests. These cast-steel valves and seats have been replaced by cast-iron ones.

recognize as a very important one, that the Strong locomotive labored under the disadvantage that any radical change in existing types must labor under. There was a very strong prejudice against the Strong locomotive on the part of the engineer running it. The fireman, it is true, was not prejudiced against it, but the engineer was taken from his regular locomotive. He was a large thick-set man, and the Strong locomotive was not quite so easy to get in and out of as his own locomotive. He was one of those Pennsylvania Dutchmen who are pretty hard to manage, and we could not get him to do what we wanted.

Mr. Nagle.—I suggest that the personal equation of the experts should enter into every test.

*Prof. R. H. Thurston.**—The locomotive has been at a standstill, so far as its general design is concerned, for so many years, that anything which gives any promise of bringing about a substantial and permanent advance in its construction and its economical operation should receive very respectful consideration. The standard engine of to-day is, in all important details, substantially the engine of Stephenson. Now and then an attempt has been made to effect the change of economical value in some detail of construction; and now and then an inventor, like Wootton, has actually made a radical modification of the machine for a special purpose; but the boiler of the standard engine is to-day substantially, on the whole, that of the engines of 1830 to 1840, and the engine and its valve motion have remained unchanged for nearly a half century. Mr. A. J. Stevens did some interesting work in the use of a cut-off valve, and Joy has brought into use a radically different type of valve motion; while a hundred inventors have tentatively applied their various forms of balanced valve with as various success. The standard engine, however, is practically, as it seems to-day, crystallized, perhaps fossilized.

It has seemed to me that the work of Mr. Strong has been, to say the least, thoroughly deserving of careful study, and his engine is entitled to the most complete and thorough test under all the usual conditions of every-day work. An examination of his plans and the study of the reported data obtained during the trials of his engines seem to me to indicate the probability that he has effected the purpose which he had in view so completely as to at least make it certain that his devices will have that lengthened and practical

* Contributed after adjournment.

trial which alone can determine whether the machine is so much better in design, construction and operation, under the usual conditions of service, as to give it fair chance of displacing the older type. The introduction of the corrugated flue as a fire-box cannot, I presume, be expected to give any observable gain in the economical working of a clean boiler, so far as cost of fuel is concerned; but the displacing of the old flat-topped fire-box, with its forest of stays and its grating of girders, its innumerable cages for scale and sediment, and its multitudinous opportunities for breakage and burning and corrosion, is most unquestionably a matter of importance. It is unfortunate that greater height cannot be secured by this system for combustion above the fire, but the combustion chamber beyond the grate is a very valuable compensation for that loss. The strength, stiffness, accessibility, and above all the simplicity of this construction, are, to my mind, improvements over the old design of no ordinary value. I judge from the results reported that no serious practical difficulties are found in firing these grates. That would be the first thing I should look for. With the corrugated furnace, it would seem that any increase of pressure which may seem likely to prove desirable may be adopted with safety and satisfactory final result. I do not, however, anticipate any very immediate and rapid advance in the direction of higher pressures in locomotive practice, although I have no question that increasing pressures are to be the element of further advance in that, as in any other department of steam engine work.

The other essential peculiarity of the Strong engine, its valve motion, appears to me, if a less obvious, a no less essential element of such success, as the limited experience so far had with the locomotive has indicated. The endeavor to secure a good distribution of steam in the locomotive with a fairly simple form of gearing has been in the thought of every engineer for many years. Nothing, as yet, however, has been able to displace the old Stephenson gear. If well designed and properly put together, it is simple, effective and durable beyond any other device yet tried up to date, unless it should prove that the Joy gear—which, however, gives a very similar distribution—should be finally found its superior in the latter quality.

But it is evidently certain that if we are to seek the best and an adjustable arrangement of both steam and exhaust sides, we must have independent steam and exhaust motions. It is further obvious that if we are to seek reduced clearance and greater power from

the same cylinder, we must use separate valves; while, if we desire to get maximum rapidity of ingress and exit of steam, large port area must be adopted; and this means, if much gain is sought in this direction, the use of the gridiron valve. The construction employed by the designer of this engine is thus logically correct, and it is simply to be ascertained by trial whether these desiderata are obtainable in the manner adopted without the introduction of other elements of more than compensating quality.

Of the two methods of securing reduced expenditure of power in the working of valves, that by balancing and that by adopting Corliss' idea of making their movement take place mainly when there is least pressure on them and they are of themselves most nearly balanced, I should regard the latter as the better, if a choice must be made. Experience with balanced valves indicates that they, if in good order, greatly reduce the internal resistances of the engine. This occurs in greater amount than is commonly suspected or realized by the average engineer, I think. I have in several cases found it to make the difference between one-fourth the total friction of engine and one-twentieth, or even less. But it rarely happens, I think, that the balancing device is satisfactorily and permanently reliable as actually operated by the average attendant. Either of these methods is good when well carried out, and their combination is very probably still better; but the system here used has the advantage, apparently, of giving certainty and permanence of useful result. If it suddenly opens an enormously large port, holds it open until it is time to close, and then promptly shuts off steam from the cylinder, a valve motion must necessarily give some considerable increase in power, and probably visible economy, as compared with the older forms. It requires no experimental proof, and trial is only needed to settle the question, How much is gained? The Walschaert system which is the basis of the Strong gear, so far as the initial part of the kinematic chain is concerned, has been long in use and with good results abroad, and I have no doubt will do well here. I see no reason to doubt that the arrangement, as a whole, will prove a good one.

The data and results of test here discussed seem to me, all evidence being given due weight, to indicate that the engine has not only merit, but, in important directions, great superiority. I do not remember to have elsewhere seen as good distribution of steam, as large cards and high power from equal cylinder capacity, and the economy shown appears to me excellent. Especially for a

service at high speed, and with heavy trains, I should expect this machine to prove valuable. The design appears to me sensible, ingenious, and well contrived in all main points, and I do not see any reason to anticipate any special difficulties arising from its peculiar features. It can hardly be denied that the prompt admission, high steam line, sharp cut-off, clean expansion and good exhaust with low back pressure, secured here, are advantages; the magnitude of those advantages may be questioned, and it is trials like those of Mr. Coon and others which must finally decide whether they are sufficient to pay for the slightly increased cost of the new engine, and the special disadvantages, if any, which the innovations of Mr. Strong may be found to introduce.

*Mr. Geo. S. Strong.**—I hope that Mr. Dean will have an opportunity to write a supplement to his paper, embodying results from data collected by himself in a test of No. 444, which I hope to have him conduct when she is in a condition to test, which was not the case when she was tested by Mr. Coon.

This engine, like all first attempts in making so radical a departure, had some defects, both in design and construction, the most serious of which was the workmanship of the boiler. This defect caused leakage, which interfered with the efficiency of the boiler, caused the use of a small exhaust nozzle, and thus excessive back pressure, which was detrimental to her performance. The steel valves which were used, but which have since been displaced by cast-iron ones, leaked.

The defects of the boiler have been remedied by putting in our latest improved fire-boxes, having a welded junction piece and Adamson joints, instead of lap seams, and the tubes have been provided with copper ferrules where they had none before. These changes enable the engine to steam freely with a very large nozzle, and thus to diminish the back pressure.

I hope the test referred to will be made when pulling heavy express trains on a nearly level road, where long runs can be made with but few stops. This will enable us to eliminate elements of uncertainty to a great extent, and avoid the wastes of coal and water by blowing off, which are unavoidable after passing the top of a heavy mountain grade.

There are many parts of Mr. Dean's paper which are worthy of careful study, especially those relating to early release and early

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* Added after adjournment.

compression, as these are matters which are but little understood, even by many well-informed engineers, many of whom believe them to be desirable qualities without regard to clearances, port areas and the action of the valves. A friend of mine once announced to me that he was going to build a small locomotive with four valves, steam and exhaust independently worked, just for the sake of settling the question of the amount of compression necessary for smooth working, and the absence of pounding when running fast. I informed him that he need not do that, as we were building large ones which would settle the question. This we have done, and it does not require an expert to determine the amounts of compression for different speeds and points of cut-off. The ordinary engineer quickly acquires this knowledge, and handles his engine accordingly.

As to one speaker's criticism of Mr. Dean's paper, and the data collected by Mr. Coon, it is quite evident from his remarks that he has not made himself familiar with the Strong valve gear. The accompanying diagrams (Figs. 341 to 345) of the qualities of the link gear as designed by Mr. Reuben Wells, formerly of the Louisville and Nashville R. R., and now superintendent of one of the largest locomotive works, which was presented to the Master Mechanics' Association, at Niagara Falls, in 1882, for the purpose of comparing the link motion with a type of radial gear [then under consideration], show very clearly what a good link motion will accomplish. Similar diagrams from the Strong gear are shown (Figs. 346 to 352). These diagrams refute the statements concerning release, compression and card areas, unless he meant at a cut-off at 20 inches, at which point the link does not show its defects. Both gears are all right as starting gears, but the comparison should be made with reference to their qualities as expansion gears.

It will be seen from these diagrams that with the link motion the earliest cut-off is at 4½ inches, when the exhaust takes place at 14½ inches, thus getting 10 inches of expansion. The compression at the same time begins at 14½ inches on the return stroke, or the engine is compressing on one side of the piston and exhausting on the other.

Now, let us imagine an engine carrying 160 pounds of steam, as in the case of the Boston & Albany engine, and suppose her to be working at 4½ inches cut-off, and that there was a perfect admission without wire-drawing. At the point of cut-off she would have

FIG. 341.

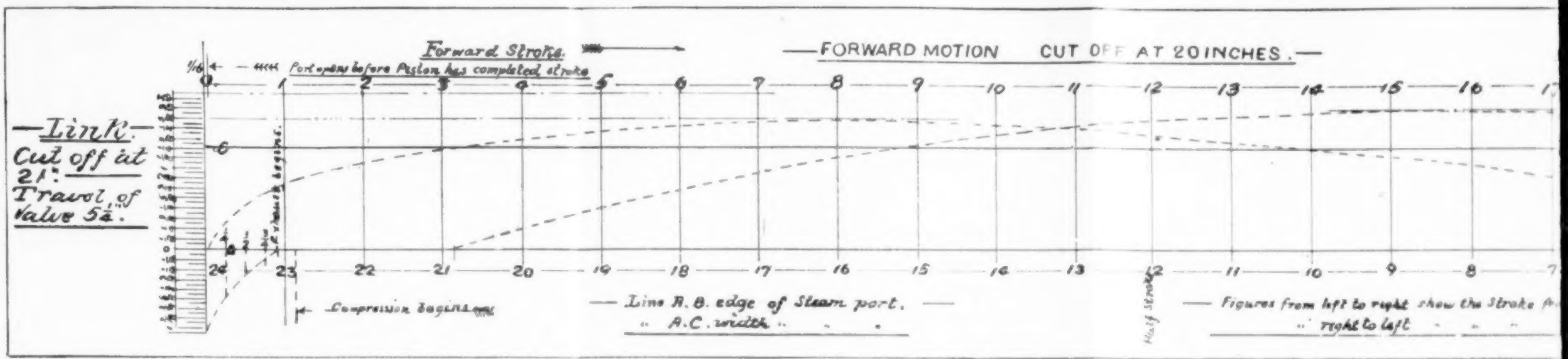


FIG. 342.

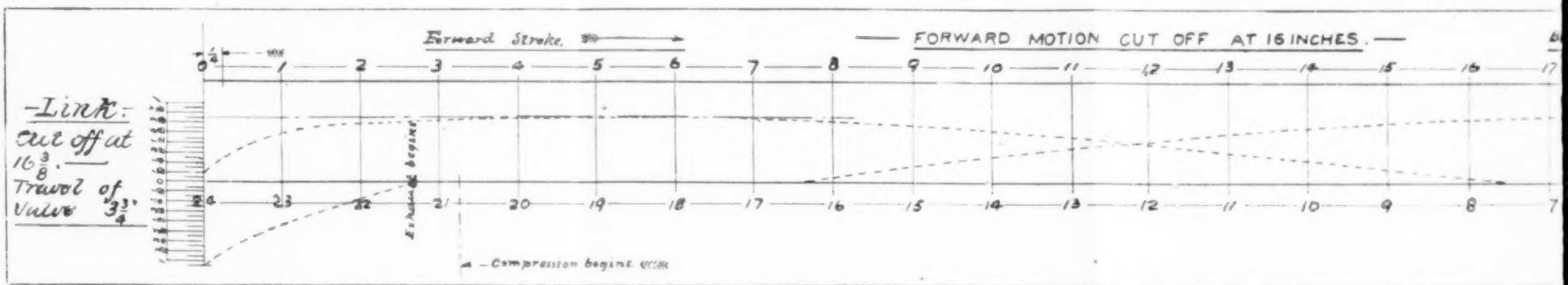


FIG. 343.

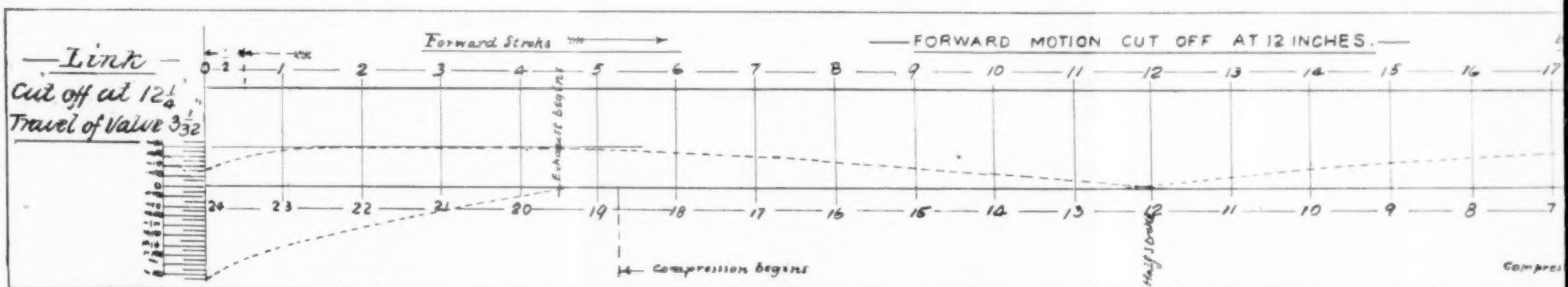


FIG. 344.

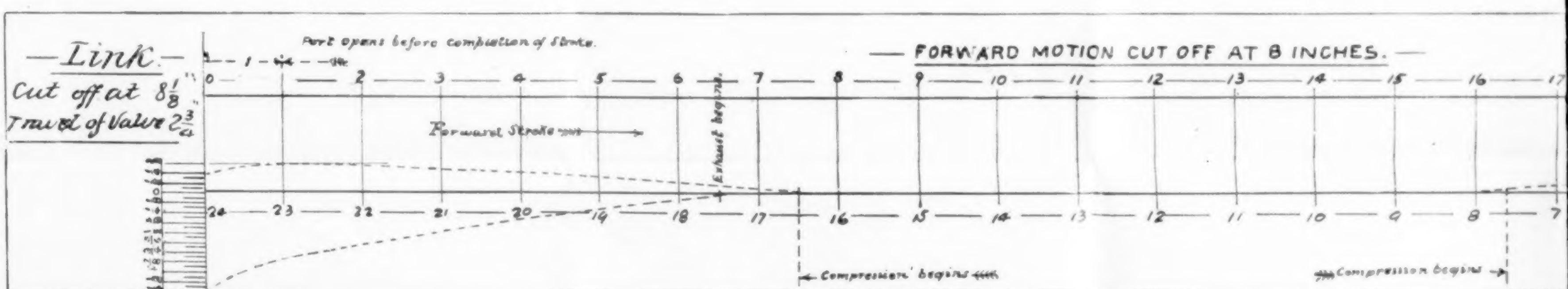
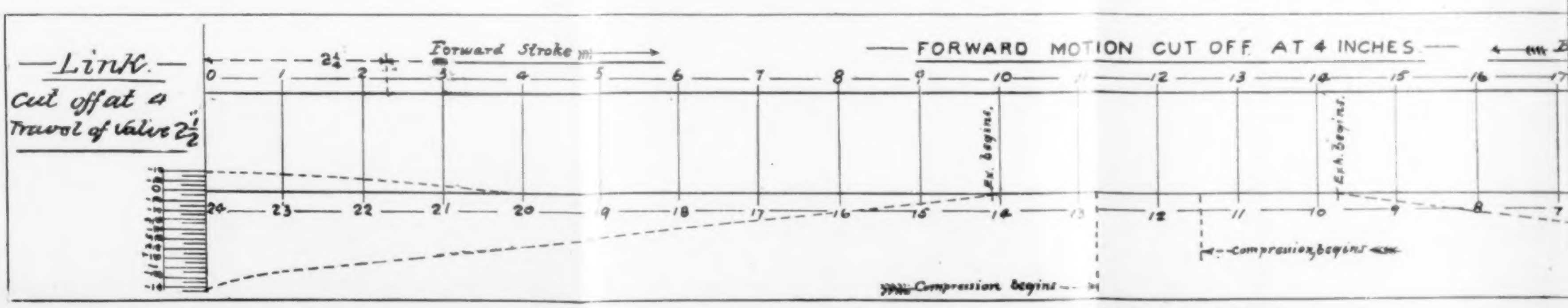
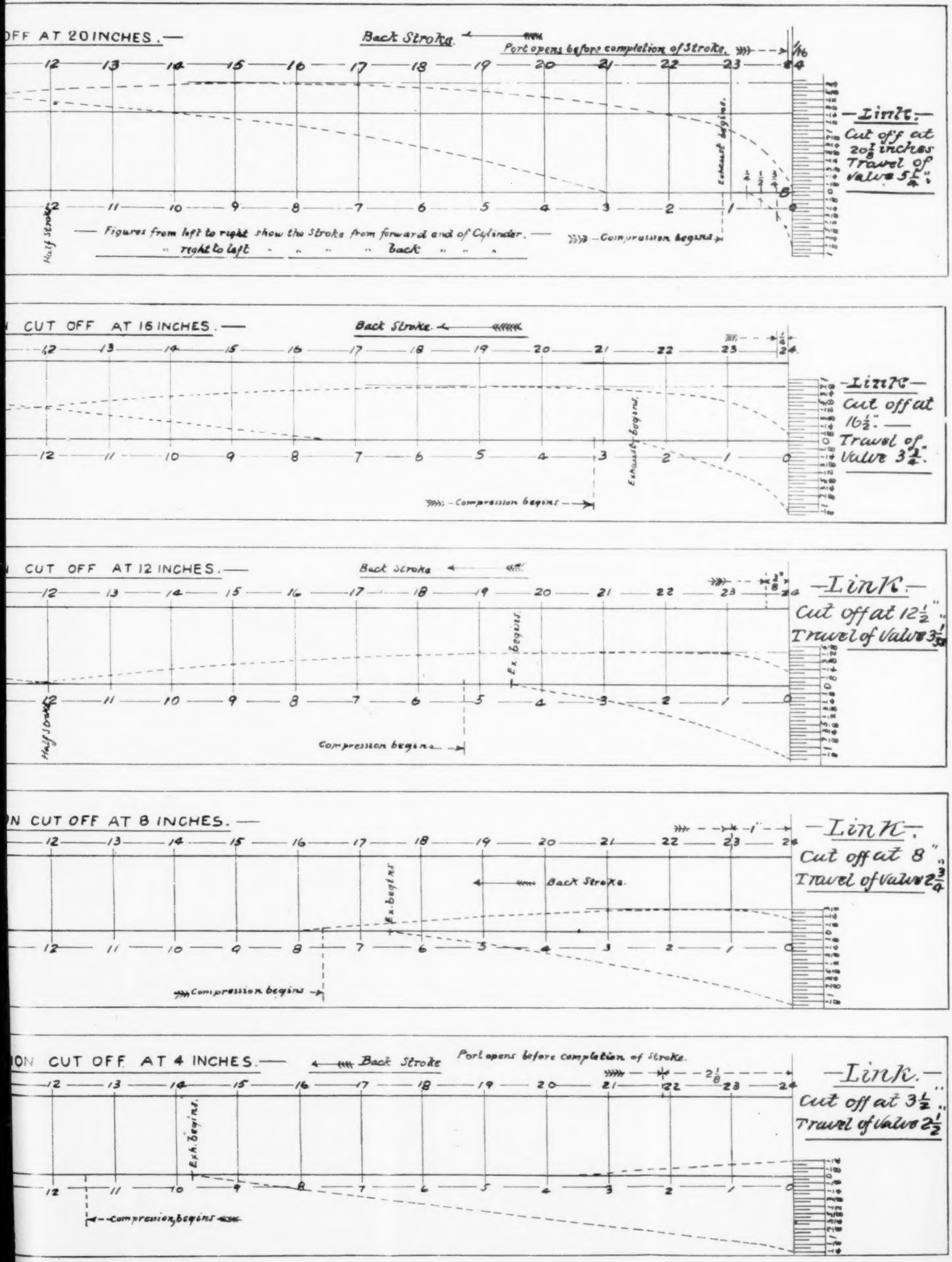


FIG. 345.



LINK MOTION DIAGRAM.



OTION DIAGRAM.

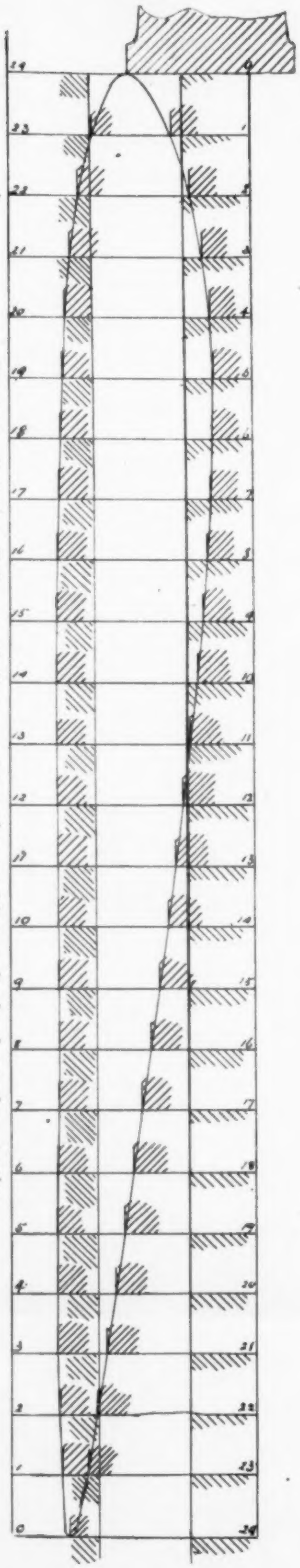


FIG. 352.—Full Gear Exhaust. Ordinary position when working engine hard, or when starting.

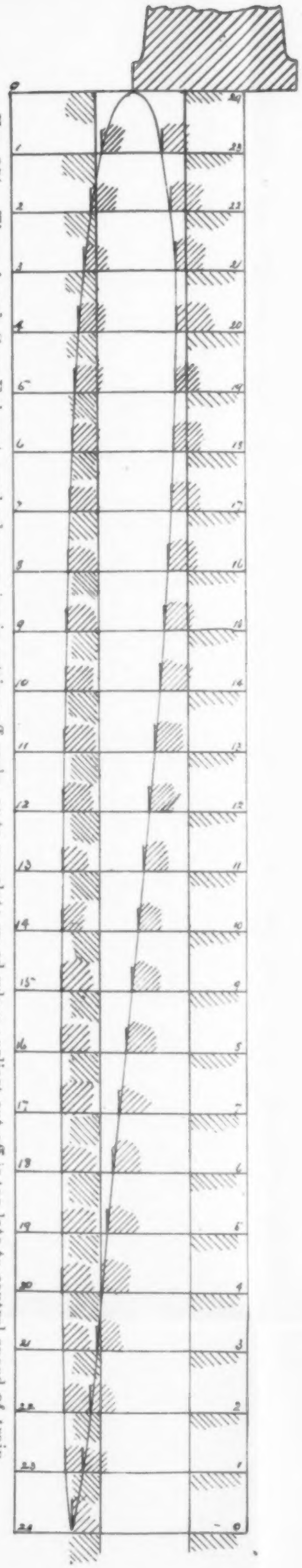


FIG. 351.—Three-fourth Gear Exhaust, used when engine is cutting off early, or to regulate speed when earliest cut-off is too late to control speed of train.

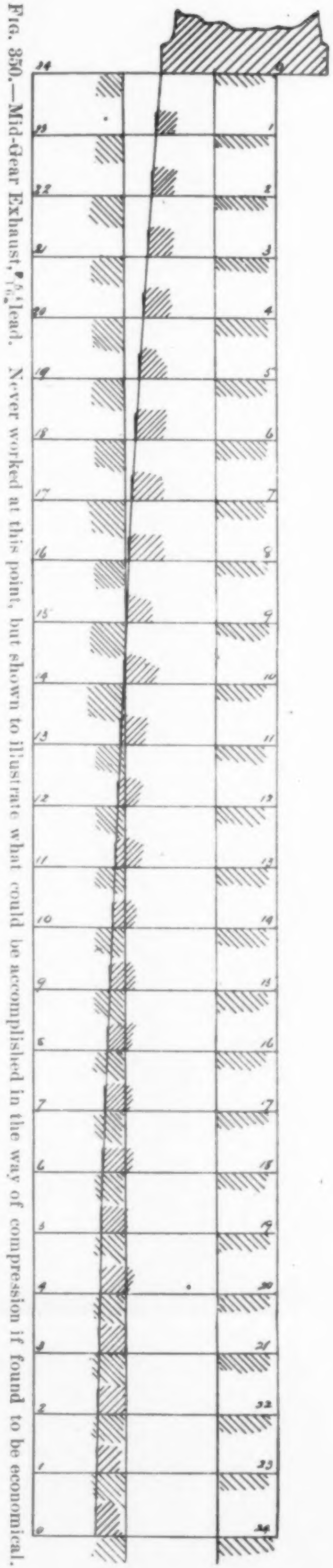


FIG. 350.—Mid-Gear Exhaust, 5/16 lead. Never worked at this point, but shown to illustrate what could be accomplished in the way of compression if found to be economical.

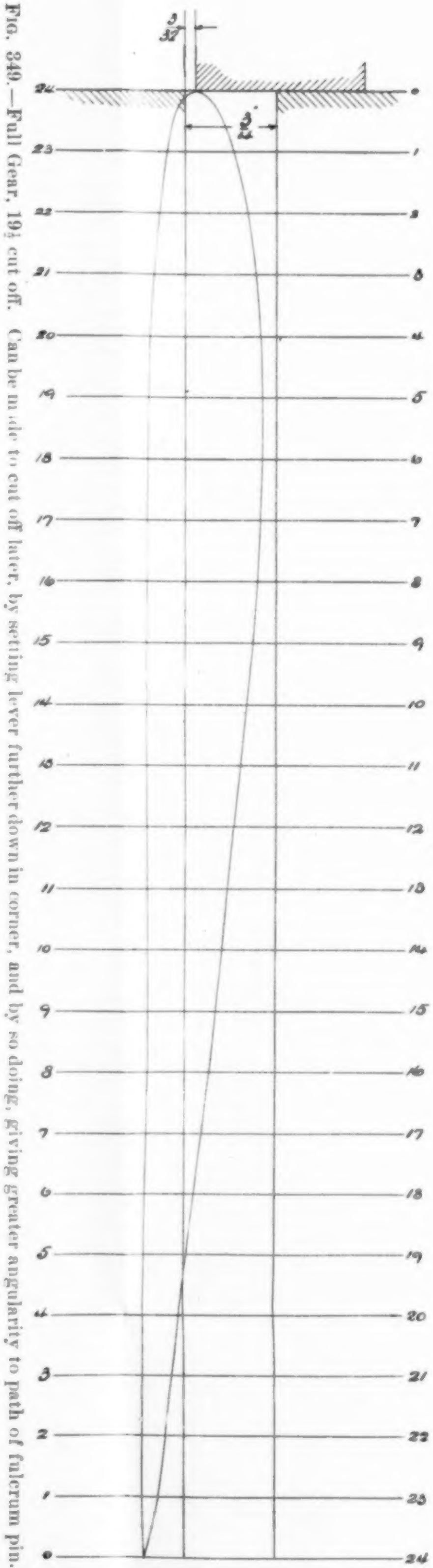


FIG. 349.—Full Gear, 19½ cut off. Can be made to cut off later, by setting lever further down in corner, and by so doing, giving greater angularity to path of fulcrum pin.

FIG. 348.—One half Gear, 12-inch cut-off.

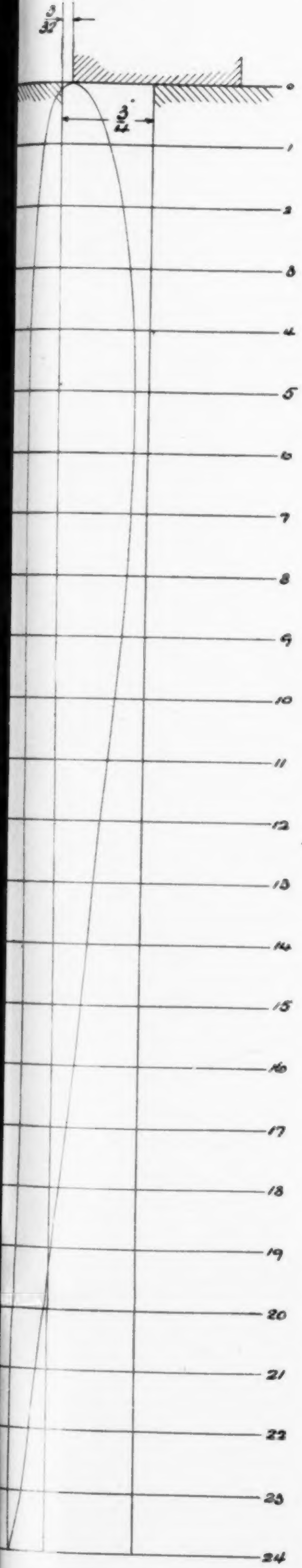


Fig. 348.—One half Gear, 12-inch cut-off.

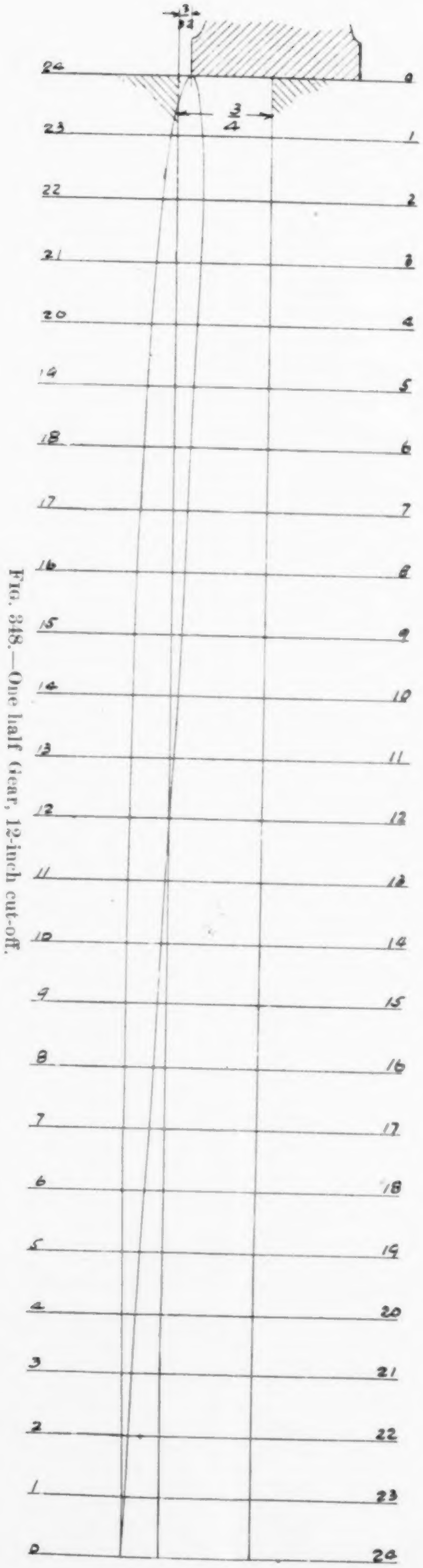


Fig. 347.—One-fourth Gear, 8-inch cut-off.

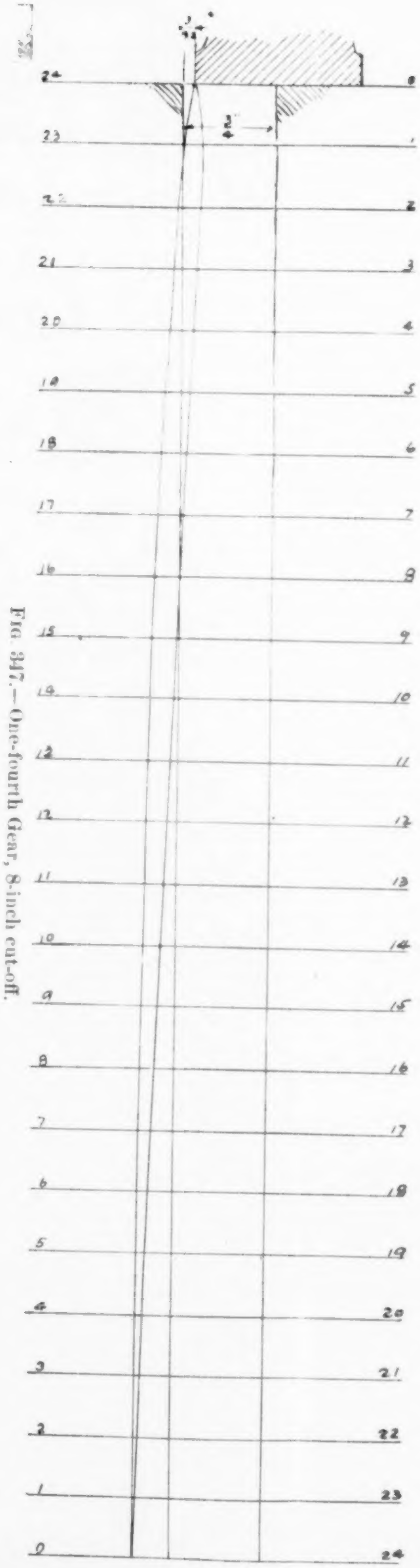
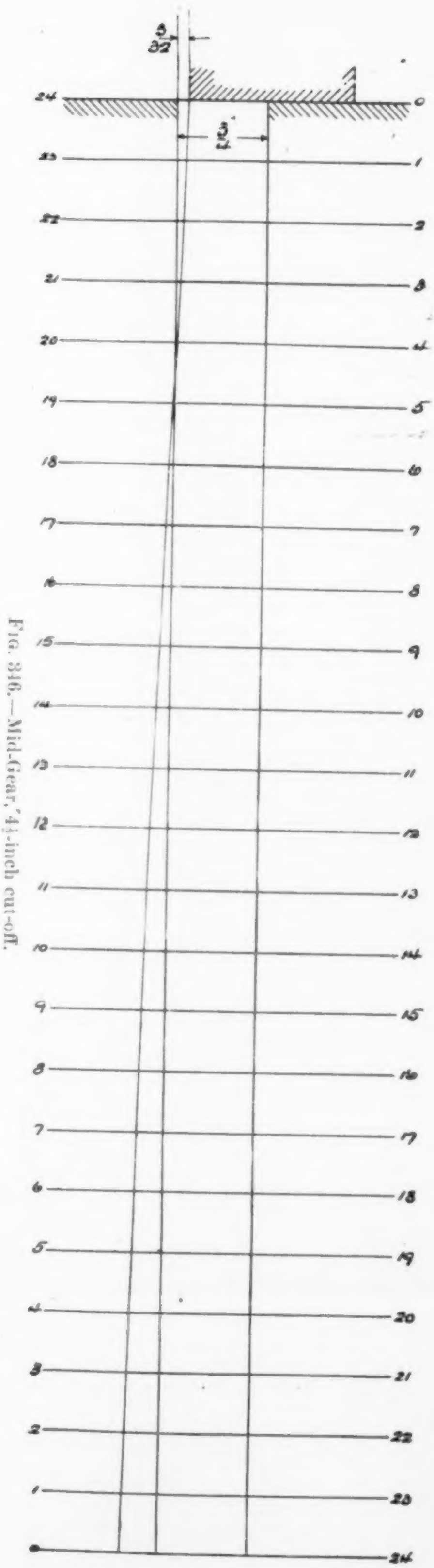


Fig. 346.—Mid-Gear, 4½-inch cut-off.



160 pounds of steam, and it would, at the time of exhaust, considering clearance, have expanded twice. If it were not for wire-drawing the exhaust, this steam would be sent into the atmosphere at 60 to 80 pounds, with the result of pulling the fire out of the firebox. The link motion cannot exhaust at this pressure, because the steam is wire-drawn at the admission, the cut-off, and at the exhaust. It is a miserable makeshift for high pressures and high speeds when considered as an expansion gear.

Now let us consider the other (Strong) gear at its earliest cut-off, as per these diagrams and by Fig. 188. It will be seen that the admission commences very soon after the exhaust valve closes, and as the port edge is very long (viz., 46 inches against 16 inches on an ordinary engine), and the lead $\frac{3}{32}$ inch, the pressure is fairly up to boiler pressure. While there is some wire-drawing, it is not great, and the high-pressure steam is expanded well down to the atmosphere, and then exhausted at $22\frac{1}{2}$ inches, instead of at $14\frac{1}{2}$ inches, as with the link motion, the terminal pressure being 20 pounds, instead of say 70 pounds. It all went out on $1\frac{1}{2}$ inches, instead of $9\frac{1}{2}$ inches. On the return stroke it was compressed through $3\frac{1}{2}$ inches, instead of $9\frac{1}{2}$ inches, as with the link.

One speaker refers to the appearance of the cards and says: "I submit that the Boston and Albany diagrams have a more pleasing effect to the eye than those taken from the other engine." I would remark that there is no accounting for taste, but taste has but little to do with such matters. Engineering is an exact science, every quantity is capable of exact measurement, and assumptions are not in order.

As to the attempt to show flaws in Mr. Dean's figures, and discrepancies in Mr. Coon's report, it should be remembered that it is much easier to find fault than it is to do faultless work.

The attempt to make comparisons of the Boston and Albany, and other engines, is unfortunate, as the former burned select bituminous coal, and the latter "run of the mine" anthracite. I am willing to substantially place engine No. 383 against any other existing anthracite fuel-burning engine of the ordinary type, with the expectation of a saving of no less than fifteen per cent. of the fuel.

In regard to the ratios of actual water consumption, and the consumption as shown by the diagrams, there is no stationary practice to compare these performances with. Experts have probably never tested a 4-gridiron-valve engine, making a piston speed of

800 or 900 feet per minute, with a boiler pressure of 165 to 170 pounds pressure.

Such a test will reveal surprises to anybody, as cylinder condensation becomes a matter of small importance, and leakage cannot occur with a tight piston and gridiron valves.

The point made by Mr. Mattes is a very important one.

*Mr. W. F. Dean.**—I distinctly made an effort to substantiate all of the claims which I made in my own name for the Strong locomotive, and when those claims could not be substantiated it was noted. The principal debaters have pretty generally conceded the points which I made, and admitted in a general way the superiority of a four-valve locomotive. When writing the paper I was aware that the quoted results on the B. & A. R. R. tests showed a smaller consumption of coal, water and steam than was realized by the Lehigh Valley engines. Knowing, as I did, that bituminous and anthracite coal consumptions by locomotives should never be compared, I gave but little thought to the coal matter. In regard to the consumption of water and dry saturated steam, I could only suppose that the easier work which the B. & A. engine had to perform enabled it to be run more advantageously. It developed much less power than the Lehigh Valley engines.

One of the speakers in commenting on the relative coal consumptions of the B. & A. and L. V. engines, says that comparisons of bituminous and anthracite coal consumptions, while not proper, are suggestive. I cannot admit that they are even suggestive, and in support of this, I append quantities which represent the relative performances of a Delaware and Hudson Canal Co.'s locomotive (No. 185), which was tried upon the B. & A. R. R. in the summer of 1883, and B. & A. engines Nos. 137 and 169. No. 185 was a 19″ × 24″ anthracite coal burner, with the typical long fire-box, and the B. & A. R. R. Co. desired to ascertain the practicability of using hard coal burners for their express passenger traffic. No. 185 was attended by a fireman from the D. & H. C. Co.'s line, who had had experience with anthracite coal. All three locomotives were used in the same kind of service. The following table, which was given to me by the late Mr. Colby, master mechanic of the B. & A. RR. at Boston, shows how badly beaten was the hard coaler:

* Author's closure under the rules.

	Miles run per ton of 2,000 lbs.	Pounds of coal per mile.	Miles run.	Cost of fuel per mile.
No. 185	29	67.07	2,600	$0.2369
No. 169	53	38.60	5,000	0.1351
No. 137	57	35.09	5,956	0.1230

No. 185 was built by the Dickson Manufacturing Co., of Scranton, Pa.

In this case the fuel consumption was nearly two to one in favor of bituminous coal.

I was once in possession of figures, pointing to the same conclusion with reference to the locomotive "William R. Robinson," of the Boston and Providence R. R. This engine was originally a hard coaler, but was afterwards converted to a soft coaler. In 1883, or thereabouts, I examined some performance sheets of the Pennsylvania Railroad, and was struck with the great coal consumption by locomotives on the New York Division, over those on the next west division. All passenger locomotives on the New York division burn anthracite coal of the best quality.

The quantities given in the table do not, of course, represent the different calorific values of the two coals, but there is an unavoidable waste in connection with the use of hard coal on a locomotive at the end of the trip. Another cause of waste in the use of hard coal is the fact that most of the companies using it produce their own coal. There is, in consequence, no need of economy in its use.

I am wholly at a loss to understand on what consideration the statement is based that in the Strong engine there is early release. The diagrams do not show it, and certainly the fact is against it. If the release was a little earlier at a high speed, it would be better. It was also stated that the compression begins too early in the Strong engine. Table C., following, shows that it does not. The indicator diagrams apparently show at high speeds that compression begins somewhere near [but later than] the middle of the backward stroke. This is due to wire-drawing the exhaust as it tries to escape. A link motion locomotive at high speeds often, for this reason, when closing the exhaust at 8 or 9 inches from the end of the stroke, apparently begins to compress at 15 or 18 inches from the end.

In working up cards, this point cannot be too carefully remembered.

That the link motion has the defects of early release and early compression is apparent from Tables A and B following.

One of the debaters seems also to think that the back pressures of the Strong locomotives are excessive. I showed in the paper that they are much lower that those of link-motion engines working under the same conditions.

TABLE A.

TABLE OF THE DISTRIBUTION OF STEAM IN LAUDER ENGINES, O. C. R. R. CYLINDERS, 18" × 24".

Notch.	Lead.		Cut-off.		Release.		Exhaust Closure.	
	Front.	Back.	Front.	Back.	Front.	Back.	Front.	Back.
1	$\frac{1}{32}$"	Exactly similar.	$20\frac{15}{16}$"	$20\frac{13}{32}$"	$23\frac{1}{8}$"	23"	$\frac{7}{8}$"	1"
2	$\frac{3}{64}$		$20\frac{5}{16}$	20	$22\frac{15}{16}$	$22\frac{7}{8}$	$1\frac{1}{16}$	$1\frac{1}{8}$
3	$\frac{3}{32}$		$18\frac{5}{16}$	18	$22\frac{3}{16}$	$22\frac{1}{16}$	$1\frac{13}{16}$	$1\frac{15}{16}$
4	$\frac{5}{64}$		$16\frac{1}{4}$	$16\frac{1}{4}$	$21\frac{3}{8}$	$21\frac{5}{16}$	$2\frac{5}{8}$	$2\frac{13}{16}$
5	$\frac{11}{64}$		$13\frac{1}{4}$	$13\frac{1}{4}$	$20\frac{1}{8}$	$20\frac{3}{16}$	$3\frac{7}{8}$	$3\frac{13}{16}$
6	$\frac{3}{16}$		$10\frac{1}{4}$	$10\frac{3}{16}$	$18\frac{5}{8}$	$18\frac{3}{4}$	$5\frac{3}{8}$	$5\frac{1}{4}$
7	$\frac{7}{32}$		$7\frac{3}{8}$	$7\frac{3}{16}$	17	$17\frac{5}{16}$	7	$6\frac{15}{16}$
8	$\frac{15}{64}$		$4\frac{11}{16}$	$4\frac{5}{16}$	$14\frac{7}{8}$	$15\frac{5}{16}$	$8\frac{1}{8}$	$8\frac{15}{16}$
Center	$\frac{1}{4}$		8	$2\frac{1}{2}$	$12\frac{13}{16}$	$13\frac{1}{8}$	$11\frac{3}{16}$	$10\frac{7}{8}$

TABLE B.

TABLE OF THE DISTRIBUTION OF STEAM IN CLASS "P" ENGINE PENNSYLVANIA R. R. CYLINDERS $18\frac{1}{2}$" × 24."

Notch.	Lead.	Cut-off.	Release.	Exhaust Closure.	Port Opening.
Full Gear.	$\frac{1}{8}$"	"	$23\frac{1}{4}$"	$\frac{3}{4}$"	"
13	$\frac{1}{4}$	8	$16\frac{5}{8}$	$7\frac{3}{8}$	$\frac{3}{8}$
14		6	15	9	$\frac{1}{4}$
15	$\frac{5}{16}$	$4\frac{1}{2}$	$13\frac{3}{16}$	$10\frac{13}{16}$	..

TABLE C.

TABLE OF DISTRIBUTION OF STEAM IN STRONG LOCOMOTIVE, 24" STROKE.

Notch.	Lead Constant.	Cut-off.	Release.	Exhaust Closure.*	Port Opening.†
Latest cut-off	$\frac{3}{32}$	$19\frac{1}{2}$	$22\frac{3}{4}$	$2\frac{1}{2}$	$1\frac{7}{16}$
½ stroke	$\frac{3}{32}$	12	$22\frac{3}{4}$	$2\frac{1}{8}$	$\frac{1}{2}$
⅓ stroke	$\frac{3}{32}$	8	$22\frac{7}{8}$	$2\frac{1}{2}$	$\frac{7}{16}$
Mid-gear	$\frac{3}{32}$	$4\frac{1}{2}$	$22\frac{3}{4}$ or $22\frac{1}{2}$	$2\frac{1}{2}$ or $3\frac{3}{4}$	$\frac{7}{32}$

The quantities in the tables show that the Strong gear is not troubled with premature exhaust nor with excessive compression.

In comparing the port openings of the Strong engine with those of the Pennsylvania engine, it should be remembered that the ports of the latter have a length 93 per cent. of the diameter of the cylinder. Reducing the openings of the Strong ports, given in the table, to this basis, the figures would be respectively $1\frac{9}{16}''$, $\frac{17}{32}''$, $\frac{15}{32}''$ and $\frac{15}{64}''$.

A charge is made that I treated the B. & A. unfairly in comparing cylinder capacities by taking a card from it which had an earlier cut-off than that used from the Strong engine. If this is true, it is because I did not possess the original of the B. & A. card, and was obliged to use the reproduction of it from the *Railroad Gazette*. A distinct effort was made not to lay myself open to this charge. In determining the point of cut-off from the card, the point of contra-flexure was taken, as near as could be determined. The same speaker also says that in making such comparisons cards should be used having boiler pressures, speeds and points of cut-off the same; I should say having *initial pressures*, speeds and points of cut-off the same.

With reference to wire-drawing, the cards show that in the Strong gear the first part of the steam line is full, but that there is great wire-drawing toward the point of cut-off. The tables just given show that the port opening is slightly in excess of that of the link motion, and the cards show that this difference is effective.

* Can be closed as early as desired up to 10 inches from end of stroke, but the quantities given are those preferred. The change is made by moving the exhaust lever in the cab.

† Reduced to an equivalent port, having a length equal to the diameter of the cylinder.

With reference to the inconsistencies of the tables of which complaint is made, they are more apparent than real. Certain representative cards were selected and very carefully worked up, and the average powers deduced from them ought not to be considered the average powers of the engines. The cards selected stand solely on their own merits. As for the ratios of 95 and 97 per cent. between the actual water consumed and the steam accounted for by the indicator, they are incorrect doubtless for the reason given by Mr. Barrus. In the notes which I used no record is given of the time during which the throttle was open, and therefore of the time during which the engine was developing power.

Mr. Coon's method of measuring the water was admirable, and could not be improved, except perhaps by checking the results with one or two water meters. The actual water consumptions as given in the tables are only valuable in comparing engines Nos. 383, 444 and 357 with each other, the same method having been used with each engine. The water consumptions should not be compared with those of the B. & A. engines.

With regard to my statement that all balanced valves leak, it is based first upon figures from an actual test of a pumping engine by an able and well-known expert. The engine was equipped with one of the best known balancing devices. A test without the balancing device gave 15,000,000 ft. lbs. greater duty than with the device. Secondly, I have often heard another "very successful" balanced valve flutter in service, showing unmistakably that it leaked badly, and this was admitted to me by the inventor.

I do not consider the fact that a balanced valve is tight on a cylinder with the heads off when the engine is still, proves anything, because nothing is happening to disturb it. There is no jar and no compression, and there can be steam only on one side of it.

CCCV.

RIVER PRACTICE OF THE WEST.

BY JNO. M. SWEENEY, WHEELING, W. VA.

(Member of the Society.)

There is nothing, at least within the knowledge of the western steamboat man, which has been so subject to divers criticisms and various experiments in attempted betterments, as the western steamboat. The term is generally employed in designating the water craft found upon the Ohio and Mississippi rivers. The criticisms come from engineers—both steam and marine—who have had their experiences with craft of other character, and generally the experimenters are found within the ranks of those who become "practical" men by reason of their having passed more or less time "running on the boats."

The navigation of such streams as the Ohio and Mississippi is a problem entirely distinct; only average results can be expected; boats must go and make something near schedule time, whether the depth of water in the channel is 30 inches or 30 feet, so that the first desideratum is minimum weight and maximum power, with all possible displacement of hull per each unit of immersion at all in keeping with anything like shape.

In boats designed for combined freight and passenger business, the hulls are constructed with all the lightness in any way consistent with safety against falling to pieces, and the machinery must have small diameter of cylinders and boilers, and consequently must be designed for high steam pressure.

Probably no single feature of the engine has received so much unfavorable criticism as the lever valve gear; while in reality it is better fitted for the requirements of the service than any other, and in support of this statement it may only be necessary to call the attention of our engineers, who are of the opinion that the method is very crude and primitive, to this: these boats are very raftlike, limber; overloading at any point will distort their shapes; "tight on their chains," when without load; "slack on their

chains" when loaded; so throughout the process of loading and unloading, one or the other effect constantly going on; unequal conditions as often occur; these in turn perceptibly vary the distances

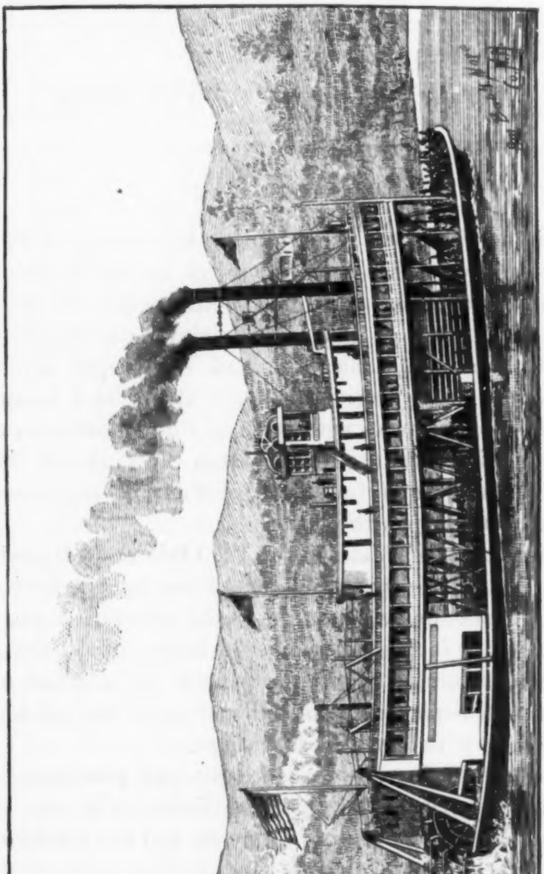


FIG. 271.

between the center of the main shaft and "rock shaft." Now the lever valve gear allows this variation without seriously affecting the timing of the valve movement; no other form of valve arrangement gives this very necessary quality of stretching. The writer

was at one time employed to place on a side-wheel boat a pair of engines 24″ diameter, 48″ stroke, slide valve. The valve gear was very carefully made and adjusted; a stationary link with shifting block was used; engines indicated with excellent results; angularity of connecting rod even was adjusted—something probably never before undertaken on a boat of like class. On first trials, and after going into active service, these engines gave every satisfaction, but soon began to cut capers, refused to "handle" properly, hung on centers, and would not "back," particularly when the momentum of the boat was on ahead. An examination showed serious derangement of the valve timing, due to incorrect length of eccentric rods. A readjustment of the rods relieved the difficulty, but it became necessary to make changes in the lengths of the rods almost daily, covering a period of about 90 days, when, the river becoming too low, the boat retired from service. Upon the resumption of navigation some 40 days later, the engine would not turn over until a radical adjustment of the rods was made, and when corrected they were found to be in nearly their original position, and the total change of the length of each rod was $1\frac{1}{8}$ inches, the distance from the center of main shaft to the link being about 14 feet. This was an exceptional case; a combination of circumstances, a very light boat, the link suspended just at the point where the greatest variation occurred, made it so; but it shows what may and does happen to such valve gears applied to the class of boats under discussion. Gradually through the past ten years, side-wheel boats have diminished in number and tonnage, and stern-wheel boats have increased (Fig. 271). At best, any of these wooden boats are perishable, and the vast difference in first cost and the cost of maintenance of the side-wheeler over the "wheelbarrow" boat, has determined investments in favor of the latter, and while to outside appearance the stern-wheel boat of to-day is identical with that of twenty years ago, it is only so in that particular. The method of hull and joiner construction is much the same. Models have materially altered and have carried with them or perhaps been caused by broad changes in the application of the rudder or steering arrangements.

This article, therefore, will be confined entirely to the stern-wheeler, as representing the most successful and recent practice.

Fig. 260 indicates in plan the water lines of a recent light-built boat; Fig. 262 half bodies, and Fig. 261 side elevation showing shear lines, etc.; Fig. 263 is a fore and aft section of a part to show

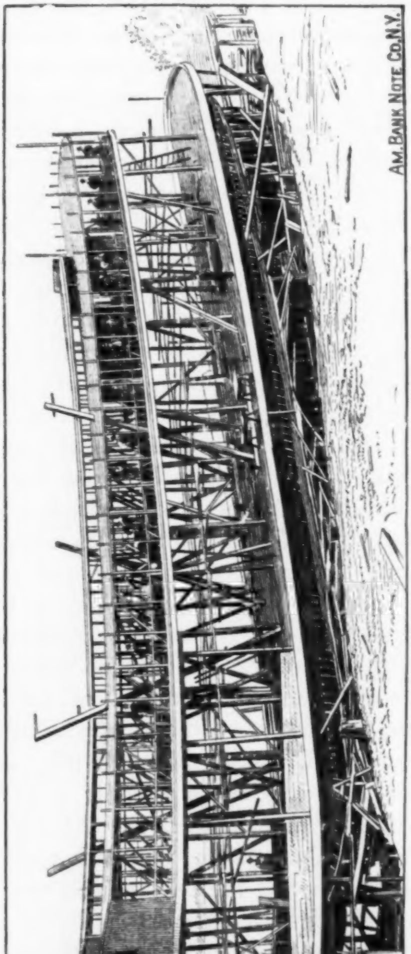
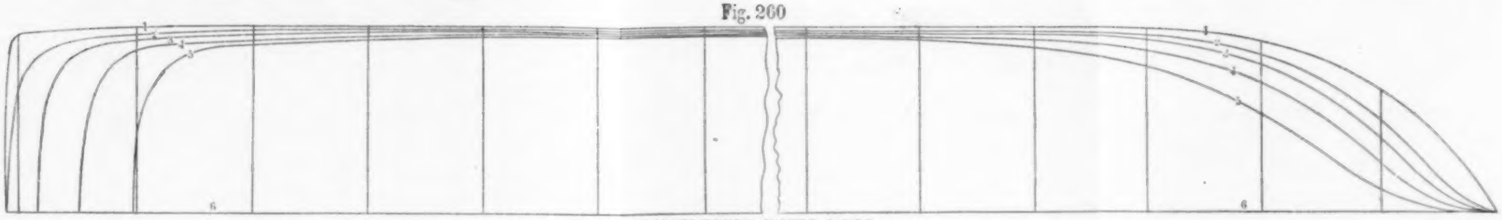


Fig. 265.

method of fastening, proportion of timber, etc., in the hull; Fig. 264, a cross-section through one of the frames.

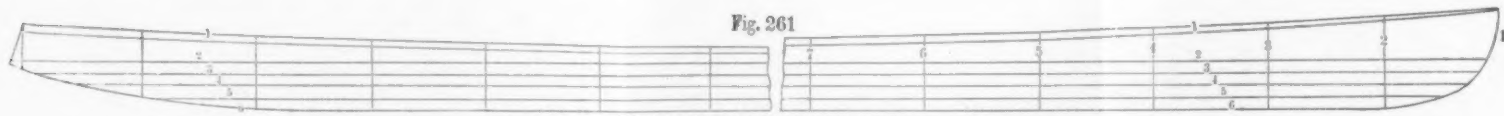
Fig. 265 is from a photograph of hull on the stocks. In connection with the figures, the following table of specifications indicates full dimensions:

Fig. 260



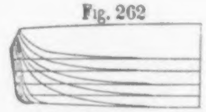
PLAN SHOWING WATER LINES.

Fig. 261



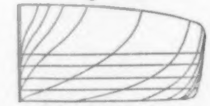
ELEVATION SHOWING SHEAR LINES.

Fig. 262



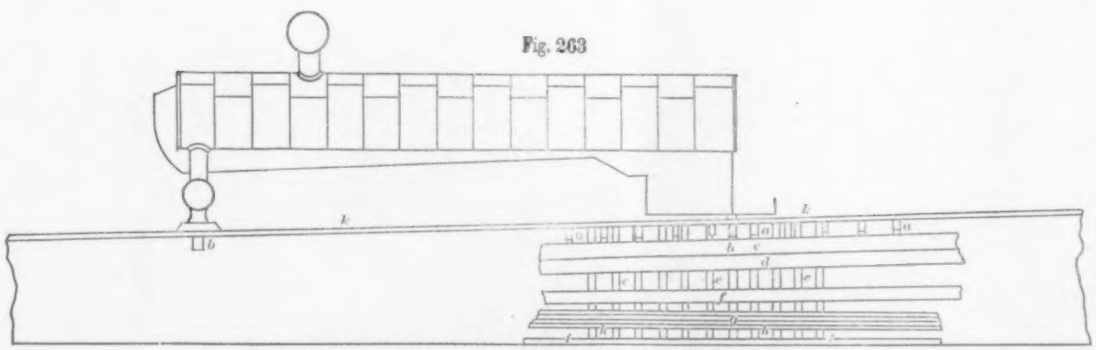
HALF BODY AT STERN

Fig. 262a



HALF BODY AT STEM

Fig. 263



FORE AND AFT SECTION OF A PART SHOWING TIMBERS IN HULL.

a-a-a Deck Beams.
b b Boiler Beams.
c & d Top Clamp 2 pieces.
e-e-e Hull Timbers.
f Side Strake.
g Main Keelson 4 pieces.
h Floors
i Bottom Plank. k Deck.

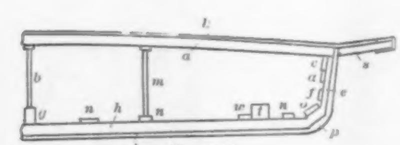


Fig. 264

HALF CROSS SECTION THROUGH ONE FRAME

l—Keelson Bulkhead
m—Stanchion in Hold
n—Floor Strakes
o—Bilge Keelson
p—Knuckle
s—Outrigger for Guard
t—Footing for Braces

Length, 180 feet.
Beam from out to out of frame, 33 feet.
Depth at lowest place in wing, $4\frac{1}{2}$ feet.
Floors $3\frac{1}{2}'' \times 6''$ centered on 14″ forward to $2\frac{3}{4}'' \times 6''$ centered on 16″ aft.
Main keelson, $5'' \times 10''$ made of 4 pieces $2\frac{1}{2}$ thick $\times 5''$.
Bilge keelson, $3'' \times 7''$.
Top clamp, 20″ deep.
Side strakes, $2'' \times 8$.
Floor strakes, $2'' \times 6''$.
Keelson bulkhead, $1\frac{1}{4}''$ thick.
Deck beams, $3'' \times 6''$ centered on $24'' \times 28''$.
Deck, 2″ thick.
Stanchion in hold, $3'' \times 4''$, $2 \times 4''$ and $3'' \times 3''$.
Boiler beams iron with wood filling.
Outriggers for guard, 30″ long.
Bottom plank, $3\frac{1}{2}'' - 3''$ and $2\frac{1}{2}''$ thick, fore to aft.
Knuckle, 5″ thick.
Wale top strake $1\frac{1}{2}''$ thick filling 2″ and $2\frac{1}{2}''$.
Knuckle full bolted — 3 bolts — $\frac{3}{8}''$ iron.
Bottom plank, $\frac{3}{4}''$ bolts.
Floor strakes bolt to every third timber.
Main keelson every other timber.
Footlings $8'' \times 9''$ run through to receive heels of main and relief chain braces.
3 Balanced rudder stocks, $8'' \times 8''$, blade 3″ thick with shoes.
Tillers, $3\frac{1}{2}'' \times 6''$ to 4″.

MACHINERY.

2 Engines $16\frac{1}{2}''$ diameter of cylinder $\times$ $6\frac{1}{2}$ feet stroke.
Wheel 20 feet diameter; 15 buckets — 22 feet long.
1 Doctor.
Steam capstan with double engine.
2 boilers 30 feet long, 47 inches diameter; each with 6 — 10″ flues, with mud and steam drums, check and safety valves.
Hand deck pump, etc.

The preferable material in woods for hull construction are the white oaks, of West Virginia, weighing from 60 to 65 pounds per cubic foot, with a tensile strength of some 12,000 pounds per square inch of section, and some 2,500 pounds to their elastic limit. The

woods for joiner work and bulkheads, yellow poplar and white pine; the joiner work is very light and "flimsy," but when properly fastened makes a very satisfactory shelter. All the cross and fore and aft bulkheads are about $\frac{1}{4}$ inch thick connected by a munton in order to stiffen them; the munton is vertical, and the bulkhead is assisted also by a strip $1\frac{1}{2}'' \times 1\frac{1}{2}''$ on which one end of bed slats rest. The decks are also very light; main deck $1\frac{1}{2}''$ to $2\frac{1}{2}''$ thick, depending on the class of boat; the cabin floor, or boiler deck as it is called, $\frac{7}{8}''$ thick; and the roof of the cabin or hurricane deck $\frac{5}{8}''$ thick; the latter decks are made of tongue and grooved lumber secret nailed.

The advances made in construction and methods have been almost entirely in small things, but make an aggregate result of some importance.

Much larger tonnage is propelled with greater speed; some fuel economies have been made, and the cost of operation and maintenance largely reduced; the boats are more manageable. Not so very long ago it was a great exception to find a boat capable of turning around, or of getting away from shore when strong winds were blowing. Now almost any of them go with slight delay.

Probably the most radical change from old forms is the disuse of wing rudders attached to skegs at the stern. Figs. 268 and 269 represent what was for a long time the standard method, for three-rudder boats, two wing rudders and one balance rudder; when the number of rudders was increased, one or more balance rudders was added, but the wing rudders and the skeg you had always with you. The best practice of to-day discards the wing rudder entirely, using only balance rudders, never more than four, usually three, and occasionally two, depending on the beam and other dimensions of the hull. When the balance rudder was first employed, that portion of the blade projecting forward under the "stern rake" (when fitted close to the bottom in the neutral position of the rudder) presented in its operation a constantly increasing space between the blade and bottom of the boat as the rudder advanced to its extreme position on either helm; this space was objectionable, because likely to catch drift and so "foul the rudder;" also because it was thought the opening diminished the effectiveness of the rudder. In order best to relieve this trouble, the builders of the time spiked blocks of wood against the bottom plank, being careful to have the blocks thick enough, and then reduced them to a shape discovered by hanging a skeleton rudder in position and traveling it as they

JNO. M. SWENEY.

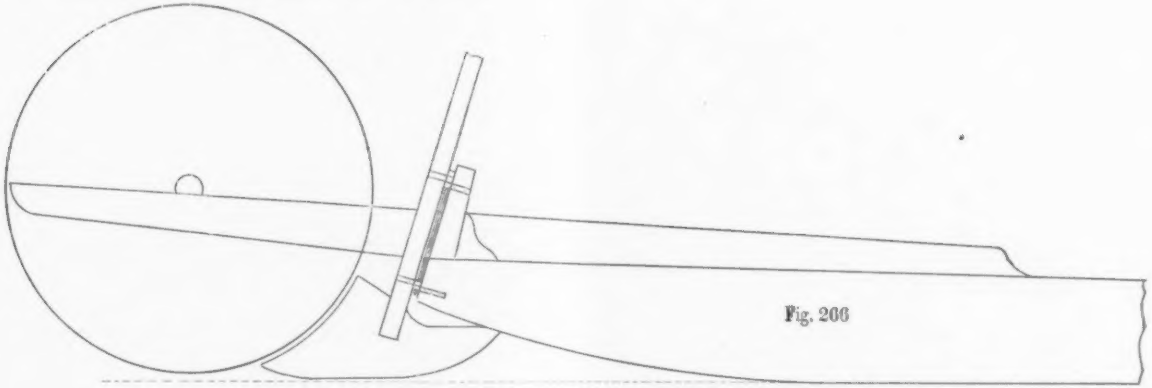


Fig. 266

ELEVATION SHOWING STERN RAKE AND METHOD OF HANGING RUDDERS.

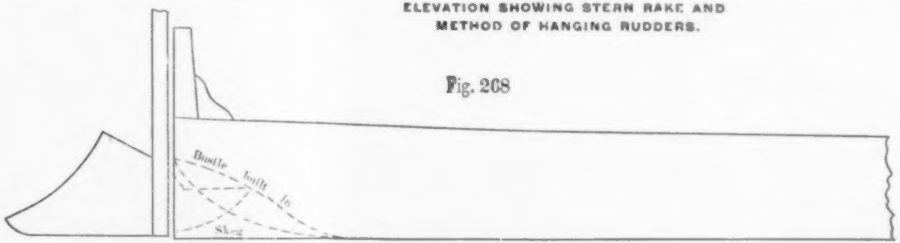


Fig. 268

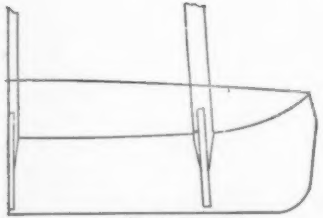


Fig. 267

TRANSOM AND RUDDERS.
3 Balance Rudders

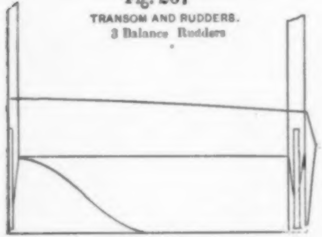


Fig. 269

BUSTLE STERN.
1—Balance Rudder
2—Wing "

cut; this was technically called "building a bustle on"; it did not last, however, very long—something like fifteen years—until some one lost to history discovered it possible to form the frames or floors composing the stern to the desired cone shape and dispense with the blocks of wood; this was a great stride and soon became the rage, and was termed "building a bustle in." It is the practice to-day of those designers who think there can be no improvement.

Coeval with this practice was the use of a very short and steep rake to the stern of the boat, insisted on because it was popularly supposed to increase the bearing of the hull and induce a consequent lighter draft, as well as better to sustain the weight of the wheel and machinery, which on the stern-wheel boat was necessarily at the stern end. It is only in recent years that probably a better conception of the laws governing the propulsion of such boats has induced retirement of the wing rudders, and a longer and more acute stern rake. The two are closely connected, because primarily the measure of the propulsion of the boat in either direction is the measure of the resistance of the body of water moved by the wheel in the opposite direction. Now the wheel must be supplied with new water as fast or faster than it displaces it, so that when the boat is being moved ahead, if the shape retards a free delivery of water to the wheel, a very bad application of power results.

In the "bustle" form of stern, the appearance of the water when the boat is moving is very favorable, often smooth and clean and without breaks between the wheel and stern rake, as well as around the outside of the hull, and many consider this surface condition to indicate proper shapes; however, a chip or block dropped into the water alongside of the boat, some 30 feet from the stern, will usually go under the boat in its effort to get to the wheel, while the shape indicated in Figs. 266 and 267 throws the chip or block off from the side.

These boats depend entirely on rudder power to govern their movements in steering, and this can only be done by creating a current on the rudders, so that when the wheel is used to move the boat astern and at the same time it is desired to deflect the stern sideways, it is the water delivered from the wheel and passing under the boat which makes the force on the rudder. When the rake of the stern is steep, the water from the wheel is banked against the end of the boat, and reacts upon the rudder, causing

the frequent deflection of the stern in the opposite direction to that desired. In adopting designs for stern shapes, as shown, and making that shape the reverse of the bustle, it is still possible to preserve an approximate contact between the forward rudder-blade top and the bottom of the boat. This is secured by discarding the old method of setting the rudder post and pintle square with the keel-line, substituting a position at right angles with the plane presented by the rise of the rake at the station met by the under blade; so that the pintle and rudder stock and post pitch forward from a right line with the keel, and rudders other than the center one pitch also sidewise to meet a right line with the thwart-ship dead rise of the bottom at the same point; therefore none of the pintles are in the same plane, and a ball joint is used on the tiller to connect the coupling bar common to all of them. A further guard against fouling the rudder action is in the opening left between the top of the rudder blade and the bottom of the hull, whereby any drift forcing in is soon relieved by the enlarging opening. The success of this plan has proven that the idea insisted on to have no current pass over the blade was a mistaken one.

The models for forward body or bow form are usually subject as much to the character of the craft as anything else. For a freight boat loading heavy weight on the forecastle, a shape very full in harping, adds increased bearing as the load increases, and at the same time allows sufficient cutting away in the first two feet to properly balance the boat when in light trim. Boats used exclusively for towing may be sharper in order to properly load with fuel carried almost exclusively amidships. The best general shape seems to be that in which well-defined rising lines are found, so that the displacement is made downward rather than sidewise. This increases buoyancy when the hull is driven hard, and holds the head of the boat up against burying; while the wall-sided forward model seems to account for cases where the same hull with larger driving power developed, actually showed a decreased speed.

The writer hopes at some near time to present to this Society the results of some careful experiments on shapes, made from models driven with a paddle-wheel, exactly as the practice is, considering those experiments made with models drawn through the water by strings attached, etc., as valueless, because the application of power is the determining factor. The best model for a canal boat, drawn by horses from the towpath does not determine

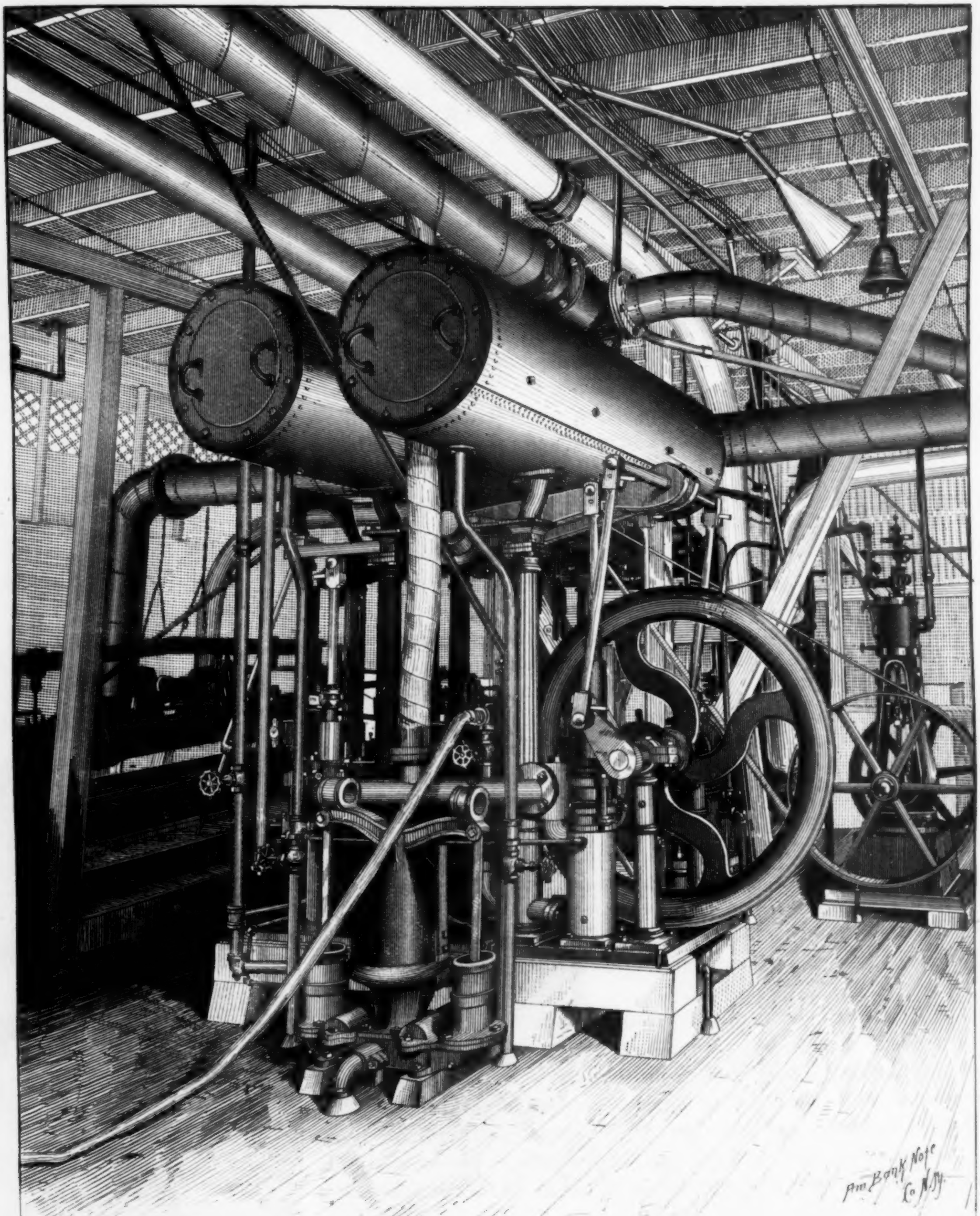


FIG. 273.

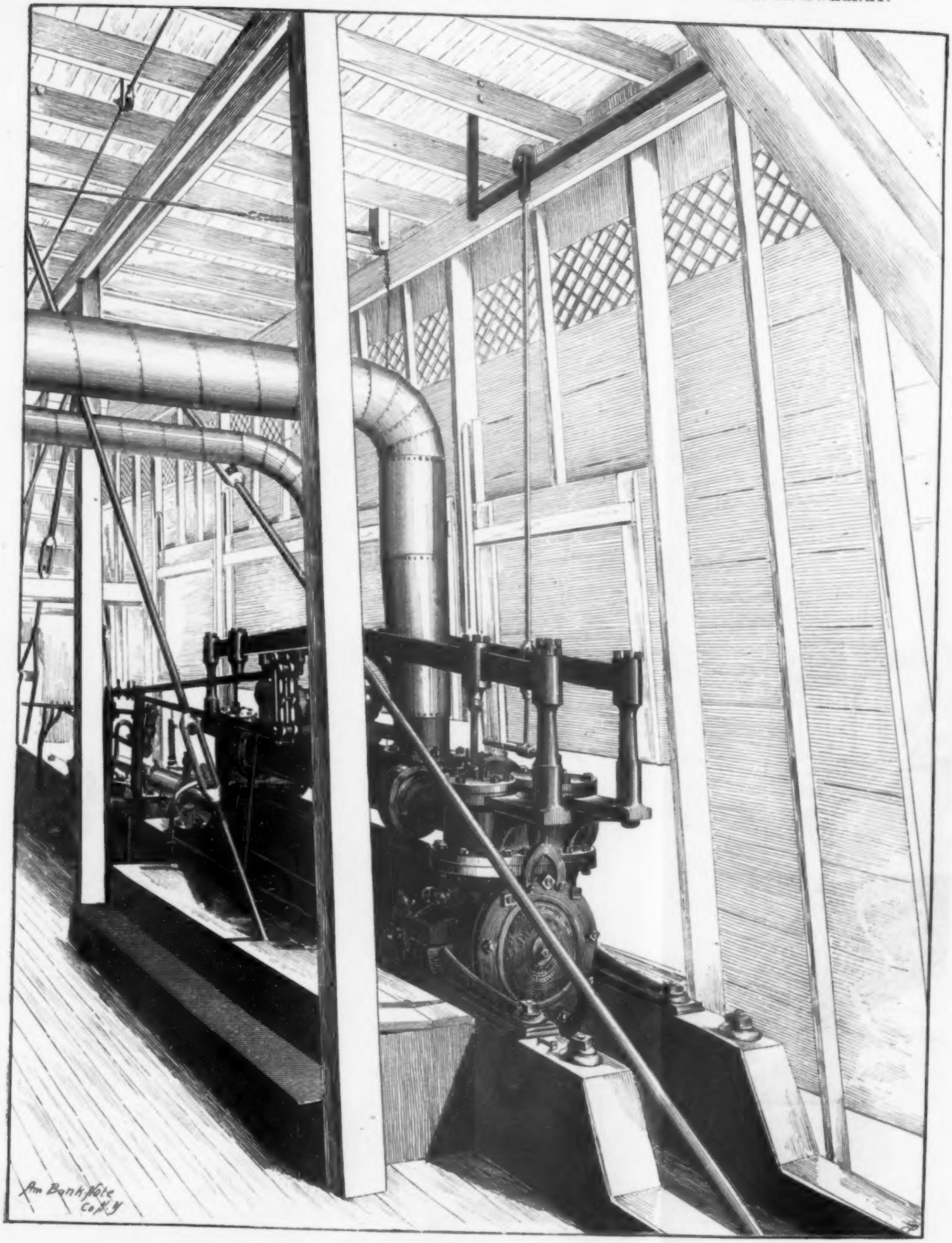


FIG. 272.

the best model for a boat driven by the action of a wheel upon the water.

In the Steam Engineering Department, the rules of the U. S. Board of Supervising Inspectors of Steamboats, adopted in 1873 work radical changes in construction. These rules, about coincident with the introduction of steel boiler plate, based the allowable working pressure of steam upon the tensile strength of the material. Now a large steam pressure is the great desire of every boatman's heart, and at once the greatest tensile strength obtainable was demanded. Seventy thousand pounds was generally adopted, but in some few cases 80,000 pounds was attempted. The amount of carbon, however, required in such plates at that period of steel-plate development, produced some very unsatisfactory results, and the further action of the supervisors requiring a reduction in area of at least 50% for $\frac{26}{100}$ plate has brought the commercial product down to about 65,000 tensile strength. Many reflections are cast upon the plan of boiler and furnace in use on the boats under discussion; and no doubt to the outsider, who never stood over a steam boiler with 200 to 225 pounds pressure on it all day, the forms used seem very crude and wasteful of fuel; but the fact remains that while many radical changes have been proposed and attempted, the result has generally been a speedy return to the accepted form. In the first place a furnace construction which will generate 90 or 100 pounds of steam-working pressure with the greatest fuel economy, will not generate 180 to 200 pounds with the greatest fuel economy; in fact, it usually will not generate the last pressure at all. Change of form is absolute, and ordinarily that form of furnace which make the desired result with the least manipulation gives the best economy. The plan of boiler most in use is the externally fired return-flue type, shell 40″ diameter, 24 feet long, two return flues 13 inches, sometimes 14 inches diameter; shells $\frac{26}{100}$ thick, flues $\frac{29}{100}$ or $\frac{3}{10}$, when made in rings 24 inches long; rivet holes drilled, and longitudinal seams double riveted. This boiler receives certificates from the Government inspectors, allowing for 70,000 T. S., a maximum working pressure of 182 pounds being one-sixth the ultimate bursting strain of the shell; but nothing has yet been devised to prevent the operators from exceeding this limit, and 200 to 225 pounds is frequently maintained. There is always a disposition to do a little more work, particularly with tow-boats. The numerous pier bridges and dams placed in the river in later years incite preparation to meet the demands in

"running" them, and nothing comes nearer doing this than a "wad" of steam at the proper time. Fifteen years ago, with iron boilers of no defined tensile strength, 160 pounds was big steam. Now the facts are as stated. The evaporative duty of these boilers is about seven pounds of water per pound of coal, and when compared on a basis of the foot pounds of work done per pound of fuel, show favorably with any water craft. The demand made upon boilers using the water of these silt-bearing streams is very heavy, and imposes conditions under which other forms, although possibly of better fuel duty, fail in points of service and steadiness. Much indirect harm is done by a requirement of the supervising inspectors, that a water space of at least 3 inches should be preserved between the flues and shell, and between the flues themselves. Previous to this enactment, about 1½ inches of space was the practice. The men who made these boilers had always used a 14-inch flue in a 40-inch shell, and they knew no other proportions. As a consequence, in order to comply with the new law, they raised the flues in the shell sufficiently to secure the required 3 inches of water space, and thereby vastly diminished the steam space, as well as curtailed the surface for the elimination of the steam. There is one other form of boiler almost as popular as the "double-flued." The shell, 42 inches diameter, contains six 8-inch flues, in two rows of three each, the one flue immediately above the other. This gives easy access to every part for cleaning and repair, and steams very well. One such boiler 18 feet long, 210 feet heating surface, 20 feet grate surface, is supplying two engines 10″ diameter, 48″ stroke at an initial pressure of 170 pounds. The engines indicate an average of about 170 horse power, and fuel consumption is 500 to 600 pounds per hour, or a result of 3 to 3½ pounds of coal per H.P. per hour. Several cases of compounding have made a better result than this, but always at greater first cost, large additions in weight and cost for maintainance, and have generally been succeeded by direct high pressure in the next boat built by the same owners. The large increase in working pressures of late years has demanded greater strength in the machinery and fastenings, and also in the hull construction, but the increase in weight has not been proportionate to the increased power developed, and the service has thereby been improved. The changes in valve gear have not been extensive. The plan shown in Fig. 270 has been introduced, and is meeting with some favor. In this valve gear the effort has been to retain the good qualities of a "cam" movement and improve the results of

its action. In the ordinary "lever" gear, four valves—two receiving and two exhaust—are employed to each engine; two engines attached to a common shaft at 90°; two cams are used to each engine—one full stroke, one cut-off. When the engines are in full gear the full-stroke cam operates four valves, raising one exhaust and one receiving valve at opposite ends of the cylinder at the same moment. This one cam does all the work in both full-gear motions of the engine, and is therefore in its neutral position when the crank is at its dead point; the cut-off is engaged after the full gear has given headway to the boat, and is used only in the forward motion. This arrangement necessarily precludes any cam position securing either an early exhaust opening or early closing of the opposite valve. Some partial remedy to the consequent poor action has been found by blocking the exhaust lifters, so securing a somewhat earlier exhaust, but likewise a later exhaust closure; so that with the cam in neutral position, and the crank on its dead point, both exhaust valves are partially open. When in full gear operation, a slight blow through arises from this condition; but after the cut-off is engaged it disappears, because the opening movement of the cut-off cam is much slower than the full stroke.

Fig. 270 indicates a recent gear employing two full-stroke cams—one for each motion—and set positioned for the usual lead, etc.; also a cut-off cam to each engine. The drop in the lifter of receiving valves allows the lead of exhaust cam to operate the exhaust valves without effecting the receiving valve until the proper time. The cut-off cam is also advanced, and its cut-off point adjusted to meet the altered position.

The tonnage of western rivers is increasing, and also the number of passengers carried, and the future demands seem to point to the adoption of composite boats using steel frames, etc., but wooden skins.

DISCUSSION.

Mr. A. F. Nagle.—Can any one answer the question whether they are using compound engines on these river steamers?

Mr. J. F. Wilcox.—I can answer that to a limited extent. Some years ago the firm of A. Hartupee & Co., of Pittsburgh, equipped two boats with compound engines. One of them went to New Orleans, and it afterward went to some South American river. The fuel there is all carried to the rivers from the Gulf

ports or England. The experience with that engine was not a favorable one down there. It was for a short while; but being away from any ports where repairs, etc., could be made, she was finally discarded and thrown out of service. The other boat made one trip to Memphis, and after that, ran up on the Missouri River and was burnt. The history of the subsequent use of compound engines upon the Western rivers is full of interest, and as I have access to much of the data pertaining to it, will prepare a paper in time for our next meeting. I disagree with Mr. Sweeney in one thing he says—"Various experiments have been attempted towards the betterment of this condition of things." With the exception of some trifling alterations in matters of pumps or boilers there has been scarcely anything done for twenty years for Western river boats. Captain Eads, when he built half a dozen monitors, did more for the Western rivers than any other engineer, but with his departure from that branch of industry there were but few, like Mr. Sweeney and Mr. Hartupee, who gave the matter thought.

Mr. Wm. Kent.—In regard to the compound engine on Western rivers, I saw a boat equipped with a compound engine at Memphis about four years ago. I think it is the boat Mr. Wilcox referred to as having been sunk.

With regard to the Western river boats of the present type described in the paper, I think that they are monuments of the worst engineering that is to be found in this country. If you consider the purpose of engineering, as the marine engineers do, to get maximum economy of fuel, to get precision of workmanship, to get great power in small space, then the Western river boats are as far away from it as they can possibly get; they are terribly expensive in fuel; they are very cheap; they are clumsily constructed; they have a stroke six times the diameter of the cylinder; they have very slow stroke and great condensation of steam, and everything else that is bad. But when you consider the conditions under which they work, it may be considered that they are good examples of engineering, in that the boat is adapted to the need for which it exists—that is, to transport from Pittsburgh to New Orleans, with the greatest economy of labor and repairs and first cost, the very cheap products sent down that river. Fuel is of no consequence whatever. They get fuel possibly at about a dollar a ton at Pittsburgh, the current helps them down the river, and they are in no hurry to get back, because the

whole investment in plant is very small, and the barges they tow down to New Orleans are frequently broken up, and they have to come up light. What they have to do is to get across the sand bars and mud bars on a very light draft of water; consequently they have boilers which are licensed to carry 175 pounds of steam presure, and in case of necessity they put a tierce of lard in the furnace and a nigger on the safety valve in order to get over the obstruction. But the ordinary questions that come into engineering, such as economy of fuel, do not come into Western river practice, and are scarcely ever thought of.

Another point in which they are peculiar is that these boats have to run on less water than boats anywhere else in the world. They are said to run on a light dew. If you requested Messrs. Cramp & Sons, for instance, to design a boat which should be very cheap in first cost, which should run on the cheapest labor possible, and which should run on a light dew, I think they would build pretty near the Ohio River boat of to-day. I think some day there will be a radical change in the construction of these river-boat engines, such as putting in screws instead of paddle-wheels; but, if you stick to the end wheel you now have, it is necessary to have a long-stroke engine, and to have high pressure in the boiler, and with the character of engineers, they have engines that can be tinkered with a monkey-wrench, and made to run somehow.

Mr. W. S. Rogers.—I want to put in a plea for that river steamboat. I ride on it occasionally; I do not want to, but I do (laughter). Mr. Kent probably is not aware that the Western steamboat man has a more discouraging time of it than the marine engine owner. Our Western steamboat man gets a steamboat from the builder and starts a very nice trade, and some fellow shoots a railroad right alongside the river. Now there isn't anybody building railroads across the Atlantic (to compete with the marine engine), and you can improve the modern steamer to go faster and do better, but the railroad man just knocks all the competition and ambition out of the steamboat man. So he just lives along and does the best he can until the boat rots, or some other fellow comes along and buys him out. He will take you for a dollar and a half, and if you cannot pay a dollar and a half, he will take you for fifty cents and board you. He does not say he will take you there in six days, but he will tell you he will get there. I do not think screw propellers can be used, on account of

the sand bars. But give that steamboat a show and let her alone (laughter).

Mr. H. de B. Parsons.—The subject of this paper is one of very great interest. I think there are many places where great improvement can be made. In dealing with the problem of designing a vessel that can be driven at comparatively high speed in shallow water, the great difficulty arises from the shallowness of the water. By shallow water we mean water that is less in depth than the length of the wave which would naturally form under the existing conditions. The length of wave is generally proportionate to the speed of travel, other conditions being equal. Now a boat of the type that is now described is probably the only type which could be used with any sort of favor. The vessel clears on what are called her buttock lines. The water does not flow in at the stern from the side, but rather from underneath, and in consequence there is a liability to produce a severe drag or suck at the stern. From that cause the position of the wheel at the stern is the most favorable point, because it is working there in the water which is dead or nearest dead, and a boat of this character will always be most efficient when the point of application of the power is in that dead water; that is, water traveling along with the speed of the vessel. There is another point in the steering: the author said the boat steered much better when they cut away the bustle form and adopted the new type, thus reducing the drag water. Mr. Froude, in England, gave some very beautiful experiments exemplifying this point for the British Admiralty. The author also says that "overloading at any point will distort their shape; 'tight on their chains' when without load; 'slack on their chain' when loaded."

Now it is very easy to calculate for all cases of loading, the character of the strains, and where the strains will come. Then place your hog frame so as to resist these strains, and your hog frame, I think, may be very lightly built and yet be very efficient in resisting any distortion of the lines when loaded, or partially loaded, or light from the original design. The author also speaks about speed in relation to power. If you attempt to drive one of these shallow boats in shallow water at a speed greater than that at which it is meant to go, the power will increase very rapidly, and also if you happen to strike a little shallow water you will find that your boat will require a great deal more power, and it sometimes brings in curious figures that way which look a little con-

flicting. That is to say, you are developing more power at one point, and you are getting in return less speed; but that is probably due to difference in the depth of the water at the place.

Mr. Geo. M. Bond.—It might be well to mention here that the English have, within a few years, adopted this type of steamboat for transportation on the river Nile, having a stern wheel and a long-stroke horizontal engine for the motive power. It is practical enough for that service to warrant them in using such a type. They are made with iron frames, but otherwise are practically the same type as the Ohio and Mississippi river boats.

Mr. Wm. Kent.—There is one thing to be considered in regard to the Ohio River steamboats in contrast to ocean steamers. An ocean steamer is such an enormously expensive thing that you have to provide steam machinery at each end to load and unload it. The investment is so great that we must economize time to the utmost. But in designing a Mississippi or Ohio steamboat you must count on that boat being laid off for a whole month on a sand bar, or bank, waiting for the water to rise, and everything must be designed with reference to the time of service of the boat—the number of weeks in the year that she can run.

Mr. Horace See.—I think the designers of the original American river steamers, both East and West, displayed great engineering skill, in that they had to adopt a design suitable to the material and facilities on hand to build the hulls and machinery with. In the East, having nothing but wood with which to build the hulls, they had a weak vessel, and one which readily altered its shape. The same state of affairs existed in the West. The working-beam engine was adopted in the East, on account of its flexibility, as was the horizontal long-stroke side and stern-wheel engine in the West. The alteration in the shape of the hull did not interfere with the proper working of these engines, where a rigid form was out of the question. In the East we have been able, on our deep rivers, to introduce the screw, which is rapidly displacing the side-wheel steamer. I would not be at all surprised if in ten or fifteen years there would not be a single side-wheel steamer built in the East for general navigation. In the West, on account of the shoal rivers, it is a different and more difficult question about the general adoption of the screw propeller for driving a boat. Thornycroft has adopted the guide-blade propeller for light craft, and some of his boats are running in twelve inches of water. He employs one, two and three screws. By multiplying them, a great

deal of surface can be obtained with very light draft. The propellers are run up to 500 or 600 revolutions a minute. The slip of the screw is very great, being as high as 50% in some cases. Several boats propelled in this way have been built for the navigation of the rivers of Africa. The great difficulty in the way of the general adoption of the guide-blade propeller is that it has to be run in one direction. Backing has been attempted by deflecting the current of water in the opposite direction through the medium of a movable tube. This is a little trappy, and I think has not been very successful.

Mr. Kent.—Your suggestion leads to a new design for a river boat, which I will give to the Society. It is to place a row of four or five, or half a dozen Westinghouse engines, each one driving a little propeller. You can run either one or the whole six of them—the whole plant, with Westinghouse engines and propellers attached, would be very light. Each engine should be placed directly at the stern, with the propeller sticking out of the end of it.

The President.—That is the design which Mr. Thornycroft has in using a number of propellers. He is not limited to the number, so that they will stretch across the stern of the boat.

Mr. Minot.—I would like to ask Mr. Kent if he does not think the inside of a man's pocket has got something to do with building the Western river steamboat?

Mr. Kent.—No, sir; not at all. Some of these men in Pittsburgh who build boats are very wealthy. The richer they are the worse boats they build.

Mr. Minot.—I have never seen a boat of this type on the Hudson river. As Prof. Sweet has said, good things cost more than poor ones. I am firmly of the belief that it is to get something for nothing, or to get something pretty cheap. My advice to a man who wants to sell something pretty cheap is to go west.

The President.—I think you mean low-priced by cheap.

Mr. Minot.—Yes; I mean low priced—costing a small amount of money. The idea is to get a dollar for about sixty-two and a half cents. It used to be the case several years ago that you could do that, but you cannot do it so well now. I do not believe that this type of river steamers is the thing that it ought to be. Now, it may be a lack of engineering, or it may be the size of the man's pocket that owns the steamer. They count on getting down to New Orleans and possibly getting back; if they don't, there isn't much loss.

Prof. Woodward.—I wish to say, Mr. President, in regard to the line of advancement of river navigation, so far as it comes under my observation, it lies in changing the whole scheme. Instead of having large steamers which carry passengers and freight and the like, so far as the traffic between St. Louis and New Orleans is concerned, the tendency is toward a strong but small tug and large barges. That tug usually runs with propellers and they have considerable depth of water—from eight to ten feet. Of course the plan is to have a small boat, which is merely a tug, and let it take from eight to twelve or fourteen barges, each with a ship-load of grain. That is the way freights are carried from St. Louis to the sea. Formerly those barges were knocked to pieces at New Orleans and used for lumber, but of late years they find it cheaper to make a good barge and bring it back empty. Passenger traffic on the river is at a very low ebb.

Mr. C. W. Livermore.—I was going to remark that some years ago I had experience on a man-of-war, making a passage from New Orleans to Vicksburg; and one of the most serious difficulties we had in navigating the river, was to keep the stern bearing in a tight condition, the mud working through would grind it out. I suppose that difficulty is always found where a screw is used.

The President.—Steamers that sail between New York and New Orleans encounter great difficulty in the stern tubes from the sand which will work in when running in the Mississippi; but they have got over a great deal of the trouble by introducing a stuffing box or flexible disc to prevent the sand from working in when backing. The sand is stirred up and thrown into the stern bearing, which it cuts out very rapidly.

Mr. Livermore.—I recall very well being on a mud bank for something like a week, and we had very serious difficulty indeed. The sand would get in in spite of us.

The President.—Some pump the water to supply the lignum vitæ bearing—that is take in clear water to drive the sand out. But that would be a very difficult thing to do on a long voyage in a dirty river.

Mr. Livermore.—It is impossible to get it on the Mississippi—at certain times—it is impossible to get clear water at all.

The President.—I think it is very well that the paper has been presented at this meeting to be incorporated in the proceedings of the Society, as so very little has been published that is

authentic about the Western river steamboat. I think it is matter of considerable historic value, as there will be a time when the present Western river steamboat will pass away and we will want some record of it. There may be something before long found to take its place—some other form of construction.

Mr. Wilcox.—There are two things about Western river steamers I have not heard mentioned here; but they are very important items. Take the large tow-boats that carry a tow of coal from Pittsburgh to New Orleans. For quite a portion of the time the engines are running backwards, the wheel is then to the front. When they come to the curves, of which there are a continual succession from Pittsburgh to New Orleans, they stop the engine and swing around with the current until they get sidewise. As a general thing, the bend is too short for them to get completely around and they back up stream to gain time, so as to enable them to swing around and face the current. When they come to pretty bad curves, the pilot will send down for all the steam the engineer can give him.

The President.—They are licensed to carry 175 pounds?

Mr. Wilcox.—They will swing around with the current and after they get facing for a short stretch, he will put on his cut-off, if he has got it, and economize his steam. Take the coal yards and wood yards running from the mouth of the Ohio clear down to Natchez and Baton Rouge. They have these little propellers; they will hitch on a barge to the steamer and unload the coal without the steamer landing. But the ice and drift-wood in the river is continually breaking the propellers. I have known boats in a trip to Natchez or Baton Rouge having to land twice to repair propeller, and finally being towed into port. With a stern wheel boat having a paddle on, three to three and a half inch oak they frequently carry quite a little wood-yard of spare paddles, and it is a matter of a few minutes to put on a new one. A boat that will go down the Ohio River at the time the ice-drift is running has little left of her wheel but splinters.

Mr. Parsons.—Is that the case when the boat runs bow first? Are they as liable then to lose the paddle?

Mr. Wilcox.—They are liable to in all cases. A piece of drift-wood will float underneath the boat and get in among the paddles. There is another feature, speaking about a railroad running along the side of the river and taking the trade away—the railroad has done worse than that. They put their bridge piers up without proper

regard to the necessities of the river traffic. By earnest effort the limit has been fixed, I believe, at 600 feet span; I think that is the correct figure. But previous to that they were putting up piers without much regard to the direction of the current, may-be right at the middle of a bend, where a man with a tow that is a quarter of a mile long cannot get around. The result is two or three of the forward barges are smashed to pieces and the coal goes to the bottom of the river.

Mr. Kent.—I believe it has been stated in the Pittsburgh papers that that transportation costs less than any other transportation in the world. There is no such cheap transportation as that of the Ohio and Mississippi rivers.

Mr. Wilcox.—I have read somewhere that the registered tonnage of the City of Pittsburgh alone, that is afloat on the Western rivers, exceeds the tonnage of New York and Philadelphia combined. You will find the reports of tonnage in Washington. Of course much of it is a mere barge tonnage.

Mr. Minot.—I understand that that cheap transportation means down stream. They have got the current to carry them down there; it ought to be cheap.

Mr. Rogers.—There is another point in regard to improving Western steamboats that has not been mentioned. It is the same that applies to years ago when they began to improve the locomotives. I recollect that when they put a pop-valve on the locomotive the engineer did his level best to condemn it. It was not as good as the old safety valve, because the duty was fixed, while with the safety valve, if he wanted 160 pounds of steam, he could stick a piece of wood between it and the top of the cab and get it. It is the same with the engineers on our Western rivers. They still stick to that old fogy idea. They want lots of machinery, and they have got it. I do not know of anything on earth that has more machinery than an Ohio River steamboat. There is enough machinery in one of those engines to replace a shop full of steam pumps and to build all the inspirators that Mr. Warren could sell. But they want to get over that, and they need to be educated up to it. I think that our Western river shops will try to keep up that old idea, because the more machinery there is in a boat the more times they have to rebuild it. I have noticed that constantly. The more machinery there is in a boat the more work there is, and the more shops there are along the river, and the engineers stick to that old idea. They do not want an improved valve

motion. They do not want any improvement. Nothing will put water into a boiler like that old upright walking-beam, grasshopper sort of a steam pump they have. If you put in a nice quick method of getting water into the boiler, and getting it in evenly and heated properly, I think the first one will be a dead failure.

*Mr. J. F. Holloway.**—The paper prepared by our member from Wheeling on "River Practice in the West" is, I think, a valuable contribution to the annals of this Society, as well as to the engineering literature of the day. I am led to say so, for the reason that I believe that there is no department of engineering which, in comparison with its age and importance, so little is known about by the members of this Society, and the public in general, as is the one referred to by Mr. Sweeney in his paper. While the marine engine, whose birthplace may be said to have been in the workshops of Mandslay & Field, Wm. Penn & Sons, the Napiers', and others in England and Scotland, has had its historians and illustrators without number, who, in books and magazines and technical journals, have described every possible form and design through which, in their slow evolutions, the monster marine engines of to-day have been evolved, and who have, by drawings and diagrams, shown the minutest bolt and bar of which they are composed until the libraries of engineers and engineering societies are filled with costly and ponderous volumes. The engines and hulls of steamers whose combined tonnage years ago exceeded that of the steam marine of all the world, and which bore upon the bosom of the great rivers of the West a population and traffic which did more than anything else to open up the "Great West," has, so far as I know, no historian, and no printed literature which, all combined, would form one lonely book.

Another reason why engineers are not more familiar with this class of machinery is, that the travel of the present is so largely done by rail that all they see or know of the river practice of the West is obtained by the hurried glances they give from the windows of palace cars as they cross high bridges beneath which the river steamer is slowly passing, or as they whirl past them along the banks of navigable rivers.

A further reason for commending Mr. Sweeney's paper is, that it gives, though far too briefly, the reasons why Western river boats and engines are thus built; and an examination of the subject may lead some, at least, to believe that the machinery still built and

* Contributed after adjournment.

still in use there is not due to ignorance on the part of those who build them, or because they are not well informed as to the progress engineering has made in other places and in other branches; but for the simple reason that both the boats and the hulls now in use on Western rivers, and which, in the main, are the same as they were half a century ago, are the best possible kind that can be made for that particular service.

It was among my experiences to have lived for a few years on the "Lower Ohio," where I had necessity as well as opportunity to know something of the "River Practice of the West."

It was by no means a rare or novel experience to watch the movements and to hear the remarks of the "Eastern man," who, for the first time, surveyed with his critical eye the motive power of The Bald Eagle, or the Amarathe No. 2. Usually he came down from the cabin after dinner with a party of lady friends, who, relying upon his superior knowledge, had availed themselves of his guidance in order to know what it was that "made the wheels go round." While the party were being led about by him, and had pointed out the wooden "pitman" and the clumsy "T head," and were shown the ropes and traps for hanging up the loose, unused "cane hooks," and were listening to his comparisons and his denunciations of the design and workmanship of the machinery, which, with a wave of his hand, he consigned to an oblivion from which, as he thought, it had much too long escaped, there was a small, greasy-looking man wandering about the space between the engines and the rear end of the boilers, jabbing now and then with a broom-handle (which had a string looped in one end) the hissing Mississippi gauge cocks that sputtered in the rear ends of the boiler; varying the excision occasionally by giving the throttle of the "doctor" a twist one way or the other. A close observer might have noticed that as he overheard the remarks made about his engine in particular, and the boat in general, his jabbing of the try cocks became more spiteful, and that he was himself getting up a pressure which, but for the presence of ladies, would have prompted an explosive reply on his part which would have been emphatic if not scientific. But the engineer of a Western river steamer always has at his command an arrangement by the use of which he can at all times drown argument and expostulation, and clear the decks as well—it is the "mud valve;" which, had it been shown in Mr. Sweeney's illustrations, would, I am sure, have been marked with a "big B."

To any one who has traveled on an Ohio or Mississippi River steamer, loaded to the guards, and with the end of the blow-off pipe under water, and has heard the roar of a "five-inch valve" jerked open with a lever, and has felt the hull, from stem to stern quake as if in a ague fit, as the steam and water, under a pressure of 280 pounds, was meeting the water under the guards, it is unnecessary to say that the lecture on engineering in this case was closed and the class scattered. When the unearthly roar and rumble had died away among the hills, and the hull had settled into its usual tremor and the scattered group had come together in the cabin and were wondering how many of the boilers had blown up, there came a moment of supreme triumph to our "Eastern man," when he said: "Oh! I would just like to have a North River steamboat or a Sound steamer here for a little while just to show these backwoodsmen what a steamboat engine is, and what it can do."

At the risk of being myself considered a "relict of the past," I have no hesitation in repeating what Mr. Sweeney's paper so well illustrates, that the steamboats of the Western rivers, in all their important appointments, are the outgrowth of long experience, and careful experiment, as fully and completely, as is the latest "greyhound of the sea," which represent in its design and workmanship, the highest attained art of modern marine engineering practice.

Any one will be greatly mistaken if he supposes there has never been any experimenting done with a view of changing or improving the machinery, now so universally in use on the Western rivers. I will venture to say that the first compound engine that ever turned a wheel on any water craft, was one made years ago for Clipper No. 3, and which for a long time plied between Pittsburgh and Cincinnati. "Befo' the Wa'," travelers on the Ohio, will readily recall that stately and magnificent steamer the Jacob Strader, which was one of the mail boats between Cincinnati and Louisville, and which had a pair of inclined low pressure engines; but which could only run when the river was on "a boom," and which every dweller along the route looked upon as a dead failure, for the reason that they could not hear her exhaust, as she swept around the bends of "La Belle River."

Mr. Sweeney by giving the dimensions of the boats, the size of the various timbers of which they are built, has given the best possible reason for the existence of the kind of engines to be

found in them. Any engineer who had placed before him the problem of holding a 500 or 600 H. P. engine, on a foundation consisting of $2\frac{3}{4}'' \times 6''$ joist, set up on edge, and on the water, and could contrive a better plan than is now and always has been used on Western waters to do so, will accomplish a feat to which the affixing a 6,000 H. P. marine engine to a stiff, steel ship, forty feet deep in the hold, is a trifle. The arrangement, especially in light stern wheel boats, of using long wooden "pitmans," so as to string out the engine and its foundation timbers, to the utmost, so as to be able to notch them on to many floor timbers and thus obtain strength by multitude of the bearings, is very ingenious and must have been found out by some remote engineer, who, having heavy loads to transport over thin ice found that a long light ladder was the best means of obtaining support thereon.

The reason given for the retention of the apparently crude valve gear was needed only by those who had never "made the riffle," on a light draft stern wheeler.

The device shown in the drawings by which a swinging link is made to do service in reversing the engines on a "stern wheel boat," is new to me, and I am not prepared to say that it is in every way an improvement on the old way of handling this class of engines, although it must make the task an easier one. Just what the "old way" of it was, I dare say, few members of the society know, and possibly I may be indulged in the time required to describe it. In the first place there were two long stroke steam cylinders with all their gear attached to one wheel shaft—one being on each side of the boat. Each engine had three hand hooks to manipulate in handling the same. One was a go-ahead hook, one a backing, and the other the cut-off hook.

On some boats where the engineer was somewhat "toney" there were small wooden blocks hung on the "carlines" under the cabin floor and over the engine, in which were rove cords, and as the lower block was fast to the "gab hook" it could thus be raised off the rocker pin instead of being "grabbed" by hand and lifted the usual way. In the center of the boat and between the two steam cylinders was the screw throttle valve. This valve, which was at the end of a very long steam pipe, was placed on a small truck or carriage, which having under it four wheels and a pair of rails to run on, was thus freely permitted to move back or forward to suit the different lengths of the cold or expanded steam pipe, or the still more uneven movements of the hull. In

all machinery there is to be found some part over which it would seem the designer had lingered lovingly, and the "finisher" had parted from with regret. On the Western river engines this particular *pièce de resistance* was the screw throttle valve, or more especially its crank handle and columns. The throttle valve was of the type now so commonly exemplified in a general way by the globe valve and its hand wheel; but it would seem that the person who first designed the throttle of the West, had started out with the idea that a lever made of a long bar of iron would open the valve most easily, but having so made it, he found there was not room enough in which to turn it around, so he then was struck with the further bright idea, that by crimping up the long lever into all sorts of curves, reverse and otherwise, he could get the handle so it would turn in a small orbit, while at the same time he would have the advantage of the long lever; and so ingeniously has he introduced bends and curves into it, that one would have to look at it a good while before he was satisfied that he had not accomplished both results in one. It will be proper to add in this connection, that the lever and handle of the throttle valve, and the piston rods are about the only polished parts of a river engine. The status of a Western boat is always governed by the number of boilers there are on it, and beginning with the most plebeian craft with its two boilers, it ranges in varying degrees up to the noble and imperious floating palaces, which years ago, with their ten or twelve boilers and corresponding crew of musical firemen, were in use, and whose gaping furnaces and monster chimneys canopied with thick volumes of dense smoke, served to form a picture once seen was ever to be remembered. On the small boat, one engineer aided by "a cub" was expected to handle the engines; and I presume it was a recollection of seeing the panting, perspiring engineer rushing from one side to the other of the boat, snatching the "gab hooks" from off the rocker pins, dropping them on the floor, picking up others therefrom and hooking them on, to which was added short excursions to the throttle valve, while over his head the music of what seemed all the bells ringing at once was increased by volleys of emphatic English that came tumbling from out the speaking trumpet, that induced Mr. Sweeney to introduce a swinging link that would enable the engine to be handled from the center of the boat. A link would seem to be an improvement in this respect, but so far it has only been used on the smaller classes of connected engines.

I have not given this subject the careful consideration and discussion it deserves. I have only in a general way endeavored to correct the very common opinion, especially in the East, that Western river boats and their machinery are relics of crude engineering which somehow have so far escaped the attention of modern engineers, and which have not for some reason or other partaken of the spirit of improvement which has so marked all other branches of engineering.

I hope what I have said will induce the "Eastern man" not only to give both hull and engines more careful consideration, but that he will have a more charitable judgment for the men who, without means, and with the aid of but few tools, built up and inaugurated on the borders of newly discovered rivers, and in the wilds of the West, a "river practice," which succeeding generations with their wealth of money and of acquired information have not been able greatly to improve.

*Mr. John M. Sweeney.**—The discussion on this paper demonstrates to me more clearly than ever, how little understood are the actual requirements for this class of navigation among otherwise bright mechanics and marine engineers of undoubted ability. The question is one that can only be decided upon the merits of a class or plan of machinery bearing upon the whole result.

Nothing is more natural than to conclude that compounding would at once give the better results, and just such attempts have been made, and I believe intelligently. Mr. Hartupee, of Pittsburgh, was a great advocate of the application, and built perhaps twenty river boats compounded; two of them are still in service, the steamers John A. Wood and Jos. B. Williams. The owners are progressive men, alive as can be to any saving in cost of their output, but these men have not and would not make another such investment, because the greater weight, more cost of maintenance and repairs overbalance any saving made in fuel.

It is quite as natural to assume that screw propellers would give greater economy. Now let us see what the stern wheel tow boat does; it is urged that because the current does the work of down stream transportation, any machinery can do it cheaply. It is ordinary service to transport 8,000 tons down stream 650 miles in 60 hours; the current averages about 3 miles, so the load of 8,000 tons is transported through an equivalent of 470 miles of still water in 60 hours with a fuel consumption of 64 tons. No screw

* Author's closure under the rules.

propeller could accomplish such work, because simply it could not steer the tow and keep it out of the bank, neither could it carry its own fuel. The great saving quality of the stern wheel boat is its ability to twist the tows into proper shapes to meet irregularities of the channel and bends, banks and bars in the course.

Several methods to secure high piston speeds of engines have been attempted; the engines have been geared to the water wheel shaft in every conceivable way, belts, friction, link chains, spur wheels, etc., but they have all ended by coming back to direct attachments, the preference being to long strokes for the purpose of higher piston speed at given revolutions of the water wheel. There are no propellers used on the Mississippi River for towage excepting for harbor purposes when they have one or two coal boats to change in their berths. In the matter of strains to be met in the chaining of these boats, it is not the known strains which make any trouble, but it is the unknown ones. Such strains as are encountered when the boat runs on a bar, or is badly loaded or unloaded; that is, during the process the freights are not properly distributed. It is a great mistake to imagine that the machinery used is costly in repairs, or that "shops" conspire to make repair bills. I have in mind a boat now running four years, and the whole bill of repairs to machinery has been two and one-half dollars ($2.50).

I cannot close this discussion without a word for the much abused steamboat "doctor." I agree with Mr. Rogers entirely on the safety valve question and as to the average capacity of the Western engineer, but I have spent time and money trying to knock out the "doctor" with Worthington and other pumps, and have always been too glad to get back to the "doctor" for help out of the trouble. I know of no method which puts water into a boiler as quickly or as hot. The same applications of reasons which induce the supply of feed water into a locomotive boiler without heating it from the exhaust—that is, the "expediency"—must be followed in considering all individual cases.

CCCVI.

WIRE ROPE FASTENINGS.

BY WM. HEWITT, TRENTON, N. J.

(Member of the Society.)

Having had occasion to make some tests of the strength of wire rope recently, the difficulty encountered in obtaining fair results suggests a consideration of the relative merits and defects of the fastenings, of which two distinct kinds are employed; one, the splice and thimble, Figs. 275 and 276, and the other by socket, Fig. 277.

Fig. 275

THIMBLE SPLICED IN
Ordinary style.

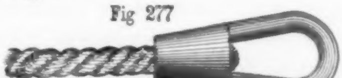
Two styles of splices are used: one (Fig. 275) in which the wires after being frayed out at the end, and the rope bent around the thimble, are laid snugly about the main portion of the rope and securely fastened by serving or wrapping with stout wire; the extreme ends which project below this wrapper being folded back,

Fig. 276

THIMBLE SPLICED IN.
Strands Interlocked.

as shown at *a*. The other style (Fig. 276) is by interlocking the strands in the usual manner of splicing, and also wrapping with wire as in the first method; the latter mode of fastening possessing the greater strength.

Fig 277



LOOP SOCKET.

The socket (Fig. 277) is a block with a conical hole in which the rope is secured by fraying out the wires at the end to conform in

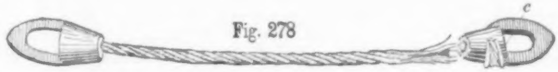
shape with the conical form of the aperture, the interstices between the wires being filled up with spikes or nails, which are driven in as tightly as possible, and the whole finally cemented with molten Babbitt metal. This is a much neater fastening than either of the preceding, but as usually made, does not possess anything like the strength. In the first tests, the specimens were secured by means of thimbles spliced in at the end, as in Fig. 275. The objection to this mode of fastening, is the thimble. This is usually made of a piece of curved metal bent around into an oval shape, as shown in Figs. 275 and 276, with the groove outside in which the rope lays, and the ends coming together in a sharp point at x. In the tests, one extremity of the thimble usually wedges itself beyond or past the other, cutting one or more of the strands, and the rope almost invariably breaks in one of the splices. This difficulty with thimbles led me to try some experiments with sockets; notwithstanding the fact that Mr. N. O. Olsen who conducted the tests, and who had made many tests of wire ropes, had repeatedly stated that no satisfactory test of a wire rope could be obtained with this style of fastening. Moreover, as Mr. Olsen's statement was somewhat of a surprise to me, and as this style of fastening, I believe, is most generally applied in elevators and hoists of all kinds, the matter struck me as of sufficient importance to be worthy of some investigation, if for no other purpose than to determine the holding power of the fastening. The tests corroborated Mr. Olsen's statement, as in every case the rope pulled out of one of the sockets under a load varying from one-half to three-fourths of the breaking load of the rope. The wires pulled out so clean from the Babbitt metal in which they were imbedded, and under such varying loads, that the defectiveness and insecurity of this mode of fastening was very apparent. It is a style of fastening so much neater than the other, however, and so much more desirable a way of securing the test specimens in order to obtain a fair test of strength of the ropes, that the matter seemed to be worthy of further experiment; and having occasion to test a cast steel rope 1½ inches in diameter, a sample was prepared with a socket at one end of the following construction: The wires, after being frayed out at the end, were bent upon themselves in hook fashion, the prongs of some being longer than others, so that the bunch would conform as nearly as possible to the conical aperture of the socket, and the melted Babbitt metal finally run in as usual. The other end was provided

with a thimble spliced in as in Fig. 276; the splice, however, was not made long enough, and the strands pulled out under a load of 129,320 pounds. The length of the splice was about 18 inches. The wires in the socket were unaffected.

As it was important to obtain a good test of this rope at the time, as soon as possible, and as some doubts were entertained as to the holding power of the socket, notwithstanding it withstood a load of nearly 65 net tons, the next sample was prepared with thimbles spliced in at each end; the thimbles in this case, however, being made from a solid piece of metal much heavier than usual, and circular in shape. Five strands broke in one of the splices, and the other pulled out under a load of 142,800 pounds, which was considerably below the estimated strength of the rope, indicating that the strand which pulled out had borne no portion of the load.

The next rope tested was one of cast steel, 2 inches in diameter with a wire center or core. As it is difficult and unusual to splice thimbles in ropes of this size, and as the last experiment with the socket was so encouraging, the test specimen of this rope was prepared in a similar manner. The wires were so coarse and stiff, however, that it was difficult to lay them snugly and compactly in the socket, and the wires pulled out of one of the sockets under a

Fig. 278



load of 228,400 lbs. (Fig. 278). About twenty of them, however, parted in the socket, the piece taken from the socket presenting the appearance shown at *c* (Fig. 278).

Another specimen of the same rope was then prepared as follows: The wires were frayed out and bent over as before, with the exception of those in the core, the latter being surrounded with a narrow conical thimble or annular wedge, which was driven in tight, forcing the wire of the exterior strands firmly against the socket. The core wires were then bent over like the others, the center filled with some fine steel points, and the molten Babbitt metal finally poured in. Instead of using ordinary loop sockets, however, loop stirrups were employed (Fig. 279), which were secured in the testing machine by shackle pins three inches in diameter. In this case the wires in the socket, with the Babbitt metal, in which they were imbedded, pulled out about three-quarters of an inch, and the rope then broke under a load of

266,250 pounds. Fig. 279 illustrates the appearance of the specimen after fracture.

Fig. 279

The U of the stirrup, which was made of a bar of mild basic steel $2\frac{1}{2}$ inches diameter, was also slightly cracked at *b* on the inside of the bend, due to the fact that the bend was made too sharp and opened in coming to a seat against the shackle pin. This rope was made of the Trenton Iron Company's special cast-steel wire, and was composed of 6 strands, each 19 wires, surrounding a core also of 19 wires, the average tensile strength of the individual wires per square inch being 190,000 lbs.

Other data are as follows:

Diameter of each wire	0.137 inches.
Lay of wires in the strand	6 "
Lay of strands in the rope	$12\frac{3}{4}$ "
Length of rope	5 feet.
Length of rope between sockets	3 "
Weight of rope	35 lbs.
Weight of each stirrup	407 "

The aggregate tensile strength of the individual wires amounting to 372,400 pounds, it appears therefore that the strength of the rope was $71\frac{1}{2}\%$ of this figure.

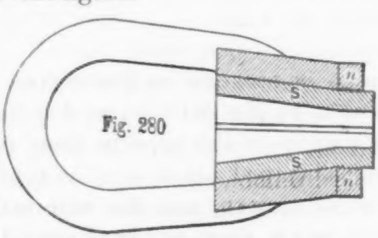


Fig. 280

Another mode of attaching the socket (and perhaps a better one) is that shown in the accompanying sketch (Fig. 280).

The socket is bored out to the desired conical form and fitted with a shell or bushing S, made in three pieces, and provided at the small end with a screw thread. The wires at the end of the rope are frayed out, but not to such an extent as in the preceding case, and bent back against the body of the rope, some of the

prongs being longer than others in order to produce the desired conical bunch or head, which surrounded by the shell S is inserted in the socket; the construction being such that the threaded portion of the shell projects below the socket sufficiently to accommodate a nut n, by which the shell is drawn tightly into the socket, thus firmly clamping the wires. Molten Babbitt metal can finally be poured in if necessary.

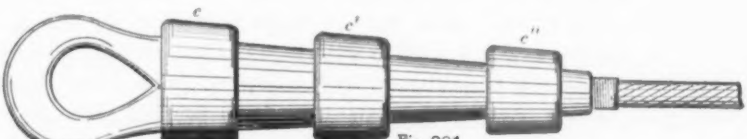


Fig. 281

WINDING SOCKET WITH WIRES TURNED BACK OVER CONE -- SECTION OF CONE.

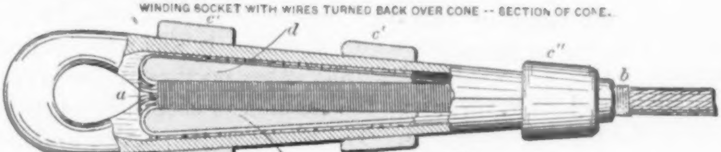


Fig. 282

WINDING SOCKET WITH WIRES TURNED BACK OVER CONE -- OUTSIDE VIEW OF CONE.

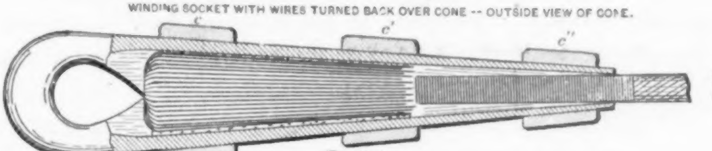


Fig. 283

A socket which meets with much favor in Great Britain is illustrated in Figs. 281, 282 and 283.

The sockets are forged in one piece, the eye being afterward bent in. The rope to be socketed is first served with annealed wire from the point a to the point b (see Fig. 282). The rings c, c', c'' are slipped up over the rope in their respective order followed by the conical-shaped block d. The rope is then frayed out from the end of the serving, and the wire ends are so trimmed in layers that when they are bent over the block cone d they will lie smoothly and regularly on its outside surface. The eye of the socket-forging is heated red-hot and bent over the rope and closed on an anvil. The rings c, c', c'' are then driven home to their respective places. To prevent the slipping of the rings, the metal of the socket is generally upset against them with a cold chisel.

For still further security, the socket is then poured full of Bab-

bitt metal in the usual way. An improvement suggested by the preceding socket, which I have described, is to cut a thread on the lower end of this socket and force the rings up to and maintain them in their respective positions by a nut which would close the small end of the socket; a light gas-pipe sleeve being fitted between each ring.

I regret that the data in connection with these tests are not as complete as might be desired, so that a more interesting comparison of the results could have been made in regard to the strength of the ropes, but I doubt if such data would have developed anything more than was determined by the experiments of the Board appointed by the Navy Department in 1876 for the purpose of making certain tests of iron and steel wire and wire rope. The report of this Board states that "one specimen of each kind of rope was tested with a splice on one end. It was not observed that the splice was weaker than any other portion of the rope." Nothing is said, however, as to the manner of securing the specimens in the testing apparatus. The report further states that 462 specimens of rope were broken. Table E exhibits a record of the tests from 153 coils. The ratio of the breaking load of the rope to the aggregate breaking load of the individual wires for each size of rope was as follows:

$3\frac{3}{8}''$ cir.	Average of	9 coils	79.21%
$3\frac{1}{4}''$ "	"	" 21 "	81.61%
$3\frac{1}{8}''$ "	"	" 18 "	82.86%
$3''$ "	"	" 27 "	82.32%
$2\frac{7}{8}''$ "	"	" 6 "	82.76%
$2''$ "	"	" 6 "	84.61%
$1\frac{7}{8}''$ "	"	" 24 "	83.94%
$1\frac{3}{4}''$ "	"	" 30 "	85.87%
$1\frac{5}{8}''$ "	"	" 12 "	83.70%

The figures indicate that the ratio is greater as a rule in the smaller ropes, as would naturally be expected, since the wires being more pliable are laid under a more uniform tension.

The following are a few of the first tests, in which the sockets were of the ordinary style.

Diameter.	Kind of Material.	Kind of Fastening.	Breaking Load.	Where Fractured.
1″	Cast steel.	Socket.	27,430 lbs.	Pulled out of socket.
1″	" "	Thimble spliced in.	58,000 "	Broke in top splice and cut one strand at bottom.
7/8″	Swedish iron.	"	22,430 "	At bottom thimble.
7/8″	" "	"	22,390 "	At top thimble.
3/4″	" "	"	21,670 "	At bottom thimble.
3/4″	" "	"	19,450 "	Cut at top thimble.
3/4″	Cast steel.	"	28,870 "	One strand broke in several places.
3/4″	" "	Socket.	23,140 "	Pulled out of socket.
5/8″	" "	"	14,290 "	Pulled out of socket.
5/8″	" "	Thimble spliced in	21,900 "	Two strands broke in splice.
5/8″	Swedish iron.	"	12,730 "	In the body of the rope.
5/8″	" "	"	14.060 "	In the body of the rope.
1/2″	Cast steel.	"	12,930 "	Broke a one-inch pin before rupture; gave way in two strands.
1/2″	" "	Socket	8,850 "	Pulled out of socket.

DISCUSSION.

Mr. J. T. Hawkins.—I would like to ask Mr. Hewitt, if in the course of his experiments in this matter he has ever, in the use of the conical sockets, known of a metallic union being made between the wires and the soft Babbitt metal incorporated between the wires? inserting, for instance, ordinary solder—first putting in soldering fluid to make the union between the wires and the solder perfect, and raising the temperature so as to solder the whole together. I notice the paper in one place describes the wire pulling out from the soft metal or the soft metal squeezing out from the wire. It occurred to me that in such cases it would be an improvement to make a complete union between the wires and the soft metal. Again it occurred to me, where that was not done, whether it would not be a good plan to finish out the interior cone of the socket so that in the slipping of the wire and the soft metal with it, it would be more likely to crowd the whole together and prevent the one drawing away from the other, having a certain angle to permit of that, and also whether the experiments have determined the proper angle for this cone. These all seem to me to be questions having an important bearing upon this method.

Mr. Hewitt.—So far as my experience has gone, I do not know of any different kind of union—any special union, such as Mr. Hawkins described, having been made between the wires and the metal in which they are imbedded. The usual way in which that

is done in the shop is simply to pour the metal in after the wires are arranged in the socket. So far as the taper is concerned, I have never made any experiments to ascertain just what the best taper is. It would be an interesting thing to determine.

Mr. Jacob Reese.—Mr. Hewitt has given us some data which will prove useful. In the wire Suspension Bridge built at Pittsburgh in 1861 by the Roeblings, the smaller ropes were furnished with loop sockets such as are shown in Fig. 277 of Mr. Hewitt's paper, while the larger cables were attached to stirrups such as are illustrated in his Fig. 279. The ropes were put into conical openings in the sockets and stirrups, the ends of the wires turned back and keyed, and the interstices filled with Babbitt metal. I am informed by those connected with the bridge that not a single rope has pulled out since the bridge was built, but it must be remembered that the strain has not exceeded 50 per cent. of the tensile ability of the ropes. Mr. Hewitt has shown us what the results would be if the wires were subjected to their limit strain. I think his data on breaking loads and points of fracture are of the greatest value.

Mr. Hewitt.—In regard to the taper Mr. Hawkins referred to, while I cannot give any accurate information on that point, as most of the sockets were patterned from existing models, yet I can state we have had difficulty with some sockets on account of the taper being too short. We found by lengthening the socket and narrowing the taper that it was a decided improvement. Our experience indicates that a long narrow taper is preferable to a short one.

CCCVII.

THE DISPLACEMENTS AND THE AREA-CURVES OF FISH.

BY HARRY DE B. PARSONS, NEW YORK CITY.

(Member of the Society.)

FOUR years ago, while engaged in studying the lines of fish, the author's attention was drawn to the subject of this paper by noticing some peculiarities in one or two specimens examined.

On following the subject, as time would best permit, on account of the frequent interruptions from office life, the author now submits to this Society the present paper, containing the results of his investigations.

Although the following paper is not strictly mechanical, the author decided to present it to this meeting from the fact that Naval Architecture is so closely related to our branch of engineering, that any information which would tend to advance our knowledge of that subject would also be of interest to the Mechanical Engineer.

It is also intended to complete this subject in the near future, by making a comparison between the lines of fish and those of the best-designed ships of the present day.

By the term "*fish*" throughout this article, unless otherwise stated, is meant that class of vertebrate animals which, swimming in water, have a "*fish-like*" form. It thus refers in this sense not only to fish proper, but also includes some of the cetacea, as for instance the whale, dolphin, etc.

At the inception of this investigation, the author's attention was very forcibly attracted by noticing that the forms of certain fish bore to each other strong characteristics of resemblance, irrespective of their size or age, their habits or geographical homes.

If this is true, namely that the possession of certain qualities of form is common to all *fish*, then this resemblance is not a mere accident of nature, but rather a gradual growth or evolution of

that form best adapted to the surrounding conditions of fish life; and again, if this is so, then there must be a general law, to which all fish approximate, according to their peculiar and individual requirements.

On account of the dense medium in which fish live, and the resistance which it offers to motion, it is only just to consider that whatever this law of nature may be, it will represent a form approaching that of least resistance for the work which it has to perform under the conditions which necessarily accompany it. Thus, we know that for any given length of body, the form must offer small resistance, while at the same time it must be of such a size as to contain the necessary muscular development either for the purpose of prey, defense, or swift propulsion; or for any combination of these characteristics. As an example, we may mention the salmon (Salmo salar), whose native haunts are both the sea and swift-running streams, and is endowed with remarkable powers in comparison to its length, being one of the strongest fish known.

Now, it is this *law* which we shall endeavor to examine in the following pages:

Fish are usually divided by ichthyologists into three parts—the head, the body and the tail. The head extends from the tip of the snout to just behind the branchiæ or gills; the body is that portion included between the head and the tail, while the line of demarkation between these latter divisions is ill defined, and is sometimes taken at the vent. But all these parts are united and joined into one smooth, continuous form, and in this respect fish differ from other well-developed forms of vertebrates.

Fish propel themselves by several different methods; some, by sucking up and ejecting the water in which they are immersed; some, by an alternate dilatation and contraction; some, by increasing and diminishing the extent of surface presented, a principle similar to the feathering of an oar; while the largest and most important class, and the one which includes all the forms of fast-swimming fish, propel themselves by means of a caudal fin, to which they give a rapid, vibrating, curvilinear movement; and it is to this latter class that we shall limit our attentions. These fish are provided with other fins beside the one just mentioned, occurring both singly and in pairs, and are chiefly used for balancing and steering. The work of locomotion falls almost, if not entirely, on the caudal fin, and in consequence its muscles

are strongly developed, and are arranged along the spinal column, constituting the bulk of the animal, especially toward the posterior extremity, as will be shown later. With the cetaceans, as the whale, porpoise, manatee, etc. (included in this class with fast swimming fish on account of their fish-like forms), the caudal fin is rotated on its axis through 90°, but the principles of locomotion remain unaltered, the fin acting vertically instead of horizontally, which materially assists the animal in maintaining its balance.

The shape of the tail, or rather the caudal fin, differs considerably. In many cases these fins are very unsymmetrical or heterocercal, one lobe being more developed than its mate; but the great majority of fast-swimming fish of the present day are homocercal in this respect. We have noted the three divisions —the head, the body and the tail; and also that the line of separation between the last two is not easy to understand. In this sense the word "*tail*" includes more than the caudal fin, and extends far into that portion of the fish which is ordinarily, though wrongly, spoken of as the body. This may be noticed very distinctly when we watch the movements of a fish in the act of swimming. Its tail, bent first on one side and then on the other, forms a curve which extends far up toward the middle of the form and terminates only at a point quite remote from the caudal fin.

For the sake of studying the displacements and area curves, we shall conceive the fish as consisting of two principal parts, viz.: *first*, the *Entrance*, including all of the fish from the tip of the snout to the position of the greatest area of cross section; *second*, the *Run*, including all of the fish from this section to the tip of the caudal fin. These terms, as well as others that may occur, borrowed from naval architecture, are used in their original meanings.

The fish selected for measurement were all well-known species, most of them common to North American waters. Their names are given on the accompanying list, as also a note from what the measurements were taken. The initials N. M. stand for "National Museum" at Washington. The last five mentioned are cetacea.

LIST.

		Measured from
1.	Salmo salar (salmon)	Nature.
2.	Salmo confinis (trout)	Plaster cast.
3.	Gadus pollachius (pollack)	Nature.
4.	Pomatomus soltator (blue-fish)	Nature.
5.	Pomatomus soltatrix (blue-fish)	N. M., No. 22,276
6.	Labrax lineatus (striped bass)	Nature.
7.	Tetrapturus albidus (bill-fish)	N. M.
8.	Scomberomorus maculatus (Spanish mackerel)	N. M., No. 25,635
9.	Scomberomorus cavalla (Cuban Spanish mackerel)	N. M., No. 16,478
10.	Orcymus thynnus (horse mackerel)	N. M., No. 16,509
11.	Coryphæna hippurus (dolphin)	N. M., No. 16,441
12.	Seriola dumerili (amber jack)	N. M., No. 16,709
13.	*Tursiops tursio (bottle-nosed dolphin)	N. M.
14.	*Grampus griseus (grampus)	N. M.
15.	*Phocæna lineata (striped porpoise)	N. M.
16.	*Delphinapterus catodon (white whale)	N. M., No. 12,490
17.	*Megaptera longimana (humpback whale)	N. M., No. 13,656

As will be seen, some measurements were made from models in the United States National Museum, Washington; one, the Salmo confinis, from a plaster cast kindly loaned by Mr. Eugene G. Blackford, of New York, while the rest were measured directly from nature and immediately upon the fish's capture. In order to insure greater accuracy, only large specimens were chosen, when the measurements were made from models (except the Salmo confinis), and all the results reduced to a common scale, smaller than the smallest fish, as will be explained hereafter.

The author desires to acknowledge his indebtedness to the late Prof. Spencer F. Baird, and to Prof. G. Brown Goode, for their valuable assistance in placing the models of the National Museum and of the Smithsonian Institution at his command.

The measurements were made as accurately as possible, and, with two or three exceptions, by the author in person. The smaller fish were gauged by the use of callipers and a steel scale. Selecting sections at regular intervals and at right angles to the axis of the fish, measurements were so taken that the area could be plotted on paper, and then measured with a planimeter. With the larger specimens a system of projection was adopted; that is, measurements were taken from each successive point on the fish to two planes of projection, at right angles to each other, but both parallel to the longitudinal axis of the fish. To illustrate, assume the model of the fish to be placed against the wall of a

* Cetacea.

room, with its longitudinal axis parallel to both the floor and the wall. Then the planes of projection would be the surfaces of the floor and the wall.

By the "*length*" of the fish is meant in "*all*" cases the "*extreme length from the tip of the snout to the tip of the caudal fin.*" This length was chosen as the most accurate means for comparing the fish, for it is not only the simplest, but any other method carries with it more or less elements of uncertainty.

On referring to the curves of areas, we will now be able to compare directly the points of difference and of similarity. Each curve represents the "*curve of areas*" of the fish as numbered and named; and the area represents the total displacement. The horizontal distance represents the length of the fish reduced to ten inches in the original drawings, from which the accompanying plates were reproduced, and the vertical ordinates, the areas of the corresponding sections. Each area of section was reduced to correspond to the scale length; that is to say, what the areas would have been had all the fishes had an original length of ten inches. By using this small scale, all errors of measurement are reduced as much as possible, and we are enabled directly to compare one curve with another in deducing any conclusions.

In vertical scale, each lineal inch represents one square inch of section. The curves represent the snout on the left hand, and the caudal fin on the right.

In Table I. are given in square inches the "*scale areas*" of cross sections taken one inch apart. For the Tetrapturus the length is taken from the snout to tip of the caudal fin, and not from the end of the bill, which protrudes 13 inches in advance. The scale areas of section were calculated for this length.

The greatest cross section is represented by the *dash and dotted line.*

TABLE I.

Scale of areas of cross section in square inches. Ordinates for the curves.

Name of Fish.	Snout.	Ordinates of Curve of Areas.									Caudal Fin.
1. Salmo salar	0	0.80	1.48	1.72	1.74	1.56	1.20	0.78	0.36	0.18	0
2. " confinis	0	0.90	1.46	1.88	2.06	1.79	1.17	0.60	0.20	0.06	0
3. Gadus pollachius	0	0.76	1.55	2.22	2.21	1.64	1.05	0.60	0.28	0.18	0
4. Pomatomus soltator	0	1.04	1.60	1.90	1.96	1.60	1.12	0.68	0.30	0.20	0
5. " soltatrix	0	0.92	1.33	1.47	1.44	1.11	0.67	0.29	0.08	0.02	0
6. Labrax lineatus	0	0.86	1.95	2.61	2.63	2.05	1.48	0.94	0.40	0.13	0
7. Tetrapturus albidus	0	0.25	0.63	0.85	0.83	0.59	0.40	0 25	0.12	0.03	0
8. Scomberomorus maculatus	0	0.51	0.93	1.19	1.20	1.10	0.84	0.40	0.18	0.04	0
9. Scomberomorus cavalla	0	0.50	0.90	1.16	1.20	0.96	0.63	0.32	0.13	0.04	0
10. Orcymus thynnus	0	1.10	2.23	2.96	3.13	2.56	1.41	0.60	0.20	0.04	0
11. Coryphæna hippurus	0	0.61	0.95	1.10	1.10	0.81	0.40	0.15	0.08	0.03	0
12. Seriola dumerili	0	1.05	1.51	1.69	1.60	1 10	0.61	0.30	0.09	0.04	0
13. Tursiops tursio	0	0.58	1.48	2.05	2.06	1.63	0.95	0.41	0.15	0.03	0
14. Grampus griseus	0	1.28	2.80	3.86	3.93	2.98	1.65	0.85	0.34	0.10	0
15. Phocæna lineata	0	0.79	1.68	2.08	2.07	1.57	0.93	0.40	0.18	0.06	0
16. Delphinapterus catodon	0	1.03	2.10	2.75	2.76	2.30	1.40	0.71	0.28	0.07	0
17. Megaptera longimana	0	1.30	2.46	3.03	2.95	2.50	1.73	0.76	0.28	0.08	0
18. Average of second means	0	0.80	1.50	1.92	1.92	1.56	0.98	0.50	0.20	0 08	0

On closely examining these curves, their general uniformity is at once made apparent. There are some differences in the curves, which may be due in a large measure to the age and condition of the fish. Thus, while many have a "full" curve between the snout and the greatest cross section, as is represented by the Pomatomi (male and female), (Figs. 287, 288), the Seriola (Fig. 295), and the Coryphæna, (Fig. 294); many others have a "hollow line," as the Tursiops (Fig. 296) and Phocæna (Fig. 298); and, again, others show an almost straight entrance. This dissimilarily will be referred to later on. The curves of the run—that is from the greatest cross section to the tip of the caudal fin—are similar for all the fish, being a smooth and very hollow curve, tapering off to great fineness at the posterior extremity, as might have been expected.

An important point to which attention is especially called, is the wonderful uniformity in all the specimens of the position of the greatest cross-section, being situated as will be seen, at 36 per cent. of the total length from the snout.

As far as the author is aware, this is the first time this peculiarity has been noticed.

In 1884, the author described this in an unpublished thesis, and extracts were made from it in a paper read before the Insti-

tution of Naval Architects April, 1, 1887, entitled the "Forms of Fish and of Ships."

Some of the figures as printed in that article were wrong.

In only one instance, the Salmo confinis, is the position of this section distorted, so to speak, from the above proportion, and it undoubtedly was due to the fact that the cast was incorrect, probably having been made after the fish had been dead long enough to swell, and so alter its original form.

The importance of this strict law is enhanced, when we recollect that it holds equally true for both the fish and the cetacea, while they vary in length from twelve inches to thirty feet, regardless of their displacements and regardless of their speed of locomotion. We are unable to state how fast these fish are capable of propelling themselves through the water; but we do know that while some are credited with great speed, others are comparatively very sluggish, yet they, one and all, obey this common law. With this one exception just mentioned, the variations from the above proportion are so slight, being within two per cent., that they may be treated simply as errors in measurement.

In the list given in Table II., are the lengths of the fish in inches; the distances from the snout to the greatest cross section in percentage of length; and a third column, to which reference will be made later.

Table III. contains, in the first column, the scale area in square inches of the greatest cross section, and in the three others, the displacements in cubic inches of the entrance, of the run, and the total respectively.

On comparing the figures given in the first column, we notice that there is no fixed relation between the areas of the greatest cross sections. Thus, the areas of the Tetrapturus, Fig. 290, the two Scomberomori, Figs. 291 and 292, and the Coryphæna, Fig. 294 are very small, while on the same scale the areas of the Orcymus, Fig. 293; the Megaptera, Fig. 300, and the Grampus, Fig. 297, are quite large; and we know from observation that both the Tetrapturus and Orcymus, the former having a small area and the latter a large one, are capable of great speed of locomotion.

The areas of the greatest cross sections of the other fish on the list, vary between those of the Tetrapturus and Grampus.

The scale areas of the Tetrapturus were calculated on the length of the fish without the bill, or thirteen inches shorter than the total length, as has been noted above.

TABLE II.

Name of Fish.	Length in inches.	Distance from snout to	
		Greatest cross section.	End of curve of replacement.
		Per cent.	Per cent.
1. Salmo salar	30	37	73
2. Salmo confinis	12.28	40	72
3. Gadus pollachius	13 2	36	71
4. Pomatomus soltator	25	36	72
5. Pomatomus soltatrix	27	36	72
6. Labrax lineatus	25	36	72
7. Tetrapturus albidus	139		
Same, without bill	117	36	71
8. Scomberomorus maculatus	30.5	36	74½
9. Scomberomorus cavalla	42.5	36	72
10. Orcymus thynnus	42	37	71
11. Coryphæna hippurus	35	36	64
12. Seriola dumerili	47	34	69
13. Tursiops tursio	90	36	72
14. Grampus griseus	63.5	36	70½
15. Phocæna lineata	65	36	72
16. Delphinapterus catodon	140.5	36	72
17. Megaptera longimana	368	36	72½
Average	....	36	72

TABLE III.

Name of Fish.	Scale Area Greatest Cross Section sq. inches.	Scale Displacement, cubic inches.		
		Entrance.	Run.	Total.
1. Salmo salar	1.76	4.42	5.31	9.73
2. " confinis	2.06	5.45	4.59	10.04
3. Gadus pollachius	2.34	4.84	5.57	10.41
4. Pomatomus soltator	1.98	4.99	5.58	10.57
5. " soltatrix	1.50	4.05	3.48	7.53
6. Labrax lineatus	2.82	5.85	7.20	13.05
7. Tetrapturus albidus	0.90	1.85	2.13	3.98
8. Scomberomorus maculatus	1.24	2.87	3.61	6.48
9. " cavalla	1.23	2.82	3.11	5.93
10. Orcymus thynnus	3.14	6.92	7.36	14.28
11. Coryphæna hippurus	1.16	2.96	2.48	5.44
12. Seriola dumerili	1.70	4.19	3.84	8.03
13. Tursiops tursio	2.14	4.37	5.06	9.43
14. Grampus griseus	4.03	8.50	9.40	17.90
15. Phocæna lineata	2.12	4.80	5.11	9.91
16. Delphinapterus catodon	2.83	6.30	7.36	13.66
17. Megaptera longimana	3.07	6.95	8.26	15.21

We cannot state, or even give a rough approximation to the peed at which these fish are capable of swimming, but while we

do know from experience that some are swifter in motion than others, there does not appear to be any relation between the area of greatest cross section and the speed.

From an examination of the total displacements, given in column four, Table III., we do not find any fixed relation between either the displacements themselves or between the displacements and the speed of locomotion.

In general, we may say, that the more powerful the fish, the larger the displacement; and since with some the muscular development is abnormally great, which affects the displacement, it does not necessarily increase the speed, for this strength may be for the purpose of defense, offense, or maneuvering powers.

These displacements must vary considerably with the age and physical condition of the fish, although the general form, or rather the relations between the successive areas of cross section remain unaltered.

There does not seem to be any fixed agreement whatever between the greatest areas of cross section, the displacements and the speed; nor between the square roots of the areas and the cube roots of the displacements. From an examination of the tables, it appears that each fish is independent in regard to these points, and is given a form best suited for the peculiar circumstances which may surround its individual existence.

Aside from the areas and the displacements, the fish agree remarkably in all the other essential points, as in the general distribution of their displacements, in the position of the greatest cross section, in the relation of the caudal fin to the rest of the body, and in their coefficients of fineness.

These coefficients of fineness are given in Table IV., and are (if we may be permitted to use the term) the mid-fish section cylinder coefficients. In comparing them, one must keep in mind that the differences in age and physical conditions of the fish at time of measurement exert a considerable influence on these coefficients, and will account, in a large degree, if not entirely, for the variations, as stated.

It will be noticed that, in general, those fish which have the greatest muscular development in proportion to their weight, have the largest coefficient of fineness. Thus, the Salmo salar (salmon) has the largest coefficient, namely, 0.553, and is probably the most powerful fish for its size on the list. It is not, perhaps, the swiftest, but it is capable of propelling itself against currents

which would force back most of the others that might attempt to stem them. Next to the Salmo salar comes the Pomatomus (bluefish), whose powers of exerting its strength are well known to anglers, and in consequence it enjoys the reputation of being one of the gamest of fish.

TABLE IV.

Name of Fish.	Coefficient fineness.	Ratio of distance from Snout to Vent to total length.
1. Salmo salar	0.553	
2. Salmo confinis	0.487	
3. Gadus pollachius	0.445	
4. Pomatomus soltator	0.534	
5. Pomatomus soltatrix	0.502	0.500
6. Labrax lineatus	0 463	
7. Tetrapturus albidus	0.442	0.479
8. Scomberomorus maculatus	0.523	0.491
9. Scomberomorus cavalla	0.482	0.470
10. Orcymus thynnus	0.455	0.548
11. Coryphæna hippurus	0.469	0.429
12. Seriola dumerili	0.472	0.521
13. Tursiops tursio	0.441	0.631
14. Grampus griseus	0.444	0.716
15. Phocæna lineata	0.467	0.631
16. Delphinapterus catodon	0.482	0.672
17. Megaptera longimana	0.495	0.687
18. Average	0.476	

The maximum variation above the average is only 7.7 per cent., and below 3.5 per cent., or a total variation of 11.2 per cent. on the volumes of the circumscribing cylinders. It will be noted that the average coefficient is about that of vessels having very fine lines and designed for high speed.

In the second column of the same table are given the ratios of the distance from the snout to vent to the total length. If we take the position of the vent as indicating the posterior extremities of the internal organs of the fish, which it does in most instances, and that all behind the vent is the "*tail*," consisting of the caudal fin and its controlling muscles, then it is curious that the ratio of the distances from the snout to the vent to total length, should be so much smaller for the fish than for the cetaceans. Thus, the average ratios are:

For the fish	0.491
For the cetaceans	0.667

1.

Salmo salar.

Salmon.

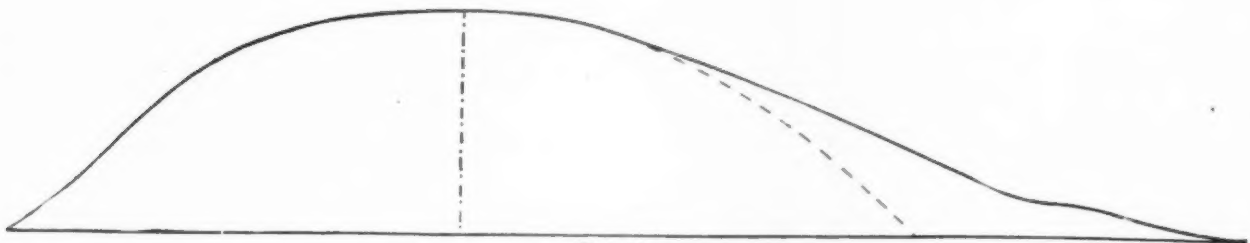


Fig. 284.

2.

Salmo confinis

Trout.

Fig. 285.

3.

Gadus pollachius.

Pollack.

Fig. 286.

4.

Pomatomus soltator

Blue Fish.

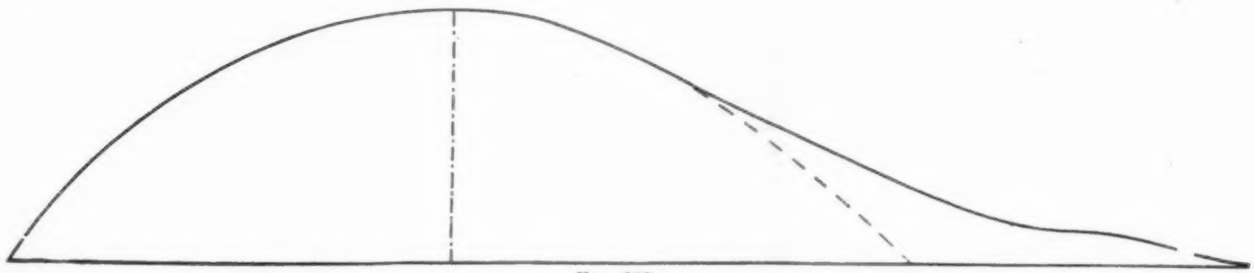


FIG. 287.

5.

Pomatomus soltatrix

Blue Fish.

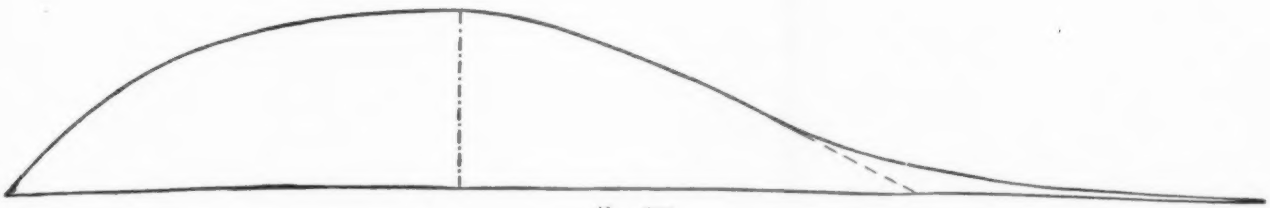


FIG. 288.

6

Labrax lineatus.

Striped Bass.

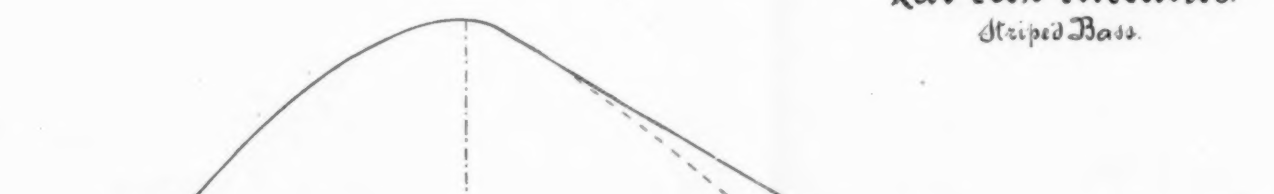


FIG. 289.

7.

Tetrapturus albidus
Bill Fish.

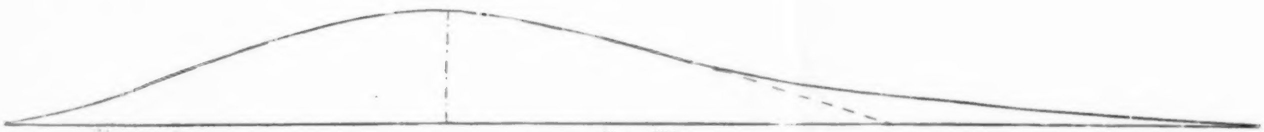


FIG. 290.

8.

Scomberomorus maculatus
Spanish Mackerel.

FIG. 291.

9.

Scomberomorus cavalla.
Cuban Spanish Mackerel

FIG. 292.

10.

Orcynus thynnus.

Horse Mackerel.

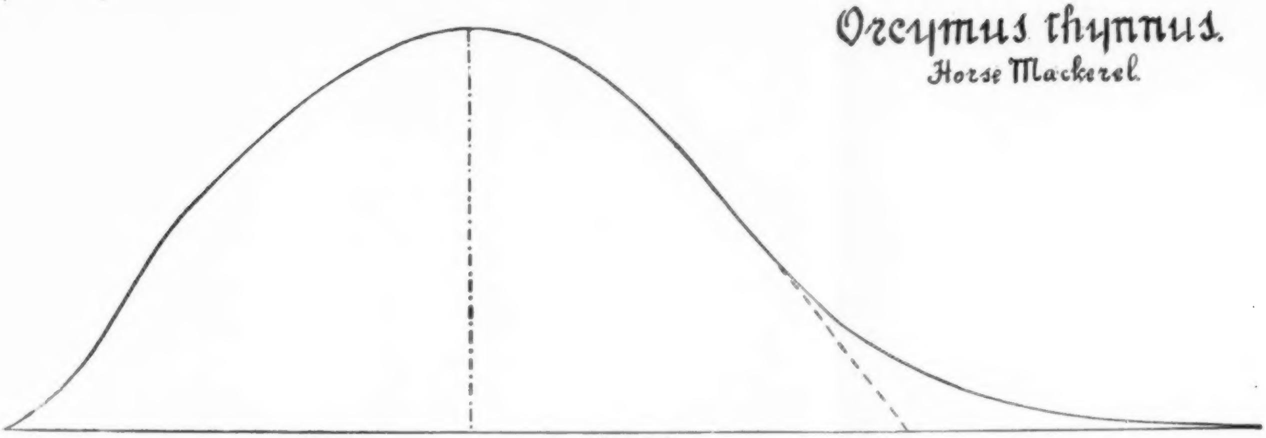


FIG. 293.

11

Coryphæna hippurus.

Dolphin.

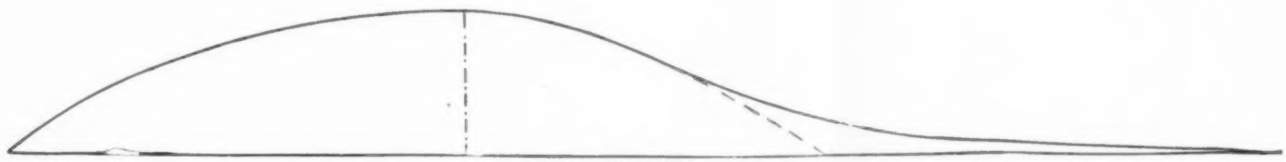


FIG. 294.

12.

Seriola dumerili.

Amber Jack.

FIG. 295.

13.

Tursiops tursio.

Bottle-nose Dolphin.

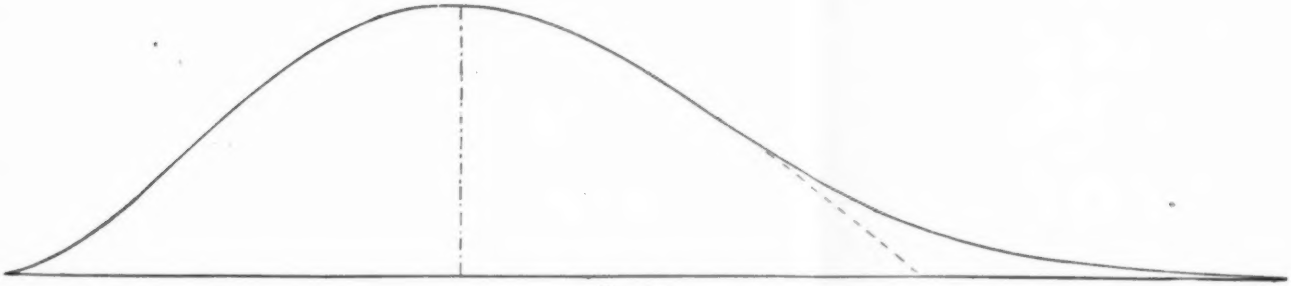
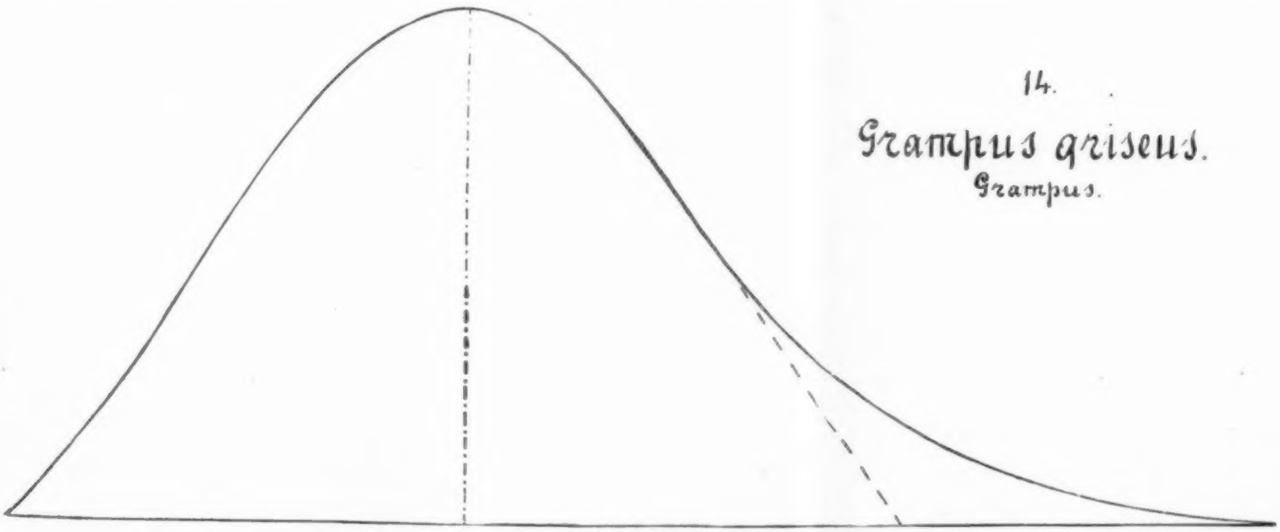


FIG. 296.



14.

Grampus griseus.

Grampus.

FIG. 297.

15

Phocæna lineata.

Striped Porpoise.

FIG. 298.

16.

Delphinapterus catodon

White Whale.

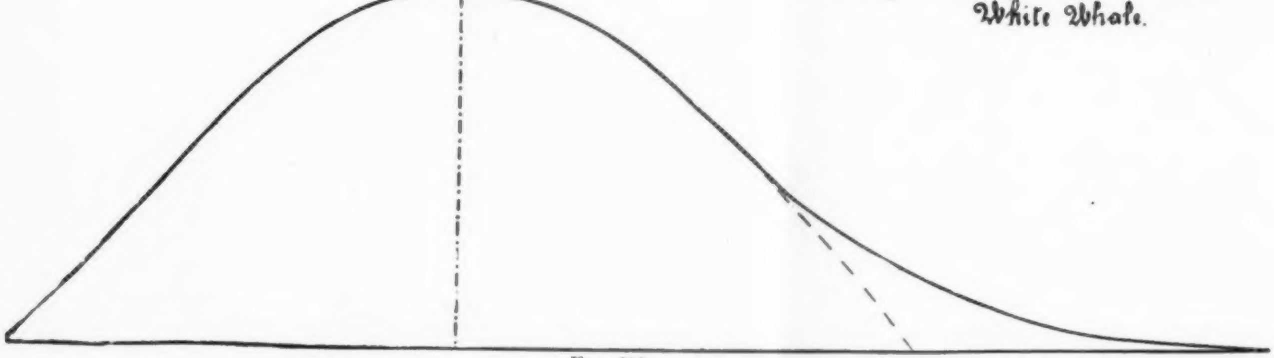


Fig. 299.

17.

Megaptera longimana.

Humpback Whale.

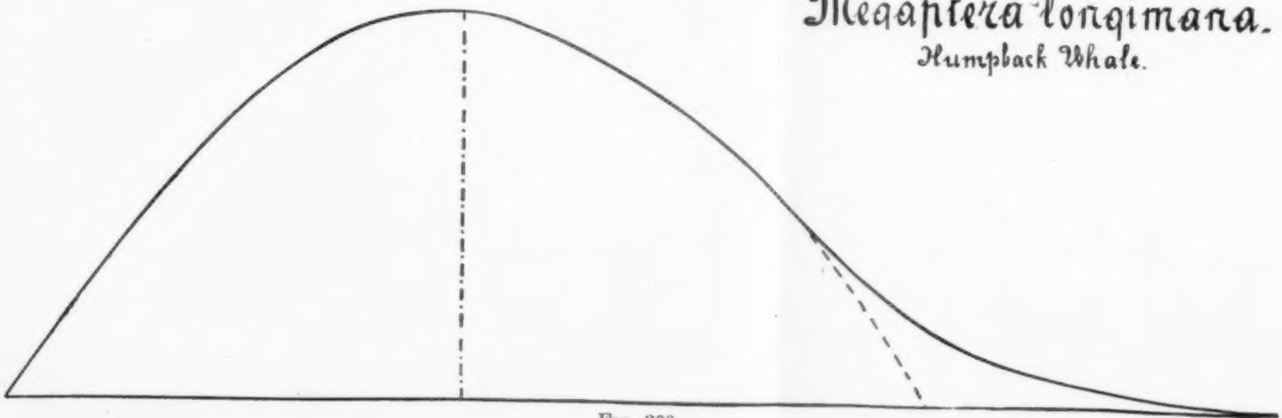


Fig. 300.

18.

Average Curve.

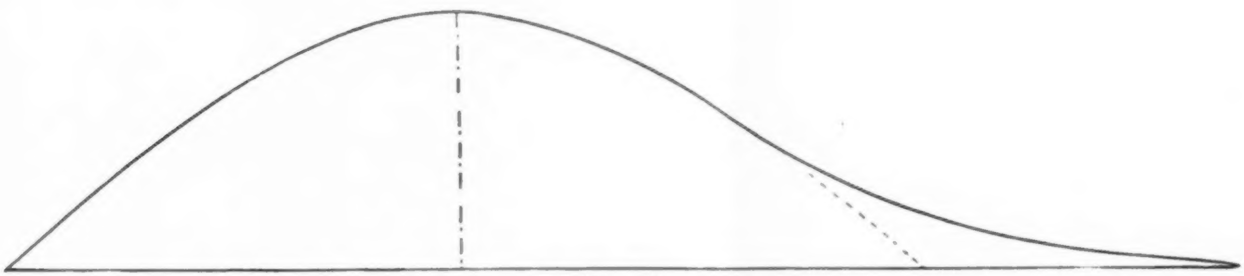


FIG. 301.

19

Entrance Curves.

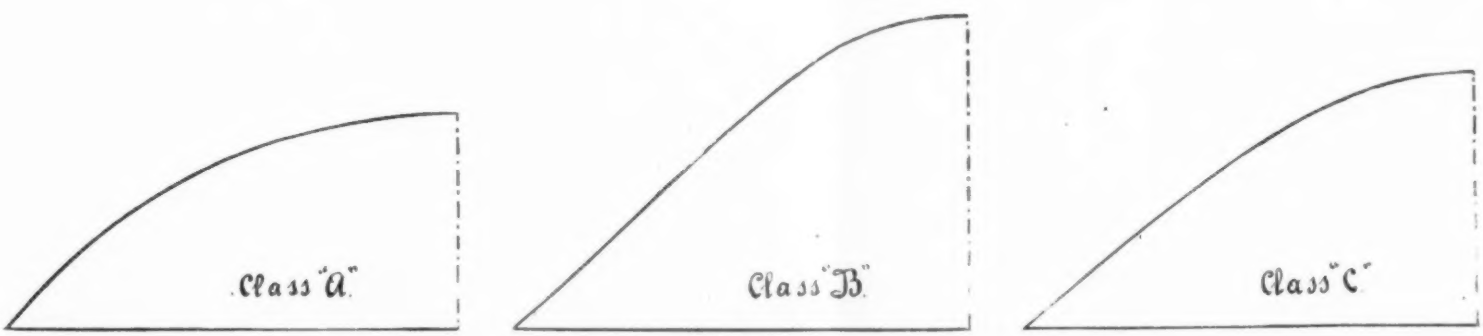


FIG. 302.

It surely cannot be denied that the "*fish form*" is the one which is best adapted for a body moving through water, when governed by the conditions which regulate the lives of those vertebrates classed in this paper under the general head of "*fish*."

While this form may not be the one of least resistance, it certainly approaches it as closely as the controlling influences will permit.

To recapitulate the points of comparison between the area curves of the fish mentioned, we have as follows: 1. That the area curves resemble each other in general form and in the distribution of the displacement. 2. That the position of the greatest area of cross section is *fixed*, being situated at 36 per cent. of the total length from the snout; the length is the distance from the tip of the snout to the tip of the caudal fin, measured on the longitudinal axis of the fish. 3. That this position of the greatest cross section is also true for the cetacea, and that it is entirely independent of size or of speed of locomotion. 4. That there is no relation between the areas of greatest cross section, or, apparently, between the areas and the speed of locomotion. 5. That there is no close agreement between the ratio of the displacements and the greatest areas of cross section, or between the cube roots of the displacements and the square roots of these areas. 6. That the mid-fish cylinder coefficients of fineness agree very closely, having as an average 0.476. 7. That the ratio of the distance from snout to vent to total length is very nearly constant, but that the constant is different for the fish and for the cetacea, being for the former 0.491, and for the latter 0.667.

If we take the areas of the greatest cross sections as unity, and give to every other area of cross section its relative value, and then find the average of the second means, we will have the ordinates for an *average or composite* curve. These averages are given in Table I., and the average curve plotted from them is shown in Fig. 301.

The author is fully aware that this average curve does not represent any one class of fish, and is, as it were, a melange of proportions of form which are more or less closely related to one another; but, although it may be open to criticism from this standpoint, it is, nevertheless, of marked value, and has a much greater and wider significance than may at first appear.

Since the position of the greatest area of cross section is fixed

and since the curves are similar in form, as well as in the distribution of the displacements, we are merely taking the average of corresponding areas, which we use as ordinates for a new or average curve, and by so doing we do not destroy any relation that they may bear one to another. Also, any errors in measurement will be reduced, and since the curves are similar in their general formation, the law represented by this average curve will be the one to which the progression of areas of each individual fish will approximate.

The equations of this average curve can be expressed algebraically, as follows:

Assume as the origin of the curves of both the "entrance" and of the "run," the point of intersection of the greatest cross section and the curves; and let

A denote the ordinate of this point, or the area of greatest cross section;
α " the ordinate of any point, or the area of any cross section;
l " the length of the entrance, or of the run; and
y " the distance from the greatest cross section to the section under consideration; that is, from "A" to "α."

Then, we will have the curves represented by the equations

$$(a) \ldots\ldots\ldots \alpha = A\left(1 - \frac{y^{1.75}}{l^{1.75}}\right),$$

for the entrance, and

$$(b) \ldots\ldots\ldots \alpha = A\left(1 - \frac{y^{1.45}}{l^{1.45}}\right)^{2.5},$$

for the run.

These equations are of the parabolic order; and would be those of the true parabola, if we substitute in equation (a) the exponent 2 in the place of 1 75; and in equation (b), a like exponent in the place of 1.45, changing at the same time 2.5 into 1.

The curve of the entrance, equation (a), is flatter than the true parabola; while the curve of the run (equation b) does not approach at all to the parabolic form, due to the fraction within the parenthesis being raised to the 2.5 power, causing the curve to be one of reverse flexure.

From equation (a), we derive the following proportion, namely:

$$(c) \ldots\ldots \alpha : \alpha_1, :: (l^{1.75} - y^{1.75}) : (l^{1.75} - y_1^{1.75}).$$

Therefore, for the entrance, any area of cross section is to any other area of cross section as the algebraic differences between the 1.75th powers of the length of entrance, and of the distance from the greatest cross section to the section considered.

Also, from equation (*b*), we have this proportion :

$$(d) \ldots\ldots \alpha : \alpha_1 :: (l^{1.45} - y^{1.45})^{2.5} : (l^{1.45} - y_1^{1.45})^{2.5};$$

that is, for the run, any area of cross section is to any other area, as the 2.5th power of the algebraic difference between the 1.45th powers of the length of run, and of the distance from the greatest cross section to the section considered.

Comparing, again, the different area curves, we notice that the curves of the after bodies bear a stronger resemblance to one another than do the curves of the fore bodies; and also that the latter curves are divided into three classes, one having a "*full*," another a "*hollow*," and the third a nearly "*straight*" line of entrance. The average curve is evidently of this third order. Now, the thought arises that perhaps there is some positive explanation for this difference, and that it is not entirely due, as we have up to this point assumed, to the differences in the physical condition of the specimens at the time of measuring.

Arranging the fish as mentioned above, we can classify them as follows :

CLASS A.

Full Entrance.

2. Salmo confinis (trout).
4. Pomatomus soltator (blue fish).
5. Pomatomus soltatrix (blue fish)
11. Coryphæna hippurus (dolphin).
12. Seriola dumerili (amber Jack).

CLASS B.

Hollow Entrance.

1. Salmo salar (salmon).
6. Labrax lineatus (striped bass).
7. Tetrapturus albidus (bill fish).
10. Orcymus thynnus (horse mackerel).
13. Tursiops tursio (bottle-nosed dolphin).
14. Grampus griseus (grampus).
15. Phocæna lineata (striped porpoise).
16. Delphinapterus catodon (white whale).

Class C.

Straight Entrance.

3. Gadus pollachius (pollack).
8. Scomberomorus maculatus (Spanish mackerel).
9. Scomberomorus cavalla (Cuban Spanish mackerel).
17. Megaptera longimana (hump-back whale).

It will be noticed that those fish which have a hollow entrance are the most numerous, and that Class B is nearly twice as large as either of the others.

We also find that, in general, Class A contains those fish which are noted for their strength, especially of the head and jaws, upon which they depend not only for their existence, but also for their defensive powers, and hence the fullness of the line of entrance in order to contain the necessary muscular development; that in Class B are grouped those which exhibit the greatest speed of locomotion, depending for the most part on this speed for their power of defense and offense; and, that in Class C are those which are midway between the other two classes, sacrificing in part their form for speed to gain an increase of strength or some other peculiarity. As examples of this theory, we may call attention to the Pomatomi, Figs. 287 and 288 in Class A; the Salmo, Fig. 284; the Tetrapturus, Fig. 290, and the Orcymus, Fig. 293, in Class B; and the Megaptera, Fig. 300, in Class C.

It is curious that the cetaceans are, with only one exception, included in Class B as having a "*hollow*" line of entrance, and that the exception, the Megaptera (whale), belongs to Class C, and is, therefore, only just without the limits of Class B. It seems remarkable that the whale should have so fine an entrance, when we consider the enormous proportions of its head. The age of this specimen is estimated at about nine months.

By a system of averaging the corresponding ordinates, as was done before, we can construct the curves, shown in Fig. 302, which will represent the mean curves of curves of entrance of the three classes respectively.

By making use of the same notation as was employed above, we can express these "Curves of Entrance" algebraically, as follows:

For class "A"—Full Entrance:

$$\alpha = A\left(1 - \frac{y^{2.1}}{l^{2.1}}\right).$$

For class "B"—Hollow Entrance:

$$a = A\left(1 - \frac{y^{1.05}}{l^{1.05}}\right)^{1.5}.$$

For class "C"—Straight Entrance:

$$a = A\left(1 - \frac{y^{1.6}}{l^{1.6}}\right).$$

The curve for class A is fuller than the parabola, while the curves of classes B and C fall below it.

For the fish mentioned in this article, the average scale areas of the greatest cross sections, that is, the values of "A" are, as below:

For class A,	*Area* = 1.68 square inches.
For class B,	*Area* = 2.47 square inches.
For class C,	*Area* = 1.97 square inches.

From these figures we see that the smaller the area of greatest cross section the more obtuse is the curve of entrance.

There are probably many individual cases which are exceptions to this, but it is presumably true in a general sense.

To those fish which are capable of high speed nature has apparently given a large area of section in order to obtain the necessary displacement, and at the same time has reduced the head resistance due to this area by a simple proportioning of the lines of entrance of the body. In order for a fish to pass through the water entirely without "shock," in would be necessary for the curve of area to be tangent to the horizontal axis at the snout. This condition is obviously impossible.

The form of fish is very prettily illustrated by the area curve, smooth and continuous from tip to tip, and supplies us with a living example of principles which have been more or less neglected by man when designing a body to be moved through the water.

In studying these area curves we cannot help noticing the great difference between the extremities, the curve at the snout meeting the horizontal axis at an angle varying from 12° to 50° with an average of about 40°, while at the caudal fin, the curve is tangent to the axis in every instance. It is, so to speak, an illustration, furnished us by nature of the greater importance of a clean, fine run than of a corresponding fineness of entrance.

Since fish are not wave-making bodies, due to the depth at which they swim, the water displaced by the snout tends to flow in a rapid stream toward the tail and thus refill the void which the body of the fish would create by its motion, thereby dispensing with a wave of replacement. The water displaced by the snout may not actually flow as described, but nevertheless the tendency for it to do so exists, and for that reason we may so consider it. Here, then, we find an explanation for the apparently extreme forward position of the greatest area of cross section. In the first place, the run, as shown on the area curves, includes the caudal fin, and secondly, since it is so much finer than the entrance it permits the velocity of the stream of displacement, which has a maximum at the greatest cross section. to be reduced before the water is acted upon by the caudal fin, thus increasing the efficiency of that important organ.

These *streams of displacement* take the place of the wave of that name, and their maximum velocity is evidently dependent upon the area of the greatest cross section. Now this maximum velocity, irrespective of its being high or low, is imparted to the water when the fish has traversed a distance equal to 36% of its total length. And, again, for fish of any length moving at any constant speed, equal divisions of fish are passed in equal periods of time; and, the water adjacent to the fish has imparted to it a velocity which starting from zero at the snout gradually increases until the maximum is reached, whence it slowly diminishes until it is zero at the posterior extremity, or rather should be if it were not for the currents set up by the propelling action of the tail. For fish traveling a distance equal to their length in a given period, the time required for these currents to reach a maximum is the same, although their dimensions, forms and displacements may be very dissimilar.

We have spoken of these currents as if they actually existed in a well-defined state, which is not probably the case. The water flows in from the sides as the fish moves forward, and the water displaced by the snout fills the space thus vacated. This produces a current which is in reality a modified form of the one described.

There is another curious feature connected with these area curves, which we may now consider.

If from the greatest area of cross section we prolong the curve of the run as if it were a continuous curve, instead of one of re-

verse flexure, we find that it will terminate at a distance from the snout of about 72% of the total length.

The actual distances for each fish, in percentages of their length, are given in column three of Table II. ; and on the diagrams, the dotted lines represent this curve, which we shall call the "*curve of replacement*," in contradistinction to the "*curve of displacement*."

Now it appears that the greatest cross section is situated at one-half the distance from the snout to the terminal point of this curve. This curve also includes an area between it and the line representing the greatest cross section, which is approximately equal to the area bounded by that section and the curve of the entrance.

For a division of the "*run*" of the fish, the author now suggests that this new curve of replacement shall be the boundary line between the body and the tail. That is, all of that portion of the fish from the head to this new curve shall be the "*body*;" and from this curve to the tip of the caudal fin, the "*tail*." It will be observed that with the cetaceans this curve terminates just behind the position of the vent.

If we imagine a fish with its tail removed, and propelled by some other means, we would have a form having its greatest cross section situated at the middle of its length, and with the displacements of the entrance and run approximately equal.

Most certainly this curve of replacement is theoretical, and is not seen on the fish-form, but nevertheless it is curious, and appears to the author to be of more than ordinary interest, and for that reason it is here mentioned for whatever merit that it may contain.

CCCVIII.

LARGE AND ENLARGED PHOTOGRAPHS AND BLUE PRINTS.

BY R. H. THURSTON, ITHACA, N. Y.

(Member of the Society.)

The writer has, on an earlier occasion, presented to the Society, with the kind permission of Professor E. C. Cleaves, samples of blue prints of extraordinary size, as made by a method, and with apparatus, original, it is believed, with that gentleman. That exhibited at the Washington meeting of this Society, and a similar one exhibited at the Kaaterskill meeting of the Civil Engineers, were 8 feet long and $3\frac{1}{2}$ feet in width. It will be remembered that they were made on a revolving cylinder, contact being secured simply by drawing the tracing tight over the sensitive paper and the underlying felt by means of suitably arranged clamps and springs. No glass was needed, and the expense and risk, and something of the trouble, of the common method of operation was thus avoided.*

The writer has since had some still larger prints prepared by a still simpler apparatus and method, original with himself, and by which an almost unlimited area of surface may be printed. The blue print accompanying this paper is a sample of what may be easily done in any drawing-room in which light can be obtained for such extent of print. It is a blue print from a tracing of one of Captain Zalinski's latest forms of pneumatic dynamite gun shells, as designed by him for the fifteen-inch gun now under construction. The print is fourteen feet long and $2\frac{1}{2}$ feet wide. It is a good sample of the fine work in printing and toning which is done by Professor Cleaves for the Sibley College of Cornell University. The formulas for the reagents are derived by a systematic course of experimental investigation directed to that end some time since.

The printing apparatus used for this later work consists of

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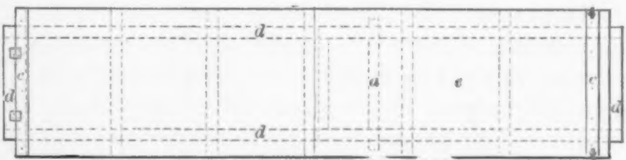
* Trans. A. S. M. E., Vol. VIII., p. 722.

nothing but a single thin board, of the length and breadth of the proposed print, with some margin for the stretching clamps. This board is covered with good felt of carefully selected quality, securely and smoothly fastened to the board by any convenient means. A line of tacks around the edges does as well, perhaps, as anything. The sensitive paper is then stretched over the felt, and the tracing drawn over that, and both are smoothly stretched by clamps or other convenient device. It is of course evident that it would not be practicable by this operation simply to obtain that complete contact and pressure throughout the surface in contact that is required for good work; but this desideratum is easily secured by the simplest expedient imaginable: The board is merely sprung to a flat arc in the direction either of its length or of its breadth, ordinarily in the longer line. This brings everything "taut," and the printing is done precisely as under glass, with the further decided advantage that no light is lost through the intervention of the glass; which, however excellent in quality, will inevitably absorb a very measurable amount.

The accompanying sketch represents the apparatus designed by Professor Cleaves for the purpose of carrying into effect the suggestion of the writer in the making of the very large prints of which that presented herewith is a sample. Figures 303 and 304 are a plan and side elevation of the board and its mounting. It will be seen that the upper, or printing board, is supported upon a lower and somewhat narrower and longer one, which, in turn, should be carried on trestles or other convenient arrangement. The printing board is stiffened laterally by strips or battens, but is free to spring longitudinally to any desired extent. The supporting board is stiffened longitudinally. At each end of the latter is a batten *d d*, set transversely, which answers as a chock, as seen later. Clamps *c c* are placed at either end of the printing board by means of which to secure the felt, paper, and tracing. One or both of these clamps may be made adjustable for varying lengths of print. Figures 305 and 306 show the method of operation. The board is first raised at one end and thus slightly sprung. The felt and paper, and the tracing which forms the negative, are stretched smoothly between the clamps and well secured. The board is next laid down on the supporting base, the two ends made fast, the one to the batten at the left, the other to another arranged at the proper distance from the right hand extremity, the printing board springing into a curve of which the bridge *a*, hinged at the

middle as in *A*, and thrown up as seen in the sketch *B*, gives the versed sine. By springing to any desirable extent as in *A*, and then reversing the curve as in *B*, any required degree of tension and stretch can be given the tracing, and thus any necessary amount of pressure and perfection of contact with the sensitive paper may be obtained. The two sections at the right exhibit the end view of the pair of boards and a section of the clamp used.

It is obvious that this scheme will suffice to print any area of blue-print that paper can be obtained to cover—a half-mile square if necessary. The amount of springing required is very small, and



PRINTING BOARD FOR LARGE BLUE PRINTS.
E. C. CLEAVES, PROF. OF DRAWING, Sibley College, Cornell Univ. Ithaca N. Y.

FIG. 303.

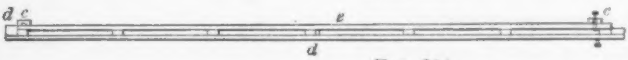
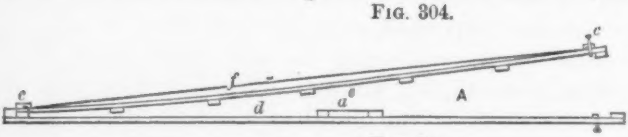


FIG. 304.



Section of Clamp

FIG. 305.

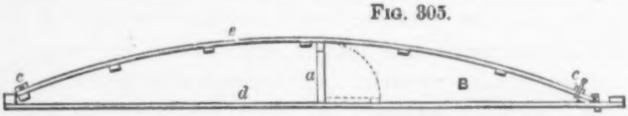


FIG. 306.

never enough to affect perceptibly the uniformity of the printing and tone of the print. If it should ever seem too great, it is easy to correct the defect by first springing the board in the reverse direction; then, after drawing the covering felt and papers tight, bending it in the first proposed direction, past the straight line, and as far as may be found desirable to secure good contact. We have found this method to work quite as well as the cylinder; which we still use, however, for what we now call *small* sizes. Perhaps this may be as well or better than the new method, up to eight or ten feet length, and for narrow prints; while the new arrangement may do best whenever extraordinary sizes—as gauged by our new standard—are called for.

We are doing another quite different kind of work in photog-

raphy, which proves to have great value for certain purposes. Professor S. B. Newberry, in charge of organic and applied chemistry, has, for some time past, been experimenting with various methods of enlarging photographs, and has succeeded in producing such enlargements very successfully in sizes which are remarkably well adapted to the purposes of the instructor in making wall-charts, maps, drawings of architectural subjects, and of machinery. I have now received from him excellent reproductions, on a scale of about $2\frac{1}{2}$ by 3 feet, and 3 by 4 feet, of full-page plates from the engineering journals; portraits from cabinet pictures of great engineers and inventors; and other interesting subjects, such as could not have been economically obtained by either redrawing, or any of the older processes of reproduction, and at a cost so small as to make them very freely available. They are clear, distinct in line and in all details, and are quite large enough and bright enough to serve admirably for wall-charts. The effect is particularly excellent in making views of architectural subjects and of machinery which is at all complicated, such, for example, as full drawings of marine steam engines.

DISCUSSION.

Mr. Albert F. Hall.—The apparatus described by Prof. Thurston, although showing much ingenuity, appears to possess the same imperfections common to most of the devices for the purpose. It is impossible, where the apparatus is in constant use, to prevent the board from warping, or to maintain a uniform thickness of the "*pad.*" Consequently, the prints soon show marks of imperfect contact. One way to obviate this has been to make the *pad* in the form of a rubber bag and fill it with air.

Another is to secure a piece of sheet rubber to the edges of a board and to maintain a constant flow of air into the space between, to make up for leaks resulting from imperfect joints; a safety valve being placed on the pipe leading to this space, for the purpose of guarding against too great a pressure on the glass.

It is not, however, always convenient to arrange such a device, and a novel apparatus, the invention of Hugo Sack, of Plagwitz-Leipzig, Germany, United States Patent No. 324,960, of August 25, 1885, seems to present a means of always obtaining a perfect blue print. The principle of the device is very simple, and consists in exhausting the air from the space between the back of the prepared paper and an elastic material, the edges of which are

held in close contact with the glass, by means of a small vacuum pump, attached to the frame, the atmosphere producing the desired pressure.

Unfortunately these frames cannot be seen in this country, and the price, which does not include the glass, freight, or duty, renders them too expensive for general use.*

Mr. Ezra Fawcett.—At the presentation of the large blue print at our Washington meeting, by Prof. Thurston, and the method of producing it, by securing the sensitive paper and tracing on a large drum or cylinder, and exposing to the light, it suggested itself to me that it would have to be turned regularly to produce an even print, and also be somewhat limited.

At that time I suggested to one of the members the plan of a long thin board; clamp the paper to it and spring to a flat arc, giving it a practical uniform exposure to the light, and limited only to the length and breadth of the device and paper obtainable; and it gives me great pleasure to note the able manner in which Prof. Thurston has brought out the device.

Mr. W. S. Rogers.—I am interested in the blue print question and always have been. I saw a description of that cylinder method, described at the Washington meeting, and I was going to try that to see how it worked in making a large print. But it was too big a job and I was tired. (I was born that way.) I had occasion, though, to make a print about twelve feet long. I made a cylinder three feet long and six inches diameter, hung it on bearings that rested on a trestle arranged to represent a horse, and then I wrapped padding around that and wrapped thick manila paper

* For the benefit of those who may desire further information, they are referred to the manufacturers, Carl Schleicher & Schüll, of Düren, Germany, or their agent, B. G. Soltmann, New York.

A list of the sizes is appended.

No.	Size of Tracing.		Size of Glass.		Thickness of Glass.	
	Millimeters.	Inches.	Millimeters.	Inches.	Millimeters.	Inches.
I.......	360 × 420	14.17 × 16.53	395 × 455	15.55 × 17.91	2 to 5	0.08 to 0.2
II.......	505 × 650	19.88 × 25.59	540 × 685	21.26 × 26.97	2 to 5	0.08 to 0.2
III......	550 × 750	21.65 × 29.53	585 × 785	23.03 × 30.90	2 to 5	0.08 to 0.2
IV......	750 × 1,050	29.53 × 39.56	790 × 1,090	31.10 × 42.91	2 to 5	0.08 to 0.2
V......	850 × 1,250	33.46 × 49.21	990 × 1,290	38.98 × 50.78	5 to 8	0.2 to 0.4
VI......	1,050 × 1,400	39.56 × 40.94	1,090 × 1,440	42.91 × 56.68	5 to 8	0.2 to 0.4

over that. Next to that I put sensitized paper, then the tracing cloth, which I rolled up very tight on the cylinder, and I marked it so that I could give it about three minutes' exposure as I unrolled it. I made the drawing room dark. Then raised one window, so as to let the sun come in from the south and strike it where it was tight on the roll. I slowly unrolled the tracing cloth and two pieces of paper and I got a very nice print. I made only one and that was so well done that I was afraid to try another. But I like that better than I do this board method. My drawing room is only sixteen feet square. I have not any room to put any extra lumber in. Now I propose to make two rolls and set the lower roll at an angle of about 45 degrees downward, and reroll the paper as I print it, and I will use a mechanism to let it print itself and roll out on the other roll and let it have a regular three minutes' exposure. I think then that I can print ten miles of paper in my sixteen foot drawing room. I am going to try that. I have tried it already on one roll and it works very nicely. If I had known of this paper I should have brought that print along. But at the next meeting I will try and bring the whole machine.

Mr. John F. Wilcox.—I might save Mr. Rogers in this respect by telling him to write to Mr. Ely up at Altoona. He will send him a blue print of the apparatus that is used up there. They are similar to the one described at the Washington meeting with the addition of the cylinder you speak of there—the winding up of the print. I have a print in the office at home that is fully seventy-five or a hundred feet long. It is a map of a line of pipe. It is on two rollers. I am speaking at random of the length of it. It may be longer.

Mr. Rogers.—I am very much obliged. On this blue print question there is another point that I wish to speak of. I notice that in nearly all the blue prints that I get—we get a great many from firms to know whether we can adapt our machines to their class of work—that the upper lines, which I generally call the North line and the West line, are very light lines and the East line and South line are heavy. That is according to the way I was taught at school. But I have abandoned that in practice, and I make all my lines representing a piece that is to be worked of the same thickness and I make them heavy on the tracing; then when it comes to blue printing they come out heavy. I have found, from watching men in the shop picking up a drawing made with shaded lines,

that they will go at it and turn it two or three different ways, and I always felt as though I ought to have written on the drawing what it was, and I find that a great many shops I go into are using that school method. I thought it was original with me, but I find a great many others are doing it. It is not exactly what we were taught, but it is a great deal more convenient for the men that have to use it, if all the lines of the sheets to be operated upon are the same thickness and heavy and the dimension lines are light.

*Prof. R. H. Thurston.**—I think that Mr. Hall is mistaken in asserting that the printing board cannot be kept from warping, if it is properly made and handled. A few battens secured transversely on the underside will always prevent lateral warping even if it were probable that it could occur to an embarrassing extent, which I think quite unlikely. We have had and we anticipate no trouble at all of the kinds suggested by him. I presume his misapprehension and his apprehensions arise from an unscientific use of the imagination rather than from experience with the arrangement described. Its object is to evade precisely the objections to the common method which he enumerates and it is practically successful. It also eliminates the objections to the German device which he apparently approves—the great cost, risk and trouble met with in the latter endeavor to meet difficulties which must always arise where great sheets of glass are used. It would be interesting to see the Sack system applied to making blue prints many feet long. I should not expect it to prove a long-lived system in competition with the simpler and cheaper and quite as effective methods which I have described and of which the Society has been shown the product.

The modified plans described by Mr. Rogers and by Mr. Wilcox would, I should expect, be very useful for the special cases referred to by them. I think that, now that two or three of us have played Columbus, this egg can be made to stand on end almost anywhere.

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* Author's closure, under the rules.

CCCIX.

TOPICAL DISCUSSIONS AND INTERCHANGE OF DATA.

XVIITH MEETING, NASHVILLE, MAY, 1888.

No. 309-58 and 309-59.

What is the most economical speed of cable in telo-dynamic transmissions for high and low power?

What data have you for design of hemp rope transmissions, especially where several parallel ropes replace a flat belt?

Mr. John H. Cooper.—The ropes should be run under the same conditions and very much in the same way as leather and other belts.

That is, the working tension of each rope should not reach and should never exceed its elastic limit, and this restriction will avoid the evil effects of overstraining and insure long life to the ropes.

The pulleys should be comparatively large and each connected pair set apart as nearly horizontally as may be, which will secure the best of the "sag" of the ropes upon both pulleys, and that the speed should be high in order that the traction may be light. The upper strips of the ropes should hang in the deep curves between the pulleys, the lower strip should drive, and neither strip should be vertical.

The wheels are usually of cast iron, made whole, in halves, or in segments with separate rim, arms and hubs as may best suit the making, fitting and shipping of them. The grooves should be accurately turned to the same shape, to suit the ropes to be used and to exactly the same diameter in the same wheel, in order that the pitch or driving diameter of each groove shall be equal, it being essential also that the effective girth of each rope in each connected pair of wheels shall be the same and that the wheels shall be well and evenly balanced for high speed running without lateral vibration.

The least diameter of rope wheels should bear about the same proportion to the diameter of the rope to be used upon it, as Mr.

Roebling gives for wire rope sheaves and that is 35 times the diameter of the rope.

As with belts, the greater the velocity of the rope the greater the efficiency of the same, and the speed may safely be increased up to 5,000 feet per minute. The driving and driven shafts may be located according to circumstances, a distance apart of from 20 to 60 feet.

Rope wheels intended for bearing up the sag of ropes, or guiding them where necessary, should have a diameter not less than that prescribed for the least driving wheels, and the grooves should be smooth and have semicircular bottoms, larger than the ropes to permit the freest passage of the ropes through them.

White untarred ropes will break under the strain of 7,000 to 12,000 pounds per square inch of section. The actual section may be be taken as that within $\frac{9}{10}$ of the circumscribing circle of the rope.

As to the durability of the ropes, some are known to have continuously run $10\frac{1}{2}$ years; in general the life of a rope is from 3 to 5 years, although the latter figure has many times been exceeded.

When a rope shows symptoms of giving away, it can be removed and a new one put in its place during the noon stoppage or in the early evening.

It is usual in the allotment of driving capacity to provide one or more ropes than the exact number necessary, so that should one fail the remaining ropes are fully competent to continue driving the plant.

The ropes used are mostly of hemp of carefully selected stock, having long fibres; they should be well and evenly twisted and uniformly "laid" and yet be soft and elastic.

It is very important that the splices should be well made, no part being of greater diameter than the body of the rope, and to do this the splices should be 9 to 10 feet long.

The driving power is due to the grip of the ropes in the angular grooves of the wheels, the ropes can therefore be run loosely, which avoids unnecessary transverse straining of the shaft and useless pressure on the bearings.

From an article on Rope Gearing by Mr. James Divine read at a meeting of the Institute of M. E., Manchester, England, Oct. 25, 1876,* a few extracts are made in reference to the comparative cost of the transmission of power by cog-gearing, by belting and by ropes. Mr. Divine says: "It is difficult to give exact figures

* See also "Use of belting" by the writer.

for this comparison, but the cost of rope-gearing is considerably less than cog-gearing for equal transmissions and it may be fairly set down as one-half or two-thirds the cost of leather belts on flat faced pulleys.

Lockwood, Greene & Co., of Newburyport, Mass., have given figures on the comparative cost of rope and belt driving, citing a case of their own planning and erection in Lawrence, Mass., where they have about 2,000 H. P. in ropes. The round figures given are $10,800 for the belting-gearing and $6,700 for the rope-gearing.

We have yet to refer to the two most important points in the successful practice of transmitting power by ropes, *the safe and continuous working tension and the shape of the rope grooves in the wheels.*

The following table will show the transmitting power of ropes at velocities ranging from 3,000 to 5,000 feet per minute, as derived from cases in practice, from which the H. P. can easily be figured when the exact velocity in feet per minute of the ropes is known.

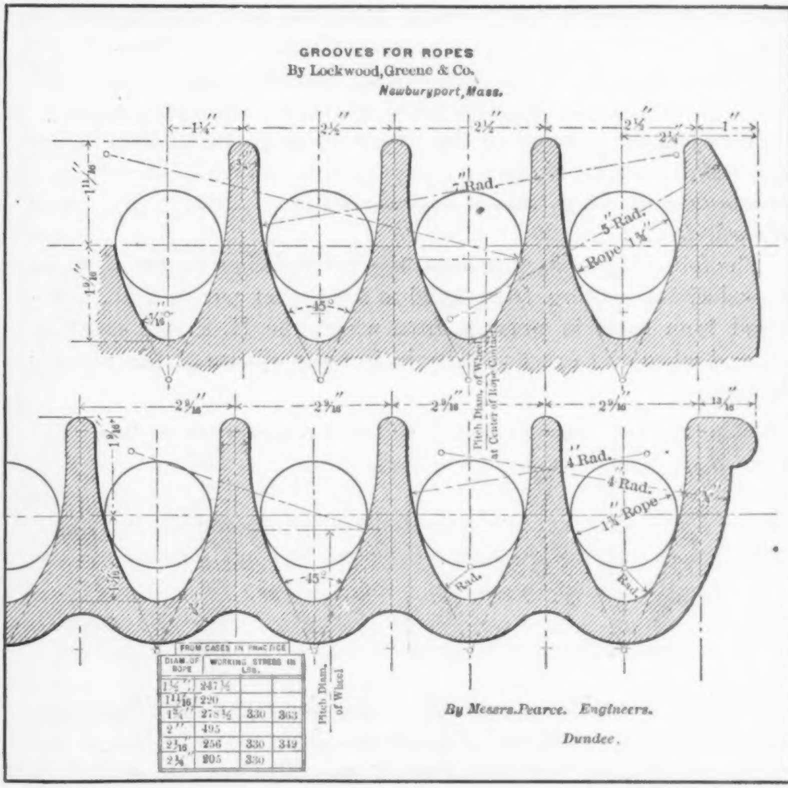
Diameter of Ropes. Inches.	Working Stress on One Rope. Pounds.
$1\frac{1}{2}$	$247\frac{1}{2}$
$1\frac{11}{16}$	220
$1\frac{3}{4}$	$278\frac{1}{2}$
$1\frac{3}{4}$	330
$1\frac{3}{4}$	363
$2\frac{1}{16}$	256
$2\frac{1}{16}$	330
$2\frac{1}{16}$	349
$2\frac{1}{4}$	205
$2\frac{1}{4}$	330

Messrs. Pearce Bros., engineers of Dundee says: Mr. Divine was our Manchester agent, and the facts so nicely given by him in the paper referred to were furnished by us. This and the arti le on rope-gearing in "Elements of Machine Design" by Unwin, for which we also furnished the data, constitute, we believe, almost the only printed information we have in English on the subject.

I present a drawing (Fig. 353) of the rope grooves from Unwin, and also give a copy of that kindly furnished to me by the Messrs. Lockwood, Greene & Co., adding their remarks: "In regard to the shape of the grooves, we have, after some investigation, adopted that shape which has been found to be best adapted to the general condition of rope driving abroad, and we have been satisfied with it, so far."

There is a difference of opinion as to the proper angle of the V

grooves, 40°, 43° and 45° being named for the inclination of the sides. It is certain that the inner faces of the inclined sides of the grooves where the ropes touch, should be concave to give the greatest area of contact with the rope when pressed into the groove.



DIAM. OF ROPE	WORKING STRESS IN LBS.		
1½"	247½		
1$\frac{11}{16}$"	220		
1¾"	278½	330	363
2"	495		
2$\frac{1}{16}$"	256	330	349
2⅛"	205	330	

Fig. 353.

M. Reuleaux quotes Wedding, a machine manufacturer in Berlin, as having found by experiment, that the resistance of a rope to sliding in a V-grooved wheel of 60° angle, is double the resistance that the same rope meets when in a semicircular groove which it just freely fits.

Notice must be taken of the fact which is graphically represented on the drawing, that the pitch diameter of the wheels from which relative speeds are to be calculated, must not be measured from the centers of the ropes, but from the centers of the rope contacts

in the grooves, or from some point between the two depending upon the amount of stretch of the rope at its center and upon the slip of the touching part.

Mr. W. W. Dingee.—The J. I. Case T. M. Co., Racine, Wis., are using in their new plant a hemp rope transmission of about 30 H. P. The rope is $\frac{3}{4}$ inch diameter and passes around the pulleys six times. The rim of these pulleys is made of hard wood with six V-shaped grooves for reception of the rope, which is returned from the last groove to the first by the use of two idlers as shown in accompanying diagram (Fig. 354). The frame in which these

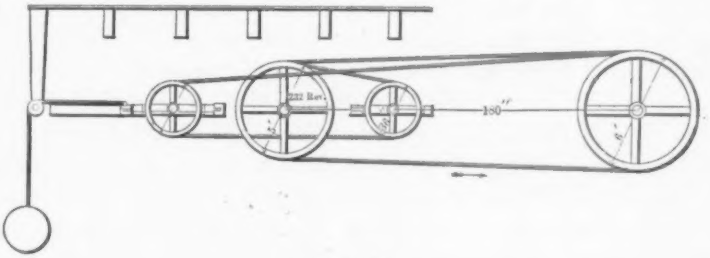


FIG. 354.

run is supported on trunnions which allow these idler pulleys to take the necessary angle to get the rope back again past the drive pulley.

One of these idlers, with its frame, is free to move on an overhead way, and by means of a weight keeps any desired tension on the driving rope.

This rope transmission of power has been in use sixty days and promises to be in every way satisfactory.

Mr. H. H. Suplee.—I have a few facts relating to this rope transmission very similar to the last one given by Mr. Dingee, which came under my own observation about two or three years ago, and on which I had occasion to make a test. In the case to which I refer there were two pulleys of equal diameter, 30 inches diameter, situated not over 12 or 16 feet apart, horizontally, and there was a half-inch manilla rope passed around five times, the one rope continuously passed around again and again; precisely similar to that shown in Fig. 354, except that the idler there shown between the pulleys was omitted in this case. This idler was tilted at an angle sufficient to make its upper edge come in line with the one side of the multiple coil of rope and the other edge come in line with the

other side. The tilting of the idler pulley provided for the difference in the position of the ropes. In this case the rope was one-half inch in diameter. In order to test it I put the indicator on the engine, and then, leaving all the other attachments the same, we proceeded to apply a brake, first taking the indicator card of the engine; the apparatus running with the ordinary load on. Then we continued to add load by putting a brake on the pulley on the driven shaft and continued to add resistance until the engine showed signs of distress, when a second card was taken. The difference between the cards showed 19 horse power. The rope was running at a speed of nearly 1,300 lineal feet per minute. In this case the grooves were about one-third of a semi-circle of much larger radius than the rope, so that there was no wedging action whatever, and the number of coils had such a grip that there was no chance of slippage. In this transmission, with which I have had some experience, in every case it is easy to obtain the required grip by taking a sufficient number of coils, so that the question of slippage is entirely eliminated and the only thing to be attended to is the ultimate strength of the rope. A similar arrangement has been placed in the Studebacker Wagon Repository in Chicago, for a vertical transmission where they transmit over 80 horse power vertically, with two strands of $\frac{5}{8}$ rope from the ground to the top of the building. I neglected to say that these were wooden pulleys, not iron pulleys.

Mr. H. P. Minot.—I have had a little experience in the rope business. I think Mr. Cooper has got the groove right with the exception that I would not have made it in the shape of a parabola, as near as he has got it. I would take it straight from the top down. If you allow the rope to touch the bottom of the V it will slip. I do not approve of running two ropes, or three or four or five; I would not run but one. You all know it will not do to let a rope touch anything. The less obstructions you can have there the better. If you allow the two strands to touch one another they will soon become frayed and wear themselves out. You want to let it run just as freely as possible. My experience is that where you run several strands you cannot get them all to draw alike. I never have had good luck with iron wheels. I think wood is the best. An iron wheel will get smooth, so that the rope does not seem to drive well. The last one I put up had, I think, a 40-inch driver and the driven was 50 inches, and it worked very nicely. The only difficulty we have had is that the man who made

the splice simply tied a knot about the size of your arm. I do not think that is a good way to make a splice. I have one that has been running now for a year and a half. It has never given the least particle of trouble; but a great many, if they put sheaves or carrier pulleys underneath as we have, to get the rope around where you want it, they make them too heavy. I put wrought iron arms in my sheaves to make them light, and they should be smoothed out so that there are no rough projections to fray the rope out. But this side elevation at the bottom here I do not believe is a good way. No matter how I was going to run my rope, I would have the takeup move up and down. I have seen a good many rigged with a spring. A good many allow the sheave to turn or swing around and adapt itself to the rope. I do not think that that is a good plan. The rope will flop off. But by all means do not run the ropes together or allow them to touch each other.

The President.—I think Mr. Suplee referred to where the pulleys were so fixed that there was no possibility of different sections of the rope coming in contact with one another. I think also the heaviest rope driving apparatus we have in this country is at Wilkesbarre, where they use two-inch wire rope on a twenty foot drum.

Mr. Suplee.—I wish to state in this particular case the experiment was first made with V grooves and also with grooves somewhat similar to this one in the illustration, but the difficulty lay in the fact that the rope tended to wedge in the grooves and carry around with the pulley farther than it should. It came out with a quivering motion; every time you could see the ropes carrying around more than the semi-circumference and then jumping suddenly out of the grooves. But when the shallow grooves were substituted and enough coils were made to give sufficient grip, it ran very much more easily, and there was much less wear on the rope. This pulling it out from the wedged condition in between the grooves rapidly wore the rope out. I might also mention that in this case the splice was made of rawhide; a short splice in the rope and then a splice of rawhide. The central thong was lapped around the splice of the rope and then the thongs that were left were worked one after another into the strands of the rope and the splice lasted as long as the rest of the rope.

Mr. Minot.—That would depend on the angle of the groove. If you should put it at 3° or 4° or 5° we should expect the rope to draw into it too tight.

Mr. Suplee.—This angle was 43° in the first place. That is what is recommended by the English practice.

Mr. O. A. Lanphear.—In our place we have a rope transmission, four ropes running parallel. We use ⅝″ rope, running at a speed of 4,600 feet per minute. We have lately changed the pulleys. It was running at a speed of 3,450 feet per minute, and we found the rope wore out very rapidly and did not give general satisfaction.

There is one thing I wish to remark about splicing. Almost every one advises a long splice. That is about the first thing they start out on—advising you to "use a long splice in your rope." They tell you it wants to be from six to eight feet long, and longer in some cases. We have tried the long splice and also have had experts come there and say nothing else would do, and we have never had a single one hold any length of time. But on the contrary, short splices about 18 or 20 inches long hold every time, and they are what ought to be used, in our judgment.

Mr. Minot.—So far as I know, in Columbus, the grand difficulty with rope transmission has been on account of the splicing, and I am of the opinion that it is because they are improperly made. Now, when I was in good old Massachusetts I used to hear some of the sailors say that they could splice a rope so that it was exactly as good in the place where it was spliced as in any other. If so, the rope is as liable to give out anywhere as in the splice. Therefore, I attribute the giving out of the ropes there to the bad splicing. That has been my experience with the rope. I think Mr. Lanphear has already had some difficulty with his ropes in regard to the splicing. He knows about it better than men about my place. An old sailor will tell you a long splice is a proper splice to make for a rope to hold and to run through a tackle block on ship-board. The splice should not be larger than at any other point. I suppose it is possible to make a splice so that it is almost if not quite as good as at any other place.

Mr. Geo. M. Bond.—I would like to ask if there is not an error in the second column. Should it not read 295 working stress of pounds on a two-inch rope, instead of 495? It is so large as compared with the two-and-a-half-inch rope.

Mr. John H. Cooper.—I can only reply that this figure "495" is deduced from the data found in one of several articles on rope gearing which I have collected, which reads thus:—"It is given by reliable makers as for 2″ rope, 15 horse power per 1,000 feet sur-

face velocity per minute." Hence: $\frac{33,000 \times 15}{1,000} = 495$. There may be an error in the original which I cannot now reach. I should, however, recommend the avoidance of it as either an error or an extreme case.

Mr. W. R. Warner.—It has occurred to some of the members here, that in making the long splice one strand is cut out and another one put in, while in making a short splice each strand is braided into the next one. In the former case the rope might be weaker than when not spliced at all, while in the latter the spliced portion is the strongest part.

I wish Mr. Lanphear would give us some information on that point.

Mr. Lanphear.—I believe the rope is cut away more in making the long splice than it is in making the short splice. There is some of the rope cut away in making the short splice, but it leaves the rope a little larger, but not very much. After it has run a little bit you can better distinguish the difference in size as the cable is passing from one pulley to another. In the long splice the trouble is that the long ends whip out, and when the ends once start to whip out it will ravel a whole splice out. But in a short splice the whole of the rope is interlaced in such a manner that if an end whips out it does not ravel out as it is still locked, and consequently the splice lasts longer.

A company * which has put up a large number of rope transmissions has always recommended the short splice. This they have always used with good success, but the long splice has not given satisfaction. Now this company recommends a new splice which they have recently invented.

Mr. H. de B. Parsons.—I should like to say that in the making of a splice by sailors there is no cutting out of the rope at all; that is in making a long splice the strands are untwisted and the two parts brought together, at the same time a strand from one end is wrapped in the groove left by unwrapping a strand from the other end, and that is continued for half the length of the splice. Then another strand is unwrapped, and a second one wrapped in its place; and at the end of each operation, the ends are entwined over and under the strands in the rope, and worked in with the aid of a marlin-spike. In a short splice the same operation takes place as regards the entwining of the ends; but there is no unwrapping

* Dodge Mfg. Co., Mishawaka, Ind.

and rewrapping of the strands. In other words the long splice properly made is not weakened by virtue of losing any of its parts. Where one part is removed, another part of the same size fills its place. The short splice is more clumsy in appearance, because there are more parts entwined together at one point. A hand-made splice cannot be quite so tight and smooth as the rope was originally, but by use it soon pulls itself into place. The actual place where the two ropes are joined, you cannot find if it is properly made. I myself made a splice of six feet in a bass fishing line, and the line finally broke at a place not in the splice. The splice you could not see except at one end where it was a little rough. The long splice is stronger than the short one, since it affords greater length to hold by friction; and being smoother than the short splice, it should pass the grooves in the pulleys.

Mr. Minot.—I believe I know the principle of splicing rope, but I am not accustomed to handling it. A long splice on a rope, say three-quarters of an inch in diameter, should be about six feet. Of course there are six strands—three on the right hand end and three on the left. You put your rope together and unwind one strand clear down for three feet. You take one strand from the other end and lay it right in, leaving an end say ten inches long. You unwind a second strand and lay the next one in. Then you take another one out and wind it along. A sailor would then take his knife and take the threads of the rope from the under side and draw each one down separately, and when done it is almost impossible to find where the splice is. I have heard men offer to bet any amount of money that they could splice a rope so you could not tell where it was. You have got precisely the same material in one part of the rope as in another, except where you have these ends. I do not believe there is any use talking about the short splice having the durability of the long one, because you have not got length enough to hold the strands in place.

No. 309-60.

How do the recent results in fuel economy with high speed engines, compare with the results of slower speed engines with releasing gear—workmanship and design supposed equally good for both cases?

Mr. Geo. H. Barrus.—I suppose one object of this question is to learn the effect which an increase of speed has upon economy,

rather than the determination of the economy existing between high speed engines and slow speed engines, as ordinarily used.

Most of the high speed engines are of relatively small size. They are seldom over 100 H. P. capacity. They are usually fitted with one valve and with a shaft governor which secures regulation by changing the point of cut-off, and this necessitates the introduction of conditions in the working of the steam, which are unfavorable to economy. Besides they have as a rule an excessive clearance. The slow speed engines usually have four valves, and they work under the best conditions for securing economical results. The comparison of the high speed with the slow speed engine, as commonly built, therefore, determines the effect produced by changes in other conditions than the single one of speed.

A comparison of this kind to be fair, should be between engines of about the same rated power. There are plenty of data in regard to the performance of slow speed engines of large size, but not so much which applies to engines of less than 100 H. P.

I have a few cases of recent date which bear on the question, and which, so far as they go, may be of interest. The first is that of a high speed single valve engine of 40 rated horse power capacity. The diameter of the cylinder was 8 in., stroke 12 in., clearance 16%, boiler pressure 82 lbs. With a mean effective pressure of 43 lbs. per sq. in., the consumption of feed water was 30.6 lbs. per I. H. P. per hour, and the proportion of feed water accounted for by the diagram was 0.76. The cut-off was 38%, and the average power developed 39.4 H. P. When the load was reduced to 32 H. P., the consumption of feed water was increased to 31.5 lbs., 0 68 of which was accounted for by the indicator. The speed of this engine was 300 rev. per min., and the steam was practically dry. The valve was a slide valve, having a balancing plate on the back.

A second test was made on a similar engine rated at 100 H. P., but in this case the engine had a piston valve. The diameter of the cylinder was $14\frac{1}{2}$ in., length of stroke 13 in., clearance 12%. The engine ran at a speed of 250 rev. per min. When the load was 81.7 H. P., corresponding to a mean effective pressure of 23.4 lbs. per sq. in., the consumption of feed water was 32.7 lbs. per I. H. P per hour, and of this quantity 66% was accounted for by the indicator. When the load was 50 H. P., which corresponds to a mean effective pressure of 18.3 lbs. per sq. in., the consumption of feed water was 34.1 lbs. per I. H. P. per hour, and of this quantity 60% was accounted for by the indicator.

The third case was that of a much smaller engine than either of these. The cylinder was $4\frac{1}{2}$ in. diameter, 5 in. stroke, and worked at a speed of 300 rev. per min. The engine had a piston valve controlled by an automatic shaft governor. The steam was generated in a boiler having a considerable amount of steam-heating surface. With a boiler pressure of 124 lbs., and a load of 6.1 H. P., corresponding to 50.7 lbs. mean effective pressure, the consumption of feed water was 41 lbs. per I. H. P. per hour. In this case the piston leaked to some extent.

I have tested quite a number of high speed engines besides the ones here referred to, and I do not remember a single instance where the feed water consumption was less than 30 lbs. per I. H. P. per hour. In cases of slow speed engines having four valves, the rate of economy has been less than 30 lbs., especially where the engine was of considerable size. These remarks all apply to non-condensing engines. A 16 in. × 36 in. engine running 72 turns per minute, with 77 lbs. boiler pressure, 32.9 lbs. mean effective pressure, cutting off at 30% of the stroke, gave a consumption of 29 lbs. of feed water per I. H. P. per hour, 81% of which was accounted for by the indicator. A 12 in. × 36 in. engine making 73 turns per minute, with a boiler pressure of 90 lbs., a mean effective pressure of 42 lbs. cutting off at one-third, gave 29.3 lbs. of feed water per I. H. P. per hour, 71% of which was accounted for by the indicator. A 23 in. × 60 in. engine working at 75 rev. per min., with a boiler pressure of 72 lbs., a mean effective pressure of 33 lbs., cutting off at 37%, gave 27.8 lbs. of feed water per I. H. P. per hour, 84% of which was accounted for by the indicator. A 28 in. × 60 in. engine running 65 turns per minute under 100 lbs. boiler pressure, with a mean effective pressure of 41 lbs., cutting off at 31%, gave a consumption of 25.8 lbs. of feed water per I. H. P. per hour, 82% of which was accounted for by the indicator.

In all these cases, the engines were tried for leakage, and the valves and pistons found in a fairly tight condition.

Prof. De Volson Wood.—If it should be shown, in the discussion of this question that there was greater fuel economy in the low speed engine, then I would like to know whether the greater first cost of such an engine would, in the course of fifteen years make it more economical than the high speed engine.

No. 309–61.

"What are the data derived from experience for the design of the paper covered friction driving gear used in the Northwest?"

Mr. H. H. Suplee.—The only data that I have been able to get on this subject is, that in the case of some of the large band-saw mills running in the Northwest, and some I believe running in the city of Nashville, in which a paper friction wheel driven by a four-inch belt, the friction wheel also being four inches face, that the friction was decidedly more powerful than the belt, and that if the machine was held so that the power was forced, the belt would slip or be thrown off before the friction would slip.

No. 309–62.

"What is the best way to secure brass flanges to copper pipe of large diameter, where steam of at least 150 pounds pressure is to be carried?"

Mr. A. F. Nagle.—The question would imply that there is a difference between brass flanges and copper pipes and ordinary iron flanged pipes. If a literal answer to the question is wanted, I cannot answer it.

But if it refers to any form of flange fastening for large pipes, I can recommend a form of fastening which will hold well.

I have in mind a 24-inch wrought iron steam pipe, lap riveted, and well riveted into a cast iron flange.

The only special precaution observed was that the corner of the flange was well rounded so as to prevent shrinkage strains.

Unless the pipe is riveted into the flange it is liable to pull out, and not only liable to happen, but I have known it to do so with disastrous effects.

The President.—Do you mean a number of small rivets, or was the metal of the pipe turned over?

Mr. Nagle.—Regular rivets, the same as you would rivet any transverse seam of a boiler.

No. 309–63 and 64.

To what extent does electro-plating reduce the temper of highly hardened steel?

Why does repeated bending of a long, thin piece of steel tend to draw its temper, when it has been hardened as hard as fire and water can make it, and will scratch glass?

Mr. Jacob Reese.—I think this is a very important question. If there is any one present who has any data on the subject, I would

be very glad to hear them. It is an important thing and it might lead to some very interesting thought, if any one has the data. I have not myself.

Mr. H. B. Gale.—I would like to ask whether any one has the evidence that the temper is affected at all by electro-plating?

Mr. Reese.—When I saw the topic there, I thought that some person had the data. It is very likely that it would produce such a result, and if so, it is a very important thing. It would give rise to very important matter if any of the members had such data. I think it possible that such a result could take place, although I have no data whatever, and it is new to me entirely.

Mr. E. S. Cobb.—I would like to ask Mr. Reese why he thinks such a condition could take place. All I ever heard about reducing the temper of steel, required that it should be done by some form of heat; meaning by temper a condition of hardness, and not temper as meaning a percentage of carbon. Perhaps I may be mistaken in the question to that extent. I took this to mean a degree of hardness due to the sudden cooling. If that be the condition of temper considered here I do not see how electro-plating could possibly affect it, unless the electro-plating be done by some method of heating with which I am not acquainted. Electro-plating, I believe, is generally done in a bath. I do not see how that could affect the temper of steel. If it does affect it, it would be a piece of interesting information to know that.

Mr. Reese.—It is a new thing to me, and I thought at the earliest opportunity I would make the test myself; but coming here I thought that the gentleman presenting the question had the data. I agree with the last gentleman. What is heat? Temperature is the measure of molecular velocity as weight is the measure of matter, and the electrical action there would increase the molecular activity and cause a readjustment of the molecules that had been placed in an abnormal position at the extraordinary cooling. I know by the production of certain phenomena in testing metals that the molecule is of the same general shape as the earth, with a short axis at its electric pole and a long axis at its equator. Its equatorial line is longer than its electric line, and in the act of chilling or tempering the electric axis of the molecules is adjusted on the line of the cooling so that they all point upwards to the surface of the cooling, while their normal condition would be in the other direction. I made some experiments some years ago, which were very interesting in this line, and I hope to renew them

so that I could give them to the society. But unfortunately, after building a machine which was about 40,000 pounds in weight, 12 or 15 feet long, 4 feet wide, and 3 or 4 feet in height, as I now remember, at considerable cost, and producing certain phenomena which I will now give to you, the investigation had to be abandoned. I took a bar of round, Hussey, Howe & Co.'s steel, of 100,000 pounds tensile strength and of one inch diameter, as near as they could get it. It was rolled through the grooves back and forward in the way that steel is generally rolled, the groove not being generally round, but having a number of flat faces. The steel, instead of measuring an inch in diameter, measured with my delicate instrument $1\frac{12}{1000}$ inches. Now, I argued that if the molecules had a long axis they would be drawn out on a line with the length of the bar. Prof. Barker, of the University of Pennsylvania, said: "Why, my dear sir, do you know what you are talking about?" Said I, "Not altogether, but I am working in that line." Said he, "Do you know how many molecules there are in a cubic centimeter of any gas?" Said I, "I don't know, but I take it for granted what Sir William Thompson and Prof. Helmholtz have given us, that there are nineteen million million million molecules in a cubic centimeter of any gas in normal condition." But it doesn't matter how many there are. If they have a long axis and a shorter one, the length of the molecule will be pulled out by the operation of rolling in that direction. To determine whether that was so I proposed to jerk these molecules around the other way so quick that they could not get back to their normal condition before I got through with them, and I built this large machine, and put them under a pressure of 392,000 pounds. I ran the bar into the machine cold, at a temperature of 60°, and ran it around 800 revolutions a minute, and caused it to pass through at the rate of 12 feet in a minute. It raised a laugh wherever I have told the story, and I will excuse you for doing the same thing. I put the bar in with a scale on, $1\frac{12}{1000}$ inches in diameter, with the most delicate and most accurate instrument made in the country to determine it, and it came out as bright as nickel-plated ware. I put it in at 60° and it came out quite hot in less than half a minute—so hot that it would burn your hands. It came out $1\frac{16}{1000}$ inches in diameter and the brightest thing I ever saw—the most splendid thing that was ever seen by living man. Mr. Metcalf, who is an expert in steel, came to the office and looked at it. He said, "Why, you are nickel-plating here." He put his hand in his pocket and pulled out a

sewing machine plate, and he went up and says, "Reese, who does this work? It is more beautiful than mine." I said, "There is no nickel on that." He called Mr. Hemphill in. He said, "Reese says that is not nickel-plated." Mr. Hemphill said, "It is not; that is bar steel." Mr. Metcalf said, "I cannot believe it." I said, "Come into the shop and put it through yourself." We took him in the shop and showed him the machine in operation. I want to duplicate the test a little farther and measure the work with a better instrument so that I may be sure that I could tell you that I had determined the shape of the molecule while Sir William Thompson and Prof. Helmholtz had determined the number of the molecules, but unfortunately I have not been able to do so yet.

By that means I have determined for my own satisfaction that there is a shorter and longer axis to the molecule the shorter one is the electric axis, and when you put in the electric bath a piece of steel, highly tempered, you heat it—just the same thing; because heat is a phenomenal measure of molecular velocity as weight is the measure of matter. You put the molecules to a high tension and motion, so that they adjust themselves and get into their normal state.

Mr. Wm. Kent.—I do not think it right that Mr. Reese's statement that the bar increased in size should go with the statement that it was due to the change of the molecules. Mr. Mannesmann does the same thing in Germany, with steel increasing the diameter. He puts a hole 4 inches in diameter through the bar.

Mr. Reese.—We have got those bars in every shape and manner perfectly solid. I have those bars in my office, and I ask the gentlemen to come there and test them, and I will pay the expense if they can find any porosity in the center. There is no hole in this thing. It is a solid fact I am telling you.

Mr. J. T. Hawkins.—As it does not seem to be a settled fact in the first place as to whether the electro-plating does disturb the temper of the steel, I do not know that we can get very much more out of the question. But it seems to me that it is not at all unlikely that something of the same kind may occur with thin steel strips; whether it would or not in larger bodies I do not know. But I remember some things that occurred in the treatment of small steel strips which would indicate that something of the kind might occur in the electro-plating bath. I had to treat some strips of tempered hoop-skirt wire, which, when soldered, were to go in a bunch around a centrally bound wire. The usual method of

preparing such things for soldering is to dip them in a saturated solution of zinc chloride, and that method was adopted. I found in putting such strips in this zinc chloride, that if they were allowed to stay in there over, say, one minute, or at least if they stood as long as five minutes, they would become very rotten. I had to be very careful how long I left them in the zinc chloride. The solution was simply the saturation of zinc in dilute hydrochloric acid. It is a certain fact that tempered steel when allowed to remain in such a solution over five minutes or anywhere nearly approaching that time, becomes as brittle as glass. One could take them in their fingers and nip them off with the finger nails after that time. It occurred to me that possibly some of the electroplating solutions may be chlorides and contain hydrochloric acid, and that some such action as that might take place.

Mr. Reese.—I think that arose from another cause. The electric action there sets hydrogen free and it is occluded in the metal. I have produced metal that has had a large occlusion of hydrogen in, and have taken an eighth of an inch hoop which you could break with your hand. The hydrogen occluded forces outward, so that the iron becomes very brittle. I think that is the cause of the effect my friend speaks of.

Mr. Geo. R. Stetson.—I am prepared to believe almost anything that any member might state as to what takes place in steel. I should expect the author of that question would have had some experiments. I have an idea, however, that electrical action on a piece of steel might facilitate or cause a rearrangement of the molecules in the steel. I am a thorough believer in the fact—at least I believe it is the fact—that steel is not a metal which lies still a great while within itself. We have too much experience in the fact that our gauges require readjustment. A week ago to-day I had one of the most remarkable facts presented in that way. I was anxious to come to this meeting and was hurrying up some experiments in drill making. At about 3 o'clock I gave orders for the reduction of a piece of forged steel, 1⅞ inches to the proper diameter for the shank of a large drill 1¼ inches. The blacksmith reduced it at a heat that was proper as indicated by the fracture, but in our anxiety to work with the steel, he dipped it so that he could handle it. The workmen worked on that piece of steel until night, having it in their hands several times. At night they put the steel on the machine, but it had not been tempered; it was just as it left the hands of the blacksmith. In the morning that

steel was about 5 inches long and about $1\frac{1}{4}$ inches of the stub end connected with the $1\frac{7}{8}$ inch end was left, but the other part of the steel was lying on the floor and was as bright and clean and perfectly fractured as ever was seen and as regular as could be; that is, the circle was as nice as if that piece had been centered and cut off in a lathe. If there had been any appearance of a blow-hole or seam, or anything of that kind in the steel, I should have felt that we had a partial solution of that phenomenon, but it was a perfectly clean and brilliant fracture. I took the piece of steel to see if it had been cooled so as to produce a temper, and it had not. I meant to have brought that piece of steel here, but it is a good way to bring two or three pounds of steel, and you have to take it on my statement. It is a fact without any explanation whatever. This strain was going on for hours and during some time in the night that steel exploded and it was in those two parts. In our manufacturing of tools it is not uncommon for us to have tools to break in four or five hours, or a day or two from the time that they are tempered, and we have gone so far as to send out what we supposed to be a perfect shell reamer of five inches in diameter or thereabouts and have it returned by the indignant receiver with a request to be informed why we sent such imperfect tools, and he had good reason, as the reamer was broken square in the center. So I believe that the molecules of steel are adjusting themselves constantly, and under certain strains it goes on for years. By keeping an accurate record of your standard gauges you find it is sometimes necessary to readjust a gauge, that they will not work at the end of a year or six months as they did before. This phenomenal pulling of the steel showed me an action that was beyond anything that we have the means of determining. If a person could say that the temper was reduced by the action of nickleplating, I should explain that as being due to the conditions that allowed the steel to fall into certain lines and change the relation of one molecule with another, and I think that perhaps it would be well to follow out and find out whether that is a fact or not. I should on general principles, say it was not a fact, but if a man said it was, I am prepared to believe it.

Prof. Hutton.—I might say that this is a fact. These two questions came to me from one of the New York members, and he simply states his experience, that the electro-plating of a piece of steel did produce this effect. It softened the steel. Now he wants to know why. Those two questions 63 and 64, both on the

effect of the hardening of steel, came from him, and he asks why. They are both of them facts, and it remains for a solution to be found.

I might supplement what Mr. Hawkins has said in reference to the effect of a zinc solution by stating that some recent experiments which I had the occasion to make, showed that the process of galvanizing either iron wire or iron pipe made the galvanized wire or pipe, when it was properly done, weaker than the plain wire or pipe, although it had apparently a greater cross section. It was the same in both iron wire and iron pipe. The solution which I offered was either that there was excessive action of the pickling acid or that the acid had not been thoroughly removed in the cleansing of the scale before the zinc coating was applied.

Mr. Stetson.—In regard to the tools that burst, it is generally the case that we can find the appearance of an air bubble that has been elongated and rolled out, but why there should be power enough in a little spot of that kind to rend asunder a solid tap of three or four inches, which is very frequently done, and which would represent thousands of pounds of strain, is what I do not understand. But there is some power locked up inside there in those imperfections that will rend the tools very frequently, and in a very great number of the losses of the year, we see this defect in the steel, but we are at a loss to account for the power that any such defect should have. But in such cases as that I cited of soft steel, I do not know that I have seen many of those phenomena, and there was no imperfection in the steel, there was no air bubble or anything of that kind. It was a perfectly clean fracture. It was a soft piece of steel and good steel too.

Mr. A. F. Nagle.—That peculiar action is by no means confined to steel. I have known a square glass inkstand to break in a clean sharp line diagonally across.

Mr. Reese.—I presume that we all admit and understand something of the science of molecular physics. Now, one of the members says he cannot see how a power could get in there to break that steel asunder. Now, that bar of steel is not a continuous body; it may be physically divided. The end of the physical division is the molecule. The molecule may be divided by chemical action; the end of the chemical action is the atom. Atoms *per se* are inert. Their power and energy are derived from the physical forces they are endowed with. Consequently when you break a piece of iron by tensile strain you do not rupture the iron

per se; you merely pull the molecules apart. When the tensile strength of steel is 100,000 pounds to the square inch, the 100,000 pounds is the strength which is necessary to rupture the physical force that holds the molecules of the bar together, the same as you put a bar of iron against a magnet and you have to pull it to get it away; it is the same thing. These are little magnets—millions and millions of them, and it is those that you pull apart. It is easy to be seen if that is the case why you are interfering with those physical forces there. When you produce what we call a strain, it is a disarrangement of the molecules, so that they lose their power on each other. For instance, if their power is in any given line, and you throw them across the line, why, you disarrange them, and you have that phenomenon in a sheet of iron which, while it may have 60,000 pounds strength lengthwise, it may not have more than 42,000 pounds when you take it across the grain as we call it.

Mr. H. P. Minot.—I would like to ask Mr. Reese if this bar that enlarged grew any shorter during that operation.

Mr. Reese.—Not a bit shorter; but I will tell you what it did do. If you put a lot of eggs lengthwise one after the other and you turn them around, you increase the distance between the eggs, don't you? The bars became weaker because the pores between the molecules were enlarged. The strength was reduced 2,750 pounds to the square inch. The ductility was increased.

Mr. Minot.—I have found that by heating cast iron pieces, or something of that kind, that they are about as apt to shorten as they are to lengthen. I did not know but that in this case he might have shortened it up and have the same amount of metal there but in a different shape.

Mr. Reese.—I know of no case where, if you heat a bar and expand it, after it cools it will be shorter than before.

Mr. O. A. Lanphear.—Some of the experiences related here in regard to steel put me in mind of the deep waters that I have been going through recently on the subject in the hardening of large pieces of steel—pieces which would weigh from 50 to 150 pounds, and in some cases 200 pounds. We have had them break under conditions where it was least expected. I remember one case just now of a die that weighed perhaps 75 pounds breaking two weeks after hardening. A pair of dies had been hardened, temper being very slightly drawn. The dies were laid in the shop on a bench, a ray of sunlight falling on one of those dies for a few minutes, it exploded like a pistol, one-half of the die flying

up against the ceiling and falling back on the bench. That die was perfectly sound to all appearances. There was no place in the center of the die or anywhere that looked as though the steel had been improperly melted or that there might have been piping. We have had them break at times, all the way from a few minutes —broken in some cases before they would come out of the water— up to two or three weeks after hardening. But we think we have got a clue to a good portion of the breaking. It is in the improper annealing. We have found the breakages confined to a number of bars of steel which we have used that have been annealed by the makers, and I feel very sure in saying that some of them, at least, were improperly annealed. We are carrying out a system of records at the present time to demonstrate whether this is the fact. We found also a large number of pieces of steel on which the steel-makers were unable, after the dies had been tempered and broken, to tell whether the defect that showed in the die was on account of piping, improper melting, or something else that they knew nothing at all about. They freely confessed that they did not know what the trouble was.

Mr. J. T. Hawkins.—I think it is not necessary for us to go into the molecular constitution of steel to explain the most of the phenomena which have been mentioned here to-night, in connection with the fracturing of large pieces of steel that have been tempered or hardened. So long as it remains a question whether there are any molecules or atoms in steel or in any other substance, or what the ultimate constitution of matter is; so long as it is a disputed question whether or not there is really any such thing as matter, it would seem to be quite unprofitable to attempt to explain certain peculiarities of steel in any way involving its molecular or ultimate constitution. I think we are rather more likely to lose ground by attempting to explain the phenomena in any such way than we would be by observing the more familiar phenomena themselves which we know appear in connection with it. In the first place, I think that you will all admit that these fractures of tempered steel occur only in large pieces. I think none of us have any experience of small taps or small drills or any small pieces hardened and afterwards drawn and tempered, fracturing, after they are laid by or during the process. It almost invariably occurs in large pieces. Perhaps the most familiar instance is in the making of silversmiths' rolls. They give out probably oftener than any other piece of mechanism that is constructed of that

material. Now, it seems to me that it is pretty easily explainable upon ordinary methods of reasoning; that is, for the same reason that we have internal strains in an iron casting. In hardening a large piece of steel it must be admitted that it is impossible to cool it throughout its whole substance at the same time; that the exterior becomes cool first. We must also admit that small pieces of steel which can be hardened throughout almost instantaneously, increase in volume when hardening, in ninety-nine cases out of one hundred if not invariably. A small piece of tool steel, merely hardened, will invariably increase in volume. If that is the effect of hardening upon steel in small pieces, the effect in a large piece of steel is that it produces that swelling or increase of volume on the exterior and that the increase takes place before the interior can be affected, and produces a state of things in such pieces of steel similar to what we have in iron castings where they have members of unequal volume and which may cool at different rates. I think that is the true solution of all breakages in large hardened pieces of steel. Because such a piece has had the opportunity in hardening to increase its volume in the exterior portion before the interior portion can have produced upon it the same effect, it is left under conditions of internal strain that finally rupture it.

Mr. W. S. Rogers.—I have been feeling lonesome on that subject for the last four years until to-night. About four years ago the firm with which I was connected had occasion to use several thousand small steel plates. They averaged about $\frac{1}{16}$ of an inch thick, and 12 to 15 inches long, and about 2^{5} inches wide at each end, and an inch wide in the center. They were supposed to be spring steel. They wanted to use spring steel. They were to be nickel plated. The firm got the steel from, I think, the house in Pittsburgh from which Mr. Reese got his bar steel, and they came to us polished bright and they were very stiff and springy. After they came back from the nickel plater's they could be bent double and bent in different shapes. The firm abandoned the use of spring steel and commenced using soft steel. I always had a sneaking sort of suspicion that nickel plating did that business. I went down to the nickel plater and he told me that I was a fool. I did not know but what he was right, and I have not said a word about it since.

Mr. J. W. Cole.—Some ten years ago Mr. George Banister, then foreman with Brown, Hinman & Co., Columbus, O., obtained a patent for a special method of heating and hardening steel by means

of metal baths. His bath for treating forks, for instance, was composed of two parts tin to one of lead.

The claim in his patent * is "the process of hardening and tempering steel at one operation, which consists in heating it to the degree required for hardening by ordinary methods, and immersing it while so heated in a molten metal bath, and retaining it therein substantially as set forth," *i. e.*, until the steel to be tempered is reduced in temperature to nearly that of the bath. Banister varied his baths, using one of three parts tin and two of lead; also a special chilling bath for giving a very hard temper of eight tin, four lead and one and a half mercury, though the last one vaporized too readily for general use, but fine steel hardened in it could scratch glass.

Upon his ordinary baths he used fine bituminous coal to prevent oxidation. Mr. Banister, owing to some infraction of prison rules, lost his position, went to New England, and I had no longer opportunity to note his practice. My impression is, however, that his process left the steel in less strain from shrinkage in cooling than the ordinary methods of tempering.

I find I have no memoranda of any trial of heating in a lead bath, and hardening in one of lead and tin, so cannot state positively whether tests of that plan were made, though they were in contemplation. I find that Banister's work was mostly done as his patent claims, "by heating the steel by the ordinary methods and then hardening in his molten metal bath" (two parts tin and one part lead). For the sake of the description, I quote from his patent papers as follows:

"The process consists, generally stated, in hardening and tempering steel at one operation by means of a metal bath, as hereinafter described.

"The process is applied to steel forgings, such as springs and cutting and other tools, but is particularly suitable and efficacious in the case of the former—to wit, springs.

"In practicing my process, I first heat the springs or tools to a cherry red, and then immerse them in a metal bath—namely, a bath of melted lead and tin—and allow them to remain therein for a short time. It matters not if they remain a long time—say several hours—since the effect is the same as it would be if they remain a short time—say a few seconds. It is only necessary that

* U. S. Patent 204,788—June 11th, 1878.

they shall not, in most cases at least, be instantly withdrawn from the bath.

"Springs for various uses require a lower temper than most, if not all, other articles, and edge-tools require a comparatively high one. To harden and temper springs, the bath of melted tin and lead must have a high degree of heat; but for edge-tools the bath requires to be heated to a less degree, the principle being that the lower the temper the higher the heat required, and, *vice versa*, the higher the temperature the lower must be the temper.

"The temperature of the metal bath may be determined and regulated by means of any suitable instrument; but I find that in practice none is requisite, since the judgment of the workman enables him to readily ascertain the temperature. The temperature of the bath, in fact, requires to be the same as in the usual process of hardening metals preparatory to drawing the temper, or softening them by subsequent application of heat.

"I am aware steel has been hardened and tempered at once by heating it and plunging it into a bath of water or oil; and, furthermore, that metal baths, both lead and tin, and their alloys, have been used as hardening-baths previously to tempering and as tempering-baths subsequent to hardening.

"The advantages I claim for my invention are: First, it will impart to steel implements a more uniform and better temper; second, it saves the loss now occasioned by steel implements warping and cracking in the process of hardening; third, steel tempered by this process, after it leaves the metal bath, is more pliable, and any defects in shape can be more easily corrected, than in steel tempered by the old process; fourth, this process saves time and the expense of keeping up a bath for the purpose of hardening the steel before it is tempered."

Mr. Lanphear.—I have no desire to prolong this discussion. I would just like to add a word in explanation of what Mr. Hawkins has said in regard to the expansion of steel in hardening. I took two pieces of steel, approximately six-inch cubes, having cut them from a bar which had not been annealed. Having squared them up—they were soft enough to square up—annealed and tempered them and annealed them again and tempered them again, at each time squaring them up so as to find how much the bulge was on each of the sides of the cubes. I found that when the pieces of steel had been thoroughly annealed that you could harden them and the faces of the cubes would not be-

come convex at all. Therefore I concluded from that and from my other experiments, that after steel is thoroughly annealed; if the molecules of the steel are allowed to come to a free and natural position before the tempering takes place, that there will not be so much danger of the steel breaking, neither will there be so much springing of the steel after it is tempered, and also there will not be so much expansion of the metal. I have made some experiments that sustain this belief, but I have not the data at hand.

Mr. W. W. Dingee.—Possibly the molecular motion of the thin blade of hardened steel caused by repeated bending may correspond to the same movement caused by heat and if so, be competent to reduce the temper of the steel and render it soft.

Mr. Wm. Kent.—I think this discussion of the peculiar phenomena exhibited in steel, is so very interesting, that we ought to have some day a sort of symposium presented by the members of this Society on steel phenomena. Each member can contribute, say, half a page, describing some peculiar phenomena he has witnessed, bringing facts, not theories. That will add to the amount of our knowledge on steel, and lead to some true or some better theory of these peculiar phenomena. I make the suggestion for the next topical discussions for the next meeting.

No. 309–65.

"How much should be removed from the edges of punched or sheared steel plate to cut away the injured metal?"

Mr. J. T. Hawkins.—I think I may be able to throw a little light on that question by describing some experiments made by myself. Although it would not appear at first sight to be very germane to the question, I think upon a little further examination it may appear to have a very important bearing upon it, and it may result in suggesting a possible means of arriving at a correct answer to the question, as regards boiler plates. The experiments that I am going to describe were not made upon boiler plates, but upon very much thinner material. The sketches here shown illustrate the construction of a steel belt with which the experiments in question were made. This belt was constructed of short strips of sheet-steel connected as shown in Figs. 355 and 356 in plan and elevation; the cross strips connecting the longitudinal ones together as shown. At each junction is a rivet *f*, extending through a

wooden cone *g*. These cones run in corresponding conical depressions in the pulleys properly spaced. The longitudinal strips, before they were made up into the belt, were bent to a curve corresponding to twice the diameter of the smaller pulley, as in Fig.

FIG. 355.

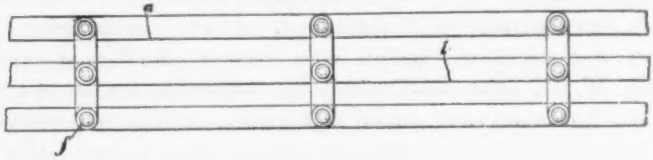


FIG. 356.

FIG. 359. FIG. 358.

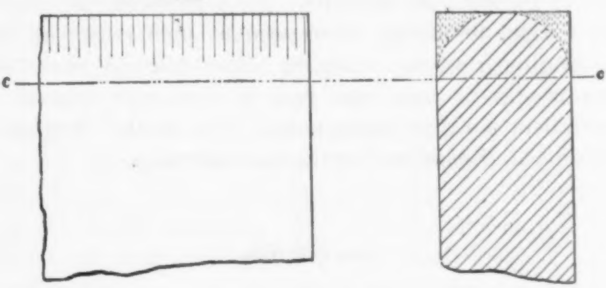


FIG. 357.

357, in which the smaller circle represents the smaller pulley, and the larger circle the belt, so that its permanent set would leave it in such a condition that it would be flexed in conforming to the pulley, equal to the amount of flexure it would receive in straightening while passing from one to the other pulley. The strips, in the first experiment, were cut from sheet-steel No. 20, Stubs' gauge, one inch wide, with shears and without any other preparation than drilling the holes and rounding off the ends of both the cross and longitudinal pieces, they were riveted together as shown.

No disturbance was made of the original edges as left by the shears. The steel was cut just as it came from the rolls, having a considerable temper in it, due to its rolling, but not tempered by any other means. In the first experiment, it was found that, in running a belt over a 24-inch pulley at about 900 feet per minute, inside of an hour it began to give out by cracks appearing at the edges of the longitudinal strips. In the experiments I set out to make, this was a great disappointment. I let it run, however, until a number of the strips broke through, the cracks just described traveling, in time, entirely across the strip. For instance, one strip would break at a, another at b, Fig. 356, and until all three strips broke at some place between two adjoining cross strips, the belt continued to hang together. An examination of the strips under a magnifying glass, after having run as above, showed that the action of the shears on them was to initiate little cracks along the edges, something as shown in dotted lines, Fig. 358, and the full lines, Fig. 359, and the continued flexure of the strips made those cracks gradually creep across them. I concluded that the best thing to do would be to remove from the edges of the strips a certain amount of the metal shown as disintegrated by the shears. I tried several experiments in removing varying quantities from the edges, by grinding, and found that, while the removal of any amount, however small, increased their life, I could not escape the cracking entirely until I removed an amount equal to half the thickness of the material. Referring to Figs. 358 and 359 (Fig. 358 being a cross section, and 359 a plan, of one edge of such a strip, greatly enlarged), I found that, in order to run it for any considerable time without any of those cracks extending, I had to remove an amount equal to half the thickness, as indicated by the dotted line cc, Figs. 358 and 359, and then it would run for several weeks. But even under that condition I would find, at the end of several weeks, a strip here and there with a crack appearing in one of its edges.

My next experiment was to round off the edges, and make the rounding-off by grinding longitudinally of the strips; and in that way I found that I need remove no more from the central portion at d (Fig. 358) than just enough to mark it by the emery-wheel or grindstone; and, so long as the marks were longitudinal and the strips rounded on the edge so as to correspond in form to one-half of a wire, in the form shown in section, Fig. 358, it would run for an indefinite period. I finally made a belt of five strands, of an inch and a quarter wide, No. 20 steel, running over a 36-inch

pulley for the smaller one, and put it at work in the American Institute Fair of 1877 as a main driver; and it drove about one-half of the machinery over three months, running at about 900 feet per minute, without sign of fracture or crack.

Now, it occurred to me that, in view of this experience, it would be a good idea to test boiler-plates in some such way. If the action of shears on thick plates, such as boiler iron or steel, conforms to the conditions shown for the thin steel strip, it can be determined by subjecting strips of boiler-plate to repeated flexure within its elastic limit. I would suggest that some apparatus might be constructed by which we could take the boiler-plate and flex, or vibrate, it within its elastic limit, and that such action should show cracks appearing in it similar to those found in the steel strips, if we had not removed sufficient metal to take away all that which was disturbed by the shears. This plan could hardly apply, however, to a determination of the effect produced upon, or injury done to, a plate by punching. As punching is, however, not done nowadays on thick plates, this is of less consequence.

Mr. J. H. Cooper.—It is stated in Shock's "Steam Boilers," "The zone of injured metal does not extend further than $\frac{1}{8}$ inch from the hole, which may be entirely removed by drilling or annealing."

"A. C. Kirk states that when the diameter of the holes is three times the thickness of the plates or greater, the injurious effect of punching is inappreciable."

"By making the die-hole larger than the punch a taper hole is produced in the plate, and the punching can be done with less expenditure of power and with less strain on the plate."

"Sellers determine the diameter of the die-hole by adding two tenths the thickness of the plate to the diameter of the punch."

"Plates should be punched from the contact sides of the sheets at the joints, and then if seaming be necessary, less metal must be removed to make the holes even, this method does most good work in the making of a plate joint at least expense."

"The action of the punch is to strain, disturb and compress the metal surrounding it. This induces initial stresses in an annulus of metal around the hole, and very probably also, as M. Barba thinks, alters its power of elongation, reaching in some cases 20 to 30 per cent."

Mr. H. H. Supplee.—In support of Mr. Hawkins's statements about steel strips I would say that in the manufacture of band-saw blades it is the practice, and it has been found very necessary, to

examine each blade thoroughly with a glass along the edges, after it is sheared from the strip. Defects are almost invariably revealed by this examination, and the back of the plate must be ground away until the deepest cracks have been ground out before the blade is considered fit to be sent out and used; in repairing saws, also, it is always customary to examine them at every point on their edge with a glass to discover just such cracks.

Mr. Wm. Kent.—I believe that the fear of the damage done to steel plates by shearing the edges and by punching holes is much less than it used to be some ten or fifteen years ago. Then we heard of the great danger of a plate going into a boiler which had the sheared edges not trimmed off, and the holes punched and not drilled. Now, we hear less of it, and the reason no doubt is that now we are using very much softer steel, and the softer the steel is the less damage the shearing and the punching appear to cause. In very soft fire-box steel we can take an ordinary sheared strip two inches wide and bend it double, flat, either hot or cold or after tempering, and it will not crack across the hammered-down edge. But if we take a little higher carbon, say .20 or .22 carbon steel, it will almost invariably crack clear across; while with some steel, if the sheared edge was shaved off it would bend double cold. That is also confirmatory of what has been said about saw plates—that the danger is very great with high carbon steel. The danger is scarcely anything in the softest steels.

Mr. Hawkins.—I would like to ask Mr. Suplee if they make any provision in band-saws, in the bottoms of the teeth, so that that side of the plates does not partake of the conditions I described in the case of a belt. It would seem to me that the teeth on one edge of the band-saw ought to present precisely the objectionable condition I found from the shearing, if no provision is made for removing a portion of the metal disintegrated by the action of the punch.

Mr. Suplee.—The teeth are first punched out and then ground out over their entire outline, with an emery wheel which has rounded edges, operated by special machinery and when band-saws do break they nearly always break at the root of the tooth where the crack has not been ground out.

Mr. C. L. Huston.—There is one question I would like to ask, if any one has any way of explaining it. It is often noticed in shearing *soft* boiler plate steel more particularly, that peculiar curved lines will appear on the surface of the plate, Fig. 360,

starting nearly parallel with the line of shearing and curving off, running sometimes as far as three or four inches—even more than that, depending on the thickness of the plate—running as far, sometimes, as six inches away from the edge of the plate. What does that indicate? Is it an injury to the metal?

I think it has been noticed that the softer the boiler plate steel

FIG. 360.

the farther those lines will run in—just showing the breaking of the scale—of the finishing scale on the plate.

Mr. Jacob R. Reese.—Have you ever ground a plate off and polished it and discovered those lines, or are they due to the loosening up of the scale?

Mr. Huston.—I have never tried anything of the kind. But it does not show in iron. In iron plates, finished in the same manner the same thickness, those lines do not appear.

Mr. Reese.—No; the oxide is of a different nature. There is a little difference in the oxides of the iron and the steel.

Mr. E. S. Cobb.—I would like to ask what direction that plate was sheared from when the lines were indicated as upon the board?

Mr. Huston.—I cannot say positively, but my recollection is that it ran from the left end to the right.

The President.—It is common on all steel plates.

Mr. Huston.—I think so.

Mr. Nagle.—How far did those lines extend?

Mr. Huston. They vary with the thickness of the plate, and the

FIG. 361.

character of the shear blades, too, has some effect on it. I think sometimes I have seen them in very heavy plates—plates an inch or more in thickness, where they ran in, six or eight inches. Another thing I thought of in connection with the injury to the plate, in shearing off the plate, to make an enlarged section of it,

Fig. 361. It leaves the plate somewhat in that shape, and in taking those strips sheared from the edge of the plate for bending tests, I found that it was always best to bend them with the side having the rounding corner out. Bending this other side out, even if the sharp corner is ground off to the same curve, will not be as satisfactory. The fibre seems to be pointed out in that direction. So that in this matter of rounding off the edges it will make a difference whether it was the edge that had been originally rounded by the shears or the sharp edge. There should be more taken off the sharp corner, to remove injured metal, particularly, in a thick piece.

By the aid of a micrometer caliper it may often be observed that there is a diminution of thickness to a greater or a less distance back from the sheared edge, extending beyond where the plate bears on the shear blade, due to the pinching effect of the shears.

In our preparation of test pieces for tensile strength and elongation, we do not feel safe without removing all of this compressed metal: it would seem reasonable to assume that removing this much would be all that is needed for any practical purpose, while for most purposes much less would do.

Mr. Kent.—I think these lines are caused by a slight corrugation of the plate due to the pressure of the shear. If you attempt to shear a narrow strip off the plate, the strip itself curls up into a ring. The plate being sheared is laid pretty nearly flat, but there is a slight wave of distortion as the shear goes along, and that wavy motion would naturally crack the scale. I think those lines are due to that slight wavy motion cracking the scale and do not injure the plate materially, for I have seen test pieces in which the whole plate was covered with them; yet there was no apparent damage in any test the plate could be subjected to.

Mr. Nagle.—I would like to have the speaker say whether he could trace any relation from the pitch of those lines to the teeth in the gears.

Mr. Huston.—No, I think not; because one pair of shears has no teeth in the gearing at all, it is a shear of the lever pattern operated directly by a crank shaft, driven by a large belt without any intermediate gearing.

The President.—I think the same disturbance of scale appears in a punched hole.

Mr. J. S. Coon.—Mr. Kent touched on the case exactly when he spoke of boiler plates. You take a piece ten inches wide. You

break that piece in a testing machine and at first some lines will appear commencing at the edge and running towards the center, showing that it is merely a cracking of the scale.

The President.—Mr. Barba made some experiments with steel plates up to $\frac{1}{2}$ of an inch in thickness and found they were not injured by shearing to a greater extent than about $\frac{1}{32}$ of an inch. I conducted some experiments a short time ago, with $\frac{3}{4}$ plates and found that rule to hold good for them also. By removing about $_1$ of an inch by planing from the edge of the plate the remaining metal was found to have the same strength as that from a piece cut by planing from the interior of the plate. The same rule held good for punching. By removing a cylinder $\frac{1}{32}$ of an inch thick from the hole by drilling, the remaining metal was found to have the same strength as it would have had, had there been no punching.

Mr. Reese.—In iron, I presume, the same thing occurs. We were making armor plates for which we had a contract with the government, and the engineer present wanted us to shear the plates wider, and plane more off. In one of the planers we had two stationary knives. We drew the plate right through it. We took off an eighth on each side. He wanted us to shear them wider and plane more off on account of the ragged appearance of the plate when sheared. He said he thought the cracks would run in half an inch. We tested the matter thoroughly and satisfied him that the cracks did not exceed an eighth of an inch in the shearing. However, we always sheared the plates before they got entirely cold. We made 10,000 tons of them. They tested them several times and we never had any complaint after the first.

Mr. Hawkins.—I submit that it is a pretty difficult matter to test a plate of that kind and determine whether a injury made by a shear is eliminated or not; and I would like to know how the test was made to determine that fact. That is what we wanted to get at—to find out whether the injury produced by the shears was actually overcome by the removal of an eighth of an inch of the metal.

Mr. Reese.—I think they determined that with acids; I think that was their method. It was perfectly satisfactory to us. They allowed us to go on and paid us our money without growling.

Mr. Huston.—I would also say that in testing a sample of steel ship plate where probably one-eighth of an inch had been removed from the sheared edge (the steel was a little too hard for the pur-

pose probably). It showed crystallization for fully half an inch, so that that edge of the strip when tested in the machine did not elongate at all scarcely and showed a bright crystalline fracture, while the other edge of the strip showed considerable elongation. The crystalline edge giving way first, the remainder tore off.

Mr. Hawkins.—I would suggest as a method of determining that matter, aside from the method proposed I would take a boiler plate that is sheared and cut a strip off the edge by means of the planer of a width such as might be supposed to contain a considerable portion of the uninjured metal besides that injured by the shears, and then cut an equal strip adjoining it entirely uninjured in the same manner. In that way, by trimming off the injured strip at the sheared edge and the uninjured to the same width, we could certainly arrive at pretty good results in the testing machine by comparing the uninjured strip with the sheared strip.

Mr. Reese.—Of course we understand that this is all of the same character of metal; that is, it is open hearth or crucible steel not over .15 carbon; because if you take Bessemer steel that contains .15 carbon and .025 or .070 silicon, the result will be entirely different. Then it will crack farther in. The more silicon there is, the farther the cracks will run. It is presumed that it is a uniform quality of steel—the open hearth steel not to exceed .03 of silicon and not over .15 of carbon.

Mr. Huston.—This piece that I spoke of as showing the crystalline fracture was Bessemer steel.

Mr. Reese.—In Dr. Dudley's statement of the test of rails for the Pennsylvania Railroad I find that the silicon in steel runs very high—so high that I thought it was open hearth steel that the gentleman had reference to. Of course the cracks would reach three times as far as they would in open hearth steel or soft crucible steel, low in carbon and low in silicon. I have some data, not with me now, but I can give it at some other time, in regard to the effect of silicon upon steel.

The President.—The steel I tested was open hearth.

Mr. Hawkins.—I think the question of whether it was open hearth steel or what the percentage of carbon was in it, is rather wide of the subject. It seems to me that the question is what amount of metal is it necessary to remove to eliminate the injury produced by shearing and punching. Of course we understand that would be different in different kinds of material. That is the question as I understand it.

Mr. Huston.—The shear blades would have a larger effect on it than anything else. If the shear blades were sharp-angled so as not to bend down the edge of the plate much, it would not injure the plate much. But if the angle was nearly a right-angle so as to bend down the edges, it would injure it for a considerably greater distance.

CCCX.

MEMORIAL NOTICES OF MEMBERS DECEASED DURING THE YEAR.

WILLIAM WALLACE HANSCOM

was born in 1839 in Eliot, Me. In 1854 he began his apprenticeship in California, to which his family had moved. In 1862 he was draftsman for the Novelty Iron Works in New York, but returned to California in the following year to become draftsman with the Golden State Iron Works. In 1866, with others, he started the Ætna Iron Works, and in 1875 he sold out from this firm to establish the Hope Iron Works on his own account. In 1879 he went into consulting and expert practice, and was for three years consulting engineer of the Presidio and Ferries Cable Ry. of San Francisco. In 1882 he went to London and designed and made the plans for the Highgate Hill Cable Tramway, the first of its kind in Great Britain. In 1883 he returned to San Francisco and resumed his consulting practice in cable railway, electric lighting and water works construction and river and harbor dredging, etc. His professional life extended over 32 years of varied experience.

He joined this Society at the Pittsburgh meeting in June, 1884. He died Jan. 19th, 1888.

BARNABAS H. BARTOL

was born at Freeport, Me., Oct. 31, 1816. In 1833 he entered the branch of the West Point Foundry, under Messrs. Kemble in New York, as apprentice, and remained with them until September, 1847. In 1835 he was sent near Richmond, Va., to erect hoisting engines, and in 1837 he was sent in charge of the erection of the beam engine for the *Richard Stevens*, the first of its type on Seneca Lake, N. Y. In 1838 he went to Cuba to erect sugar machinery. In 1839, at the consolidation of the Cold Spring and New York establishments of the West Point Foundry, Mr. Bartol

was asked to become its superintendent, on the retirement of Mr. Chas. W. Copeland. The latter had been secured by the Navy Department to design the machinery for the S.S. *Mississippi* and *Missouri*. From 1847 to 1867 Mr. Bartol was engineer and superintendent of the Southwark Foundry of Philadelphia, under Messrs. Merrick and Towne, and Merrick & Son. During this time much important work passed through his hands. The machinery of the U. S. S. *Wabash* and the design and construction of the machinery and plating of the *New Ironsides* and other steamers, were so noteworthy in the marine field, that the position of Engineer-in-Chief, U. S. N. was tendered to Mr. Bartol during the war, but it was decided that he could be of better service to the country's cause by remaining to supervise the work then in the shops. His plans for supplying the locks on the Chesapeake and Delaware Canal received the offered premium; much gas-works construction came into the foundry through Mr. S. V. Merrick, and the steam-hammer contracts with Mr. James Nasmyth and those with Mr. Norbert Rillieux, inventor of the "triple effect" system of sugar boiling gave a very wide experience.

In 1867 Mr. Bartol left the foundry and devoted himself to other business. He managed the Grocers' Sugar House in Philadelphia, which he had built in 1859, and which was the first there to use centrifugal dryers. He was President of the Washington Gas Light Co. (D. C.), from 1864 to 1883. From 1871 to 1880 he was Director of the American Steam Ship Co., and Chairman of the Committee on Ships. In 1851 he published a "Treatise on the Marine Boilers of the United States."

He joined this Society in 1882, at the annual meeting in New York and was also an interested member and contributor to the Franklin Institute of Philadelphia. He died February 10th, 1888, in that city.

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